Jagdish Mehra
Helmut Rechenberg

THE HISTORICAL DEVELOPMENT OF QUANTUM THEORY

VOLUME 6

The Completion of Quantum Mechanics
1926–1941

Part 2

The Conceptual Completion and
the Extensions of Quantum Mechanics
1932–1941

Epilogue: Aspects of the Further
Development of Quantum Theory
1942–1999

Subject Index
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The Conceptual Completion and the Extensions of Quantum Mechanics (1932–1941)

Introduction

The invention of quantum and wave mechanics and the great, if not complete, progress achieved by these theories in describing atomic, molecular, solid-state and—to some extent—nuclear phenomena, established a domain of microphysics in addition to the previously existing macrophysics. To the latter domain of classical theories created since the 17th century applied—principally, the mechanics of Newton and his successors, and the electrodynamics of Maxwell, Hertz, Lorentz, and Einstein. The statistical mechanics of Maxwell, Boltzmann, Gibbs, Einstein, and others indicated a transition to microphysics; when applied to explain the behaviour of atomic and molecular ensembles, it exhibited serious limitations of the classical approach. Classical theories were closely connected with a continuous description of matter and the local causality of physical processes. The microscopic phenomena exhibited discontinuities, 'quantum' features, which demanded changes from the classical description. In the standard scheme of quantum theory that emerged between 1926 and 1928, notably in Göttingen, Cambridge, and Copenhagen, the following description arose:

(i) Microscopic natural phenomena could be treated on the basis of the theories of matrix and wave mechanics, i.e., formally different but mathematically equivalent algebraic and operator formulations.
(ii) The quantum-mechanical theories satisfied the known conservation principles of energy, momentum, angular momentum, electric charge and current, etc.
(iii) The visualizable (*anschauliche*) particle and wave pictures of the classical theories had to be replaced by 'dualistic' or 'complementary' aspects of microscopic objects which exhibited simultaneous particle and wave features.
(iv) The causal structure known from the classical laws—i.e., the differential equations—remained valid for the quantum-mechanical laws, but the behaviour of quantum-mechanical objects deviated from those of classical ones.
(v) Based on Born's statistical interpretation of the wave function and Heisenberg's uncertainty (or indeterminacy) relations, Bohr (on the physical side) and von Neumann (on the mathematical side) proposed a subtle formalism that accounted for the measurement of microscopic properties by macroscopic instruments (and observers), in which the classical subject–object relation introduced 300 years earlier by René Descartes was replaced by a different one.

The completed physical theory of microscopic phenomena that thus arose, and was soon characterized as the 'Copenhagen interpretation of quantum mechanics,' was by no means accepted by *all* physicists, not even by all quantum physicists universally. Especially in Middle Europe, a lively debate arose from the late 1920's onward concerning several characteristic aspects of this interpretation. We have mentioned in Chapter II that already since the origin of the complementarity view, Erwin Schrödinger and Albert Einstein vigorously attacked the validity of its very basis, namely, Heisenberg's uncertainty relations. While Bohr and his associates, in particular Heisenberg and Pauli, had emerged victorious in this debate on the uncertainty relations—by demonstrating that the quantum-mechanical scheme was fully consistent as a mathematical theory and gave an adequate description of microscopic phenomena—a new debate started around 1930 (i.e., after the defeat of Einstein's arguments by Bohr *et al.* at the sixth Solvay Conference on Physics) about the consequences from the uncertainty relations for the principle of causality in quantum mechanics. Now Planck and Schrödinger argued vigorously against renouncing the (classical) causality concept, which had formed the basis of all previous successful physical theories and beyond. On the other hand, a powerful philosophical movement in Germany and its vicinity, notably positivism and the related views of the 'Vienna Circle,' supported, more or less fully, the Copenhagen interpretation. Simultaneously with these epistemological debates, certain theoreticians worked on the problem of whether the uncertainty relations would not break down when one would seek to extend quantum mechanics to relativistic phenomena. These investigations, carried out between 1930 and 1933, ended with the result that uncertainty relations existed also for relativistic fields; hence, the Copenhagen interpretation remained valid also in this domain.[797] The debate on causality and the extension of the uncertainty relations will be dealt with in Section IV.1.

In spite of his defeat in 1930, Einstein would not yield to the claim of the validity of the quantum-mechanical description of microscopic processes. After several years of preparation, he would publish with two collaborators a new and, as he believed, decisive blow: the so-called 'Einstein-Podolsky-Rosen (*EPR*) paradox' did not argue against the consistency of the modern quantum theory (involving, especially, the validity of the uncertainty relations) but rather attempted to show that the entire, though so successful, scheme violated the very essence of a physical theory, namely, to describe the 'reality' of nature completely. Bohr, Heisenberg, and others hurried to reply to Einstein's accusations by demonstrating that the view of physical reality assumed by their distinguished colleague simply did not apply to the microscopic domain. At the same time, Erwin Schrödinger analyzed, partly independently of Einstein, the intuitive (*anschauliche*) content of quantum mechanics and published his famous 'cat paradox.' This nonrelativistic example addressed the same reality problem which had been discussed by Einstein and his quantum-mechanical opponents in the relativistic example of *EPR*. We shall treat the purely epistemological debate between Einstein and Schrödinger, on

[797] We recall from Section II.7 that the most eminent quantum-mechanical experts were ready to accept a breakdown of their theory in the domain of relativistic and nuclear physics.

the one side, and the partisans of the Copenhagen interpretation, on the other, in Section IV.2.

It has been noted and emphasized by several historians of science that the debates on the philosophical contents of quantum mechanics were carried out mainly in Middle Europe. Paul Forman explained this fact by associating the philosophical ideas leading to the creation of quantum mechanics and its interpretation with the general sociological conditions of the Weimar Republic: the political and economic necessities following World War I in defeated Germany also ultimately nourished the emergence of doubts in the causality of physical phenomena (Forman, 1971). Nancy Cartwright, on the other hand, in an analysis of the response of some American physicists to the Copenhagen interpretation, argued that the limited participation of her fellow countrymen in the philosophical debates rested on 'the well-known American doctrines of pragmatism and operationalism':

> [This] philosophy stressed two things: (1) hypotheses must be verified by experiment and not accepted merely because of their explanatory power; and (2) the models that physics uses are inevitably incomplete and incompatible, even in studying different aspects of the same phenomena. (Cartwright, 1987b, p. 417)

The 'basic attitude' of the progressive young American quantum physicists (including J. C. Slater, E. U. Condon, J. H. Van Vleck, E. H. Kennard, E. C. Kemble, D. M. Dennison, N. Wiener, H. P. Robertson, and less so J. R. Oppenheimer) around 1930 'was that the task of physics is not to explain but to describe' the natural phenomena (*loc. cit.*).[798] However, by discussing the laws of quantum mechanics in the light of previously recognized principles of physical theory (notably, the relation between cause and effect), Bohr and his associates sought to found the new atomic theory as a generalization of the old dynamics: as in the old theories, they wished *to explain* rather than just *to describe* natural processes. And yet, in spite of all their epistemological efforts, they could not satisfy the demands of physicists of the older generation, whose ideal seemed to be the status of late nineteenth-century science before the quantum and relativistic phenomena enforced a change of the description. Some of them, like Philipp Lenard and Johannes Stark, were even more extreme and used the new political philosophy of the Nazis, ruling Germany since 1933, to demand a return to what they called the good 'German physics (*Deutsche Physik*)' or '*Aryan* physics,' i.e., the mathematically less abstract, directly visualizable physics that existed before the advent of the modern quantum and relativity theories. They accused Heisenberg, Sommerfeld, and even the old Max Planck of having created—together with Einstein and many well-known physicists who were driven out of German universities and

[798] In a sense, one might say that the different attitudes toward quantum mechanics represented by the Central Europeans, on the one hand, and the Americans, on the other, followed the old antipodean schemes of deductively describing the laws of nature from metaphysical principles (e.g., by Leibniz) or inductively deriving otherwise unexplained laws from observations (e.g., by Newton), respectively.

research institutes—a 'Jewish,' a 'degenerate' physics. The *Third Reich*, with its racial laws and other anti-democratic, nationalistic measures, certainly damaged seriously the cause of modern atomic theory in Germany—many Jewish and several of the other creators and distinguished representatives of quantum and relativity theories took away with them the fruits of the great tradition established in Middle Europe before and during the Weimar period.

Still, quantum theory continued to flourish during the 1930s, even in impoverished Germany, through many applications and extensions, which we shall sketch in the following sections of this chapter. In particular, the fields of nuclear and high-energy physics were incorporated into the descriptions based on quantum mechanics and relativistic quantum field theory. These successes began with the introduction of several new elementary particles, i.e., basic constituents of matter, besides the already known proton and electron, two of them having been predicted and the others detected by surprise. In Section III.7, we have already mentioned the 'neutron,' assumed hypothetically by Pauli to rescue energy conservation in the beta-decay (December 1930). Then, in summer 1931, Dirac interpreted the negative-energy states of his relativistic electron equation as a positively charged 'anti-electron,' which would be identified in the following year by the American physicist Carl Anderson with a particle detected in cosmic radiation and named the 'positron.' The existence of the positron, and in particular, the creation of electron–positron pairs by very energetic gamma-rays, explained many phenomena in high-energy physics, as well as the so-called Meitner–Hupfeld anomaly referred to in Section III.7. On the other hand, a neutral particle, having approximately the mass of the proton, was identified in February 1932 by James Chadwick in certain nuclear reactions. This object, foreseen clearly by Rutherford in 1920, performed a tremendous job in removing most of the previous difficulties encountered by the physicists when they tried to apply the quantum-mechanical scheme to nuclear structure: that is, all of the problems noticed earlier in connection with the existence of electrons in nuclei and the statistics of certain nuclei disappeared all of a sudden. Thus, in 1932, nuclear physics became a well-defined branch of standard quantum mechanics. Heisenberg hurried to make use, in the same year, of Chadwick's heavy 'neutron' to establish the proton–neutron structure of the atomic nuclei and started to explain their masses, or, more accurately, their binding energies by assuming new exchange forces; two years later, a young Japanese physicist—Hideki Yukawa—associated these exchange forces with another new elementary particle, which he called the 'heavy quantum.' Even earlier, in December 1933, Enrico Fermi developed a consistent quantum-theoretical description of beta-decay by making use of Pauli's light 'neutron' of 1930, which he (in 1932) properly renamed the 'neutrino.' These wonderful discoveries in nuclear and high-energy physics, which came to a peak in the '*annus mirabilis*' of 1932, will be treated in Section IV.3.

Also in the low-energy domains of condensed matter physics, namely, solid-state and low-temperature physics, the 1930's proved to be a quite fruitful period for the application of quantum-mechanical methods and principles, as we shall summarize in Section IV.4. On the one hand, the theory of metals and solids, established so successfully between 1927 and 1932 (and described in Section III.6), was further developed, especially in the USA (with John Slater and the Hungarian immigrant, Eugene Wigner, and their students playing a leading role) and England (where, for example, Nevill Mott in Bristol formed a new school). On the other hand, the newly investigated anomalous behaviour of helium at temperatures around 2K (notably, the superfluidity discovered by Peter Kapitza in late 1937) became amenable to treatment by means of quantum theory. Only the old riddle of low-temperature physics—superconductivity—still lacked real theoretical understanding from first, microscopic principles, in spite of the great progress made in the macroscopic description of the phenomenon.

The most exciting results in the second half of the 1930's were again achieved in nuclear and high-energy physics, although the formalism exhibited, at least in the relativistic domain, serious defects as had been noticed already by Heisenberg and Pauli in their pioneering work on quantum field theory of 1929. All of the field theories devised to explain elementary particles and their interactions—whether the original quantum electrodynamics, the so-called 'Fermi-field' theory (developed from Fermi's beta-decay theory in order to account for binding and scattering processes in nuclear or high-energy physics), or the various forms of Yukawa's heavy quantum (soon to be called 'meson') theory of nuclear forces—yielded most disturbing infinite results for fundamental properties, like the masses of particles or cross sections of characteristic processes. These principal infinities would be handled only later by the procedure of 'renormalization,' first in the case of quantum electrodynamics. On the other hand, many experimental findings, in the first place the discovery of the 'mesotron' by Anderson and Seth Neddermeyer toward the end of 1936, encouraged the quantum-field theorists. The new cosmic-ray particle not only seemed to have the properties demanded by Yukawa for the 'meson'; since it was unstable and decayed in milliseconds when at rest, it also accounted for the existence and the properties of the hitherto unexplained 'penetrating component' of cosmic radiation. The special task for the theoreticians remained to select from the available quantum-field theories—the scalar theory of Pauli and Viktor Weisskopf of 1934 or the vector theory proposed in late 1937 independently by Yukawa and his Japanese collaborators, on the one hand, and several theoreticians in Great Britain (Nicholas Kemmer, Homi Bhabha, Herbert Fröhlich, and Walter Heitler), on the other—*the* suitable candidate, which would allow one to calculate both the binding energy of characteristic nuclei (especially of 'deuterium,' the nucleus of the heavy water atom) and the high-energy scattering of nuclear particles. While a host of problems remained unanswered in the late 1930's, the general investigation of quantum-field theories as *the* tool for describing elementary particles made great progress; thus, Pauli and Markus Fierz in Zurich proved the general spin-statistics theorem (1938), and Frederick Belinfante in Leyden discovered the symmetry of quantum fields under 'charge conjugation' (1939a). We shall close Section IV.5 of this chapter with another discovery in the domain of nuclear physics, which would create an even bigger stir among the scientists and the public at large: nuclear fission. This new, and completely unexpected, mode of nuclear reactions, observed by the chemists Otto Hahn and Fritz Straßmann (1939a) when scattering slow neutrons by uranium nuclei, could be immediately explained on the basis of the standard quantum-mechanical nuclear theory.

In the early 1930's, the time began when quantum mechanics advanced to the status of an established fundamental theory, on the basis of which the various branches of physics became reorganized: atomic physics, molecular physics, solid-state physics (with its subfields of metal, semiconductor physics, etc.) and the physics of condensed matter (especially low-temperature physics), on the one hand, and nuclear and elementary particle physics (emerging, to a large extent, from cosmic-ray physics and still called, until the early 1950's, high-energy nuclear physics), on the other. The community of quantum physicists, whose eminent members earlier covered in their theoretical investigations several, if not all, fields, now began to split into well-defined parts or groups of specialists dealing with one or at the most two topics. The history of quantum theory consequently branches out into separate histories of all of these fields, each of which deserving its own detailed treatment. Such a task would clearly surpass the goal of the present series of volumes on *The Historical Development of Quantum Theory*. Sections IV.3 to IV.5 therefore address only the essential quantum-theoretical ideas involved in the new fields; none of them would discuss any topic in its entirety, as this would require a series of different historical accounts—only a few of which have been attacked so far (e.g., in the book of Hoddeson *et al.* (1992) on the history of solid-state physics, or in that of Brown and Rechenberg (1996) on the origin of the concept of nuclear forces).

The development of these new topics demanded the work of many scholars; even new schools arose, for example, the Bristol school of Nevill Mott in Great Britain or the MIT school of John Slater in the United States, both devoted to research on solid-state physics. Of course, also in the more specialized physics of the 1930's, the great figures, who had ushered in the quantum-mechanical revolution in the 1920's, remained leaders in many of the new developments, especially Bohr, Dirac, Heisenberg, Pauli, and Wigner, supported by their early gifted disciples (from Bloch and Heitler to Peierls and Rosenfeld). On the other hand, the influence of old masters like Born and Sommerfeld became diminished, less by their age than by the formidable difficulties created by the *Third Reich* in Germany, which forced the former into emigration and denied the latter to choose an appropriate successor to continue the work of his school.

Indeed, the forced emigration of a large part of the best of the older as well as the younger generation from Germany played a decisive role in the contributions from various other countries to quantum physics of the later 1930s. One can say that through the actions of the Nazi Government (which came to power in Germany in early 1933) nearly a whole generation of the most talented quantum theorists got lost to Germany; not all of them were Jews or of Jewish origin (hence, fell under the racial laws of 1933 and 1935), but quite a few also had to leave or left voluntarily because of political reasons (because they were associated with liberal to leftist ideas).[799] The emigrating quantum theorists then flooded in large numbers into the other countries, preferably to the West (mostly to Great Britain and the USA, less so to France, Spain, and some countries of South America), but some also to the East (from Czechoslovakia to the Soviet Union, Turkey, and even China). Much has been argued about the effect of this emigration, in particular about the role played by the theorists coming from Germany in establishing and strengthening quantum physics in their new home-countries. Paul Hoch, who studied the situation in several cases more closely, arrived at more modified conclusions concerning the immediate influence of the emigrants from Germany, pointing out that their reception by the scientific communities abroad was often lukewarm; thus, he warned about overrating the support the emigrants received, especially in the most favoured host countries, Great Britain and the USA, by referring to the historical situation as it existed then:

> This is all very well, and was to be very important as a foundation for the growth of theoretical physics in America in the subsequent decades. But in 1933 the number of theoreticians on the physics staff at those institutions considered to be the main centres of this discipline in America was—with the possible exception of the University of Michigan—tiny compared to those primarily engaged in experimentation, and rarely exceeded one or occasionally two permanent staff.
>
> If one is going to write a dispassionate history of the transmission of this new branch of knowledge, one has to bring into the equation not only those factors facilitating it but also those opposing it. The predominant attitude to physics in Britain and America at that time was that it was—and should be almost as matter of morality—an experimental science (at Oxford it was still known as "experimental philosophy"). Cambridge had a considerable mathematical aspect to its physics at least since Maxwell, which embraced in part the work of Kelvin, Rayleigh and J. J. Thomson among others. However, by the 1930s it was assumed even within this tradition that Cavendish physicists did their own experiments and that those not doing experiments were not physicists and belonged to the mathematics faculty. This was still the case in the early 1930s, even for the Stokes Lecturer in Mathematical Physics, Ralph Fowler, and for his most promising students Nevill Mott and Alan H. Wilson, all of whom were attached to mathematics. [Moreover, Fowler's] own main interests were within statistical and older mathematical physics, rather than, for example, in the new "German" quantum mechanics and its application to solids, in which such visitors to Germany and Denmark as Wilson and Mott were to play a considerable part. In the years after 1933 a great number of refugee theoreticians obtained temporary accommodation at Cambridge including Hans Bethe, Max Born, Rudolf Peierls and P. P. Ewald, among others. But none of these was able to obtain a post on the permanent staff and all went elsewhere. (Hoch, 1990, pp. 24–25)

The situation, as Hoch described further, was similar at Oxford, where actually only the brothers Fritz and Heinz London settled for a longer period.

[799] Many others (like Hermann Weyl and Erwin Schrödinger) just resigned from their posts, because they did not wish to live in the atmosphere created by the *Third Reich* and refused to swear an oath of allegiance to the *Führer* Adolf Hitler.

To these reasons, which arose from the general background of science in Great Britain and USA, often also anti-Semitic sentiments in the faculties (especially in the USA) were added to prevent an early integration of the immigrants. They mostly occupied positions and treated subjects which the local people did not favour, investigating especially theoretical problems of nuclear physics (partially needed by the well-established experimentalists in their host countries).[800] Many of them later participated decisively in the nuclear energy projects during World War II in Britain and in the USA and, as a consequence of their meritorious work at that time, established themselves as respected members of the scientific community after the war. But this is quite another story which transcends the aim of this chapter and the subjects to be discussed here.

IV.1 The Causality Debate (1929–1935)

(a) Introduction: The Principle of Causality in Quantum Theory

In a dictionary of physics, the concept of causality is defined as follows:

> The physicist considers causality as identical with determinism, that is, with the unique fixing of the future events by the present ones according to the laws of nature. (Westphal, ed., 1952, p. 649)

Since in classical physics the fundamental equations of nature were differential equations, the 'deterministic hypothesis,' as formulated toward the end of the 19th century, says: If one knows at a given instant *the initial values of all* parameters describing the system considered, then one can calculate the values of these parameters for *all future times*. Evidently, this hypothesis worked well in Newtonian mechanics and Maxwell's electrodynamics. It could also be taken over into relativistic dynamics and Einstein's theory of gravitation of 1915, as David Hilbert remarked:

> By knowing the [physical quantities and their time derivatives] in the present, once and for all the values of these quantities can be determined in the future, *provided they have a physical meaning*. (Hilbert, 1917, p. 61)

Clearly, the classical statistical mechanics also did not affect the 'deterministic hypothesis' as such: The probabilistic description involved in it was considered only as a device to calculate in a simple and comfortable manner the gross properties of a large assembly of particles, and the clever 'Maxwell demon' would then be able to disentangle all individual particle trajectories, which obeyed deterministic classical laws.

[800] Particular examples have been discussed with respect to the 'British' theoreticians developing the meson theory of nuclear forces in 1937 and 1938 (Brown and Rechenberg, 1996, Section 7.4).

The situation changed only with the investigation of certain phenomena after 1900, especially the blackbody radiation law of Max Planck (1900f), the nature of the law of radioactivity by Egon von Schweidler (1905), and the emission and absorption processes of radiation by Albert Einstein (1916d). Hence, the former trained physicist and later philosopher Moritz Schlick attempted to endow the 'causal principle' with a more adequate formulation by stating:

> The causal principle is not a natural law itself but rather the general expression of the fact, *that* everything which happens in nature obeys laws which are valid without exception.... First, one realized that the events happening at one instant of time are only determined by the events happening at the immediately preceding instant, i.e., the dependence does not extend without intermediate action over distant times.... A further extended and increasingly better justified experience has made it very probable that ... in space there exists as little an action-at-a-distance as in time: the natural processes therefore are completely determined by those in the immediate vicinity and depend only via the intermediate action of the latter on those at a larger distance. *The intermediate action could occur also discontinuously, hence finite differences would replace the differentials. The experiences of quantum theory warn us not to lose sight of this possibility*. (Schlick, 1920, pp. 461–462)[801]

Around 1920, a new discussion indeed arose, especially in Germany, about the meaning of causality, triggered by the progress of quantum theory.[802] Walter Schottky of Würzburg published in June 1921 a popular article on '*Das Kausalproblem der Quantentheorie als eine Grundfrage der modernen Naturforschung*

[801] We have added emphasis to the last two sentences by italics.

Moritz Schlick was born in Berlin on 14 April 1882, the son of an industrialist. He studied at the University of Berlin and received his doctorate in 1904 under Planck's direction with a thesis, '*Über die Reflexion des Lichtes in einer inhomogenen Schicht* (On the Reflection of Light in An Inhomogeneous Layer).' Immediately afterward, he turned his attention from the problems of theoretical physics to those of a very general philosophical nature and started a career in the philosophy of science. In 1910 he received his *Habilitation* at the University of Rostock; in 1917, he was promoted to a professorship there, before moving to Vienna in 1921 as a full professor to occupy the philosophical chair previously held by Ernst Mach and Ludwig Boltzmann. In Vienna, he created a school of the logic of science (*Wissenschaftslogik*) and the foundations of mathematics, the '*Wiener Kreis* (Vienna Circle).' Schlick was shot to death on 22 June 1936 by a former student in the University of Vienna. With his publications on the philosophy of modern physical theories, especially relativity theory and quantum theory, and through the 'Vienna Circle,' he became one of the most original and influential teachers in the philosophy of science. For more details on the life and work of Moritz Schlick, see his obituary by Zilsel (1937).

[802] In an article on the cultural origin of statistical causality, Norton M. Wise traced 'the idea of indeterministic statistical causality' back to the period between 1870 and 1920 in Central Europe, referring especially to the physiologist Wilhelm Wundt and his Leipzig colleague, the historian Karl Lamprecht, and later to the Danish philosopher Harald Høffding, who influenced Niels Bohr (Wise, 1987). We might add here the name of Franz Exner, the teacher of Erwin Schrödinger, who pondered at least since his rectorial address of 1908 about the statistical nature of physical laws, and expanded on the subject in his 1919 book on a new concept of causality, arriving at the following conclusion: 'There must exist causes which direct the *average* processes, but only those and not the individual ones, into lawful courses. (*Es müssen Ursachen vorhanden sein, welche das durchschnittliche Geschehen, aber auch nur dieses, und nicht Einzelheiten bedingen und in gesetzmäßige Bahnen leiten*.)' (Exner, 1919; quoted from the second edition, 1922, p. 676)

überhaupt (The Central Problem of Quantum Theory as a Fundamental Problem of Modern Science in General).' (Schottky, 1921b) Schottky argued in particular that the interaction between matter and electromagnetic radiation, as considered by Planck, Einstein, and others, seems to put the strict causal law of classical mechanics into doubt; that is, when considering a quantum jump, one cannot ask the question: 'How does such a jump occur from one "orbit" to another, under what conditions does it happen, how long does it last, etc.?' (Schottky, *loc. cit.*, p. 507) Rather:

> What can be grasped by the concept of causality ... are the conditions for the frequency of occurrence of elementary events of a definite type. However, for this purpose the laws ... are so strict and general in validity that one never finds a deviation, as long as one just takes a sufficiently large number of elementary processes together or adopts a point of view, in which the assumed "structure" of processes does not show up anymore. (Schottky, 1921b, p. 511)[803]

Though based on other results of modern atomic theory, Walther Nernst argued for a similar weakening of the causality principle in his Berlin rectorial address of 15 November 1921 (Nernst, 1922, especially, p. 494).

The turbulent development of quantum theory in the following years led to a series of speculations about the nature of physical laws, of which the radiation theory of Niels Bohr, Hendrik Kramers, and John Slater, proposed in early 1924, suggested the most radical departure from classical causality.[804] The quantum and wave mechanics, which emerged in 1925 and 1926, then restricted these speculations again. The conservation laws, violated in the previous Bohr–Kramers–Slater approach, regained full validity in the new atomic theory; however, now Max Born's statistical interpretation of the wave function implied a breakdown of the causality hypothesis for all atomic processes, which Born replaced by the statement: 'The motion of particles conforms to the law of probability, but the probability itself is propagated in accordance with the law of causality.' (Born, 1926b, p. 804) This interpretation of the quantum-mechanical formalism initiated—as we have shown in Chapter II in this volume (and in Volume 5, Part 2, Section IV.5)—the fundamental debates, especially in Copenhagen, leading to Heisenberg's uncertainty relations and Bohr's principle of complementarity. Then, in March 1927, Heisenberg drew the radical conclusion:

[803] Walter Schottky, whom we have mentioned several times in earlier volumes, was born on 23 July 1886, in Zurich and studied at the University of Berlin from 1904 to 1912, obtaining his doctorate under Planck's guidance in 1912 with a dissertation on relativistic dynamics. Then, he became an assistant to Max Wien in Jena and worked on problems of electron tubes and on the thermal emission of electrons in high-voltage electric fields ('Schottky effect,' discovered in 1914). In 1915, he accepted a position in the laboratory of Siemens & Halske in Berlin; in 1920, he returned to an academic career at the University of Rostock (extraordinary professor, 1923, full professor, 1926). From 1927 to 1951, he finally worked as a leading scientist for the Siemens & Halske and Siemens-Schuckert Companies in Berlin and Pretzfeld, developing in particular the theory of defects in crystals and of semiconductors. Schottky, one of the great pioneers in this field, died on 4 March 1976, at Forchheim.

[804] The BKS theory and its implications have been discussed in Volume 1, Part 2, Section V.2.

> Since all experiments obey the equation
>
> $$[p_1 q_1 \sim h] \qquad [(631)]$$
>
> [with p_1 and q_1 denoting the minimum inaccuracies of momentum and position of quantum-theoretical particles], the incorrectness of the law of causality is a definitely established consequence of quantum mechanics itself. (Heisenberg, 1927b, p. 197)

He found, especially, that in the old statement of causality—'The exact knowledge of the present allows the future to be calculated'—'not the conclusion but the [initial] hypothesis is false' (Heisenberg, *loc. cit.*).

About a year later, he had accepted the wider conclusions from his own findings, as formulated by Bohr in the principle of complementarity, and stated in a public address the deficiency of the causality principle as follows:

> The [classical] formulations of the causal law have shown themselves to be untenable in modern physics.... To obtain a statement about an object to be agreed upon, one must observe it. This observation implies an interaction between the observer [i.e., subject] and the object, which changes the object. For the smallest particles the interaction becomes so strong that observation often means destruction. Bohr has coined the concept of "complementarity" to describe the situation more appropriately. An accurate knowledge of the velocity [of the particle] excludes an accurate knowledge of [its] position; the former is complementary to the latter. Or, the causal description of a system is complementary to the space-time description of the same system. Because, in order to obtain a space-time description, one must observe, and this observation disturbs the system. If the system is disturbed, we cannot follow anymore its causal connection in a pure manner....
>
> [Consequently], the simple deterministic concept of nature that existed in the previous [classical] physics cannot be carried out anymore. The interaction between observer and object renders a clear causal connection impossible. Of course, one can again think of formulations of the causal principle, which are compatible with modern physics. The most trivial example would be, say, "Everything that happens, also must happen." This statement is, however, meaningless, it does not tell us anything. Or, also, "If one knows the parameters of a system accurately, one can describe the future." This statement is equally meaningless. (Heisenberg, 1984d, pp. 26–27)[805]

When Heisenberg published his paper containing the relation [(631)]—in the above quotation—he created a considerable echo that reached beyond Europe. For example, E. H. Kennard of Cornell University, who—during his stay in Copenhagen in summer 1927—had already analyzed the derivation of Heisenberg's uncertainty relations and defined the inaccuracies as 'mean square deviations' (Kennard, 1927), later argued that they referred less to 'the errors of a simultane-

[805] The quotation is from the address, entitled '*Erkenntnistheoretische Probleme in der modernen Physik* (Epistemological Problems of Modern Physics)' post-humanly published (in Heisenberg, 1984d, pp. 22–28). This address may have been Heisenberg's inaugural lecture upon his appointment as Professor of Theoretical Physics at the University of Leipzig; lecture delivered in 1928.

ous observation of both [canonically conjugate] quantities' but rather 'primarily to the "statistical situation" determined by the experimental conditions and our knowledge of them' (Kennard, 1928, pp. 345–346). Arthur E. Ruark of the University of Pittsburgh, on the other hand, proposed 'an arrangement of the apparatus which seemed to make possible the simultaneous determinations of the coordinate q and the momentum p of a free particle, so accurately that Heisenberg's relation $\Delta q \cdot \Delta p \sim h$ is violated'; however, he noticed immediately that 'the precision of the measurement of both p and q is limited by statistical fluctuations in the measuring apparatus,' especially: 'The true reason for the validity of the principle is that slight velocity changes occur when the particle passes through a variable slit.' (Ruark, 1928, p. 709) Finally, Howard Percy Robertson of Princeton provided a mathematical proof of the relation for generalized coordinates in a letter of 18 June 1929, published in July of that year (Robertson, 1929). Altogether, the Americans did accept Heisenberg's result readily, but showed little interest in the complementarity philosophy in which the Copenhagen protagonists had embedded the relation [(631)].[806] They rather asked practical questions connected with it, for example, about the 'the length of light-quanta' (Breit, 1927). Such a question seemed to his philosophically ambitious European colleagues quite irrelevant: Philipp Frank of Prague discussed in a paper, entitled '*Über die "Anschaulichkeit" physikalischer Theorien* (On the "Visualizability" of Physical Theories)' and published in a February 1928 issue of *Naturwissenschaften*, the consequences derived from Heisenberg's work on the concept of *Anschaulichkeit* (visualizability, perceptualness, intuitiveness), which was also at the basis of Bohr's investigations leading to the principle of complementarity.[807]

After sketching the contents of Heisenberg's pioneering paper (Heisenberg, 1927b), Frank wrote the following comments:

> If one then thinks that nothing has been stated [in Heisenberg's γ-ray experiment] about the position and the velocities of electrons themselves, but only something about the possibilities of obtaining an accurate measurement, we have to reply: One must distinguish between the mathematical concepts of position coordinates and the velocity coordinates as physical events. As to the latter, they are grasped just through the properties of the scattered light; in the former sense, however, quantum mechanics demonstrates that the components of the coordinates of points do not constitute the most suitable quantities to represent radiation phenomena. But there is nothing "visualizable" in these material or electrical points [i.e., in the mathematical position coordinates of particles or radiation]. (Frank, 1928, p. 124)

Frank further claimed that 'the requirement for a representation [of atomic objects] by moving points or aether vibrations has nothing to do at all with the

[806] For details, see the review by Cartwright (1987b).

[807] When Frank wrote his paper, he had not yet seen Bohr's papers on the principle of complementarity (which he therefore did not refer to).

requirement of *Anschaulichkeit* but is just connected with a certain *Weltanschauung* (world view) which is composed of two [quite different] aspects,' namely, the 'materialistic view of nature' and the 'idealistic view': The former assumes the existence of completely impenetrable small particles in vacuum, while the latter is based on the (Kantian) trinity of space, time and causality. Only a third point of view, the 'positivistic view' (represented by Ernst Mach) would allow one—in Frank's opinion—to resolve the contradictions between the first two views and to describe the situation in quantum mechanics on the basis of Heisenberg's results. Clearly, he claimed that the problem of visualizability was very much connected with the problem of causality; Bohr and Heisenberg would agree, in principle, though Frank's answer, given within the framework of Mach's positivistic view, would be somewhat different (see also Frank, 1929, and the discussion below).

(b) Heisenberg's Discussions Concerning the Positivism of the 'Vienna Circle' (1929–1932)

At the opening session of the fifth *Deutsche Physiker und Mathematikertag* in Prague, on 16 September 1928, two speakers addressed the philosophical consequences from the new physical theories, the local physicist Philipp Frank and the Berlin mathematician Richard von Mises. Frank (1929) embedded the results of relativity and quantum theories into the more recent epistemological thoughts of Henri Bergson, William James, and Ernst Mach, as well as those of Rudolf Carnap, Moritz Schlick, and Hans Reichenbach. From the quantum-mechanical situation, he concluded: 'The question can never be asked, therefore, as the physicist of the [old]-school philosophy puts it: "Does strict causality govern nature?" but rather: "What are the properties of the correlation between the events and the state variables connected by strict [mathematical] laws?"' (Frank, 1929, p. 993) He continued:

> When looked at from the point of view of the old philosophy [*Schulphilosophie*], the [physical theories of the 20th century] imply an undermining (*Zersetzung*) of rational thinking; they simply are prescriptions for representing experimental results but do not yield any recognition of reality which is left to other methods. However, for those who do not accept the non-scientific argumentations, *the present theories enforce the conviction that even in such questions, as the ones about space, time and continuity, still there exists a scientific progress which proceeds with the progress in our experiences*; that it is, therefore, not necessary to assume besides the green and growing tree of science a grey region occupied by the problems that never will be solved...., *but rather that there are no limitations where physics passes over into philosophy*, if one just formulates the task of physics in the sense of Ernst Mach—as formulated by Carnap—"to organize the perceptions systematically, and to derive from the existing perceptions conclusion for future perceptions." (Frank, *loc. cit.*, pp. 993–994)

Thus, Frank argued vigorously that the theories of the 20th century should deal with the extended Machian positivism of the 'Vienna Circle.'[808]

While Frank discussed the philosophical interpretation of the causal and the statistical features of the new theories, von Mises spoke about the mathematical formulation of these aspects (von Mises, 1930a). First, he pointed out that the causal principle had received many different expressions in the past and summarized:

> If physics, or science in general, based on progressing information, has finally adopted fully the methods of reasoning (*Schlußweisen*) and the ideas of [mathematical] statistics and accepted them as indispensable tools, then after some time nobody will think that thereby any philosophical demand will remain unsatisfied. In a word: The causal principle *will be changed* and will be *subdued to what physics requires*. (von Mises, *loc. cit.*, pp. 145–146)

The previous deterministic *Ansätze* of classical physics, so Richard von Mises argued, were connected with certain macroscopic concepts, such as density, dielectric constants and with 'directly observable' motions, say, of celestial bodies; as soon as one proceeds to the situation involving many bodies (especially atoms), however, the statistical description must be used that naturally implies a certain irregularity, e.g., a molecular disorder. This description then requires, in the first place, the theorem 'that every physical statement must represent a fact which can be checked by observation, i.e., with the help of a real experiment,' and 'the observations possess as a decisive property that are repeatable arbitrarily often, be it at different times, at different places, or by different means' (von Mises, *loc. cit.*, pp. 150–151). Of course, due to errors in measurement, the individual observations will exhibit a fluctuation; hence, one must take as the 'true value of a measurement *the expectation value of the ensemble under consideration*,' or: 'A [physical] theory will be verified by experiment, if the value calculated agrees with the "true value" of the observation, i.e., the expectation value as determined through the measured object and the measuring device which, strictly speaking, can be obtained only after having performed infinitely many measurements.' (von Mises, *loc. cit.*, p. 151)

Now the partisans of causal, deterministic theories insisted on a further 'arbitrary' assumption, namely: 'To each theoretical result there exists an infinite sequence of different experimental arrangements having an increasing accuracy

[808] The '*Wiener Kreis*' ('Vienna Circle') was founded, as mentioned earlier, by Moritz Schlick, and was described as follows: 'Numerous pupils, eager to learn and devoted [to learning], assembled around the new *Ordinarius* [in Vienna, 1922] and the more advanced students together with some teachers—among them the philosopher Rudolf Carnap, the mathematician Hans Hahn and other mathematical colleagues—to form a circle, which discussed under the guidance of Schlick problems of the logic of science and of fundamental mathematical research and investigated together the development of the philosophical results obtained. Through several publications within a common framework and the organization of several philosophical congresses, this working community became known—also outside—as the '*Wiener Kreis*' (Zilsel, 1937, p. 161).

such that, if measured in constant units, the size of fluctuations of the distribution obtained decreases continuously and finally approaches zero.' However, the mathematician Richard von Mises continued, the very existences of finite atoms rendered this extrapolation impossible, since:

> One would have to imagine that there exist measuring devices whose precision supersedes atomic dimensions, which would obviously imply giving up any physical content. Recently, especially Heisenberg has pointed out the necessity of describing the atomic experiments in detail, and by this he has thrown new light on the discussion of causality and statistics. (von Mises, *loc. cit.*, p. 152)

Indeed, the new quantum mechanics allowed one to calculate quantities in agreement with a statistical evaluation, and Heisenberg's considerations led to the conclusion: 'The actual measuring process, also in microphysics, does not represent an elementary but rather a statistical situation. (*Der konkrete Meßvorgang ist auch in der Mikrophysik kein Elementarvorgang, sondern ein statistisches Geschehen*).' Hence, von Mises declared:

> Strict determinism, as is ascribed usually to classical physics of differential equations, is only an *apparent* (*scheinbare*) property; it cannot be upheld, if one considers a theory in principle only as valid in connection with experiments that allow one to test it, i.e., one restricts oneself to what is perceptible by senses (*sinnlich Wahrnehmbare*) or observable "in principle." In macroscopic physics, the indeterministic elements are contained in the *objects* of observation and partly in the *measurement processes*; every microscopical phenomenon, however, contains intrinsically the statistical element, because this alone permits the transition to a mass phenomenon (*Massenerscheinung*) [as opposed to an individual one] and each *measurement* already represents such a thing [i.e., a mass phenomenon]. (von Mises, *loc. cit.*, p. 153)

These quite lively presentations show that the latest results of the quantum theorists were soon understood, accepted, and properly incorporated into the philosophy of science and the mathematical description in Germany.[809] The high point of this very positive reception of the recent results of the quantum physicists occurred at the *91.Naturforscherversammlung*, held from 6 to 11 September 1930, in Königsberg. A special '*Tagung für Erkenntnislehre der exakten Wissenschaften* (Conference on the Epistemology of Exact Sciences)' during this large assembly of scientists and physicians dealt with two topics, namely, the epistemology of science and the foundations of mathematics. On the latter topic, distinguished speakers discussed the main lines of these hot developments at that time: for example, R. Carnap treated the mathematical 'logic (*Logizismus*)' (of Bertrand Russell and others), A. Heyting the 'institutionalism' (*à la* L. Brouwer), J. von Neumann the 'formalism' (of Hilbert), and F. Waismann a 'critique of language' (*à la* Wittgen-

[809] Richard von Mises repeated his positive remarks on the quantum-theoretical results in developing a 'world view (*Weltbild*) of science' in a public lecture delivered on 27 July 1930 at the University of Berlin (von Mises, 1930b).

stein); finally, D. Hilbert gave his famous talk on the mathematical problems, ending with the enthusiastic statement, 'Instead of a stupid *Ignorabimus* our parole should be: "We must know, we shall know (*Wir müssen wissen, wir werden wissen*)"' (Hilbert, 1930, p. 963). Hilbert's optimistic remark also applied to the discussions of the second theme, dealing with 'the philosophical questions arising from quantum mechanics.' In a brief summary, Hans Reichenbach reported about the talks:

> W. Heisenberg (Leipzig) delivered a lecture on causality and quantum mechanics, preceded by one by H. Reichenbach (Berlin) on the concept of truth in physics (*physikalischer Wahrheitsbegriff*). The latter talk [Reichenbach's] started from a philosophical critique of the previous physics and explained how, already since some time, the emergence of the probability concept has led in physics to a revision of the physical concept of truth via replacing the alternate logic (*Alternativlogik*)—hitherto the only one known—by a logic of probability, for which a given theorem may have any degree of probability, chosen from the continuous values between 0 and 1. These ideas connected smoothly with Heisenberg's lecture which argued that [absolutely] rigorous statements about natural processes in microphysics cannot be made anymore, hence they are meaningless. Following these two talks, in which a remarkable agreement was expressed between the results of research in the philosophy of science (*Naturphilosophie*) and physics, a stimulating discussion took place that further clarified many details. (Reichenbach, 1930, p. 1094)

In a paper, entitled '*Die Kausalität in der gegenwärtigen Physik* (Causality in Present Physics)' and published in February 1931, Moritz Schlick wrote in his introductory remarks:

> The turn taken by physics in recent years on the question of *causality* could not have been foreseen in any case. As much has been philosophized about determinism and indeterminism, about constants, validity and checks of the causality principle—no one has as yet hit upon the possibility offered by quantum physics as the key to recognize that a kind of causal order exists in reality. Only *a posteriori* do we recognize where the new ideas branch off from the old ones, and we are a little amazed that we have previously always passed carelessly through the intersection. (Schlick, 1931, p. 145)

What the Viennese philosopher wished to emphasize was that—perhaps different from the impression given earlier by Frank, von Mises, and Reichenbach—the crucial new philosophical idea did not arise so much from the previous lines of argument, but was triggered by Heisenberg's 'uncertainty relations (*Ungenauigkeitsrelationen*).' 'The new thing which physics has contributed to the problem of causality does not consist in the fact that the validity of the causal law has been challenged at all, nor [in the claim] that the microstructure of nature would have to be described by statistical instead of causal regularities. All these ideas have been expressed earlier, in part a long time ago,' he declared, and further:

> Rather, the new thing consisted in the up to then never anticipated discovery that by natural laws themselves a limitation—in principle—in the accuracy of predictions has been fixed. This is sometimes totally different from the obvious idea that factually and practically there exists a limit in the accuracy of observations, and that the assumption of absolutely exact natural laws can be dispensed with in any case if one wants to account for the empirical facts. (Schlick, *loc. cit.*, p. 153)

How did Heisenberg, the man responsible for this drastic change in natural philosophy, see the situation?

In his lecture in Königsberg on '*Kausalgesetz und Quantenmechanik* (Causal Law and Quantum Mechanics),' delivered on 6 September 1930, Heisenberg presented in detail what he considered to be the causal law in the old physics and the extent to which it was violated by the new quantum mechanics (Heisenberg, 1931a). His earlier formulation (Heisenberg, 1927b) had been attacked recently in a book by Hugo Bergmann of the Hebrew University in Jerusalem, who had argued in particular that 'one cannot talk about a definite statement of the causal law being not valid in quantum mechanics, but at the most about its inapplicability' (Bergmann, 1929, p. 39), i.e., Heisenberg's statement 'if-then' would not be sufficient to prove the principle as being invalid.[810] Heisenberg answered by taking a longer excursion into the concepts which, he said, were empty and uninteresting if they could not be refuted formally. Thus he stated the simplest form of the causal law as: 'Everything that happens, also must happen.' (Heisenberg, 1931a, p. 174) On the other hand, the more serious formulation that 'if the present state of an isolated system is known through all the parameters, the future state can be calculated,' still remained valid, provided the interaction between the observing subject and the object could be made arbitrarily small. 'In the new quantum theory ... it is impossible, in principle, to determine all the parameters of an isolated system,' Heisenberg emphasized and continued:

> Therefore, the just mentioned formulation of the causal law is not proven to be false but just empty; it does not possess any domain of validity or application any more, hence it does not interest the physicist. (Heisenberg, *loc. cit.*, p. 175)

Clearly, Heisenberg wished to say the following. Even the well-known formulation of the causality principle by Immanuel Kant—i.e., 'If we find that something happens, we always assume that something precedes it, upon which it follows according to a [well-defined] rule'—had not been proven wrong by quantum mechanics, because the great philosopher had assumed it to be 'an *a priori* synthetic judgment' which could not be checked by experience. Now in quantum physics, the statement simply turned out to be 'impractical (*unpraktisch*).'

[810] For a brief account of the early discussion of the philosophical consequences of Heisenberg's relations, see Jammer, 1974, pp. 75–78.

(Heisenberg, *loc. cit.*, p. 176) From the properties of atomic systems, it rather followed that:

> The indeterminacy relations show first that an accurate knowledge of the parameters, which is needed in classical physics to fix the causal connection, cannot be achieved. A further consequence of the indeterminacy is that also the future behaviour of such an inaccurately known system can be predicted only inaccurately, i.e., only statistically. It is evident that through the indeterminacy relations the foundation for the precise causal law of physics gets lost, both whether it applies to the particle or the wave picture. (Heisenberg, *loc. cit.*, p. 177)

By just referring to the Schrödinger equation, which appears to be a causal equation (in the sense of any classical theory), Heisenberg said, one cannot reinstate the classical causal law, because the wave function does not determine the state of the system uniquely in space and time: To reach this situation, one had to observe the system, but then the indeterminacy relations would spoil the case. Even the idea of describing the observer and the system by a single wave function would not solve the problem, as there exists no space-time description in that case either.

In a lecture on '*Die Rolle der Unbestimmtheitsrelationen in der neuen Physik* (The Role of the Indeterminacy Relations in the Recent Physics,' presented on 9 December 1930, in Vienna, Heisenberg returned in detail to the causality question (Heisenberg, 1931b).[811] He now made the following statement about causality:

> In classical physics the causal law was formulated as: "If at a certain time all data are known for a given system, then it is possible to predict unambiguously the physical behaviour of the system also for the future." In quantum theory one may consider as data practically the representative [Schrödinger] function. . . . Then the prescription of the classical law is certainly wrong, because the physical behaviour of a system can in general be predicted only statistically from the Schrödinger function. (Heisenberg, *loc. cit.*, p. 370)

That is, the mathematical formalism of the theory 'does not realize anything from the indeterminacy relations'; just the transition from the Schrödinger function to the physical behaviour implies the statistical hypothesis; hence, 'one can always consider the perturbations created by the measuring apparatus on the system as the cause of the degeneracy' (Heisenberg, *loc. cit.*). Heisenberg finally concluded:

> If nature has built the universe from small constituents of finite size, namely electrons and protons, then the question: "What happens in regions smaller than these constituents?" should not make a reasonable sense. Therefore, these constituents should behave "*unanschaulich*," i.e., different from the objects of the daily life, in order that

[811] It should be noted that in 1930 Heisenberg always talked about the 'indeterminacy relations (*Unbestimmtheitsrelationen*)' rather than the earlier 'uncertainty relations' (which Heisenberg had referred to as '*Ungenauigkeit*' or '*Unsicherheit*' in that context, e.g., in Heisenberg, 1927b). But from 1930 onward, he systematically replaced the word 'uncertainty' always by 'indeterminacy.'

> nature in the small can be considered to be a closed system (*abgeschlossen*). Modern physics, for the first time has shown how such a closure of the microworld might be conceivable in principle; the epistemological (*erkenntnistheoretische*) discussions, which have led to this goal, have clarified our thinking, made the language precise, and offered us a deep insight into the essence (*Wesen*) of human knowledge about nature. (Heisenberg, *loc. cit.*, pp. 371–372)

As we have mentioned earlier, the Viennese philosopher Moritz Schlick took up Heisenberg's results on causality and embedded them into a more professional philosophical system. He also rejected the criticisms of his colleagues like Hugo Bergmann's, though with a slightly different argument:

> There do not exist synthetic judgments *a priori*. If a theorem states anything at all about reality (and only if it does so, it of course contains some knowledge), then by observing reality one must be able to show whether it is right or wrong. If there exists no possibility, in principle, for testing, i.e., the theorem is compatible with any possible experience, then it cannot contain any knowledge about nature. If, by assuming the theorem to be wrong, anything in the world of experience were different from the situation for which the theorem is right, a test would be possible; hence the impossibility of a test by experience means that: the view of the world does not depend at all on the theorem being right or wrong, hence it says nothing about nature. (Schlick, 1931, pp. 153–154)

In general, Schlick continued, three different attitudes could be taken toward the principle of causality, namely:

1. The principle of causality is a tautology. In this case it would always be true but without content (*nichtssagend*).
2. It is an empirical theorem. Then it is either true or false, either knowledge or error (*Erkenntnis oder Irrtum*).
3. It constitutes a postulate, a demand to look further for causes. In this case, it can be neither true nor false, but at most useful or not useful. (Schlick, *loc. cit.*, p. 154)

Now, a tautology was certainly not what was needed in science; on the other hand, the causality principle used so far did not seem to have the character of a physical law; hence, only the third interpretation remained. Indeed, from Heisenberg's indeterminacy relation, definitely a physical law, there followed 'a rejection of determinism.' However, Schlick continued that this 'rejection cannot be considered as a proof for a statement to be untrue, but rather as the demonstration that a rule is not suitable,' and 'there always remains the hope that the causality principle will again become triumphant as knowledge progresses.' He concluded:

> The rejection of determinism by modern physics means neither that a statement is wrong nor that it is empty; but the prescription, which as the "causal principle" shows the path to every induction and every natural law, is unsuitable. The unsuitability is claimed only for a well-defined, limited domain, but there it is connected with every certainty implied in the physical experience of today's research. (Schlick, *loc. cit.*, p. 156)

Evidently, Schlick wished to retain as much of the causality principle as possible for future physics.[812]

It seems that Heisenberg either met Schlick in Vienna or got into direct contact with the philosopher about that time. In any case, toward the end of December 1930, he wrote him a letter thanking him for his 'interesting essay on the law of causality' and said that he had 'learned much from it' and that:

> the tendency of it [Schlick's essay] is extraordinarily pleasant (*außerordentlich sympathisch*) to me. In particular, the clear distinction among the three possibilities [to interpret the principle of causality] was very instructive for me; I have tried to present something similar in my lecture at Königsberg, but I did not succeed in bringing it out clearly. (Heisenberg to Schlick, 27 December 1930)

Still, Heisenberg had a few objections. He did not understand really 'the difference between [the terms] order, lawfulness and "statistical lawfulness" ' used by Schlick, and he especially criticized the latter's description of Born's interpretation of the wave function as being 'split into two parts: in the strictly lawful propagation of the ψ-wave and the existence of a particle or a quantum that is absolutely accidental (*schlechthin zufällig*) within the limits of "probability," as given by the ψ-value at the position under consideration' (Schlick, 1931, p. 157). After proposing an example in atomic physics to discuss this point, Heisenberg wrote:

> Now what does "absolutely accidental within the limits of probability" mean? I cannot see any difference between your "statistical lawfulness" and that which we know from atomic physics. Further, I do not see which intermediate between full causality and disorder plus probability law can still be found. . . .
>
> I am also a bit unhappy that I am always quoted along with the statement of the "invalidity of the causal law," as if I were in opposition to Born's conceptions. At that time I considered the phrase "invalidity" quite carefully, intending to express two things: first, that the principle of causality has lost its applicability in physics . . .—which is not the same as the assertion that "it is *wrong*"; second, that a theorem having no domain of validity can really not be interesting. The word "invalid" seemed to me to lie just right in the middle between "wrong" and "inapplicable," but unfortunately it has always been identified with the word "wrong." (Heisenberg to Schlick, 27 December 1930)

Needless to say, Heisenberg agreed with Schlick's refutation of Hugo Bergmann's position. He closed the letter to the 'highly esteemed colleague (*sehr verehrte Herr Kollege*)' by correcting a few statements of the latter about Bohr's ideas of complementarity when applied to biological systems, and thanked him again for his 'extraordinarily instructive essay.'

In an immediate reply, dated 2 January 1931, Schlick expressed his apprecia-

[812] Schlick also contradicted the proposal of Hans Reichenbach that the causal law could only be extended to the future; hence, it fixed a direction in time for natural phenomena (see Reichenbach, 1925 and 1931).

tion of Heisenberg's quick reading of his manuscript, especially the clarification of Bohr's position. However, he still hoped to be able to retain the differentiation between strict lawfulness and pure accident in atomic theory, as stated in his manuscript (and later paper: Schlick, 1931). More than a year later, Schlick sent Heisenberg a new essay with the title '*Positivismus und Realismus* (Positivism and Realism),' published earlier in the journal *Erkenntnis* (Schlick, 1932). In it, he tried to summarize the principles of 'the philosophical methods (*Denkweisen*) known under the name positivism' since the invention of this concept by Auguste Comte in the first half of the 19th century, which consisted in the removal of the contrast between the 'true' or 'transcendental existence' of reality and the 'apparent existence,' as noticed by perceptions. In particular, Schlick focused on the different attitudes assumed by the 'realists,' on the one hand, and the 'positivists,' on the other, by investigating the two sets of problems: 'The meaning of statements' (in Section II) and 'What is the meaning of reality? What does the "external world" signify?' (in Section III). We shall return especially to the second part of Schlick's arguments in our next Section IV.2, but here we shall refer to Heisenberg's reaction, as it throws light on his position with respect to the positivistic movement which had thus far embraced his physical results.

In a letter dated 21 November 1932, Heisenberg thanked Schlick for a copy of his essay, and began by stating: 'Most of your assertions concerning your programme, I consider to be absolutely right, or, as you indicate on p. 8 yourself, completely trivial. To doubt the statement, which you regard as the central theorem of positivism, seems to me completely absurd.'[813] But then he immediately pointed out disagreements about their understanding of what is philosophy: In particular, Heisenberg did not appreciate the establishment of systems of 'artificial definitions' which seemed to him to suppress the 'important values' also of philosophy (which he felt to be closer to art than to photography). Hence, he wrote especially:

> Your definition of philosophy on p. 6 seems to me—please forgive me—completely off the track (*abwegig*). The question whether a certain philosophical statement is true or false, in most cases is completely uninteresting and irrelevant for the value of philosophy. For many deep statements of truth rather the fact applies [as Bohr once said] that the opposite of [a deeply true statement] is also a deep truth.... Of course, one may say that "these truths therefore contain only statements about experiences of sentiments," but this excuse appears to me as very suspicious. If we say, "Here is a table," what else is that but "the expression of the existence of certain feelings, which induce us to definite reactions of speech or other nature" (p. 28). If you reply, "I can show the table to everybody else," I'll tell you, "similarly one can create in every person the experiences, which are meant by the statements of philosophy." Perhaps, you will object, that philosophy is *partly* art and therefore valuable, but therefore no "science." I would, at this point, at the most admit that philosophy is a type of "chemical compound" of science and art (not just a mixture!) ..., at any rate, a compound transferring knowledge. (Heisenberg to Schlick, 21 December 1932, p. 2)

[813] In the published paper, there is just one statement marked as the 'central theorem (*Hauptsatz*),' namely: 'Only the given things are real (*Nur das Gegebene ist real*).' (Schlick, 1932, p. 4)

Even in science, Heisenberg added, the nonanalytically discovered 'suddenly sparkling recognitions,' such as Newton's discovery that the gravity of all bodies causes the planetary motion, constitute 'valuable knowledge.'

Heisenberg concluded by emphasizing the importance of the logical calculus for philosophy—'though this instrument [Heisenberg called it a "brilliant (*herrliches*)" system] is not yet philosophy.' While he did not believe at all in the possibility of a 'really "clear" language,' he believed 'that the best [thing] to achieve is to create clarity at the one little place, where a contradiction directs our attention to an obscurity.' As an example, he cited simultaneity in Einstein's investigation leading to special relativity theory. 'Please forgive me that I have used the words light-heartedly, ... I hoped you would prefer to hear a natural opinion rather than read a learned (*ausgetüftelte*) essay,' Heisenberg concluded his letter on philosophy to Schlick, and signed it with '*herzlicher Hochachtung* (cordial respects).' He had indeed written quite clearly about what he considered to be the central message of quantum mechanics for philosophy, when he remarked: 'It seems to me more than an unfortunate accident that [Philipp] Frank and [Hans] Reichenbach in their work hardly mention the real point of quantum theory, namely Bohr's [principle of] complementarity, and instead of it reproduce the much more superficial aspects in Born's papers and mine.' (Heisenberg to Schlick, *loc. cit.*)

(c) The Indeterminacy Relations for Relativistic Quantum Fields (1929–1933)

In spring 1931, the young Carl Friedrich von Weizsäcker (not yet quite 19 years of age) submitted his doctoral thesis under the direction of Werner Heisenberg in Leipzig, dealing with the determination of the position of an electron by a microscope (von Weizsäcker, 1931). He carried out, in particular, 'a rigorous calculation of the problem ... with the help of the Heisenberg-Pauli formulation of quantum electrodynamics' (von Weizsäcker, *loc. cit.*, p. 114). According to the standard discussion of the γ-ray *Gedanken*experiment of Heisenberg, the uncertainty of the position Δq assumed at least the value

$$\Delta q \sim \frac{\lambda}{\sin \varepsilon}, \tag{632}$$

with λ denoting the wavelength of the light used and 2ε the angle of aperture of the bundle of rays used for the imaging procedure. Von Weizsäcker then found that the procedures involved in the measurement of position were more complex than Heisenberg and Bohr had assumed in their 1927 discussions of the *Gedanken*experiment; especially, they included the illumination of the original electron at a space point P, the emission of radiation by this electron under the angle of aperture (2ε), *and* the stimulation of a second electron at the point P' of the observation screen. If he treated the problem according to proper quantum electrodynamics (with the Dirac equation describing the electron), he indeed obtained for the position probability a wave packet of size $\lambda/\sin\varepsilon$.[814]

Von Weizsäcker's thesis completed the demonstration of the 'elementary' relations by using field-theoretical methods. Two years earlier, Heisenberg 'contemplated how one could elucidate the uncertainty relations (*Unsicherheitsrelationen*) for the [electromagnetic] wave amplitudes.' As Heisenberg wrote to Bohr:

> As a matter of course, any measurement would yield not [the electric field strength] $\mathscr{E}$ and the [magnetic field strength] $\mathscr{H}$ at an exact *point* but average values over perhaps very small spatial regions. Let ΔV be the volume of this spatial region, then the commutation relations between $\mathscr{E}_i$ and Φ_k look like this,
>
> $$\mathscr{E}_i\Phi_k - \Phi_k\mathscr{E}_i = \delta_{ik}2hci\frac{1}{\Delta V} \qquad [(633)]$$
>
> where $\mathscr{E}_i$ and Φ_k are now to be interpreted as average values over the spatial volume $\Delta V(=(\Delta L)^3)$. Consequently, one would expect indeterminacy relations of the form
>
> $$\Delta\mathscr{E}_i\Phi_i \geq \delta_{ik}\frac{hc}{\Delta V} \quad \text{or} \quad \mathscr{E}_i\Delta\mathscr{H}_k \geq \frac{hc}{\Delta V\Delta L} \qquad [(634)]$$
>
> (Heisenberg to Bohr, 16 June 1929; English translation in Bohr, 1996, pp. 5–6)

Heisenberg then indicated two—as he admitted, not 'quite solid'—methods to derive Eq. [(634)]. In spite of the shaky derivation, however, he took over the last Eq. [(634)] into his lectures at Chicago [Heisenberg, 1930a, p. 50, Eq. (38)].

Bohr did not respond to the detailed contents of Heisenberg's letter until early in 1931 when the situation had changed drastically, as Léon Rosenfeld recalled nearly two decades later:

> When I arrived at the [Copenhagen] Institute on the last day of February 1931, for my annual stay, the first person I saw was Gamow. As I asked him about the news, he replied in his own picturesque way by showing me a neat pen drawing he had just made. It represented Landau tightly bound to a chair and gagged, while Bohr standing before him with upraised forefinger, was saying: "*Bitte, bitte, Landau muss ich nur ein Wort sagen!*" ["Please, please, Landau, may I just say one word!"] I learned that Landau and Peierls had just come for a few days before with some new paper of theirs which they wanted to show Bohr, "but" (Gamow added airily) "he does not seem to agree—and this is the kind of discussion which has been going on all the time." Peierls had left the day before, "in a state of complete exhaustion," Gamow said. Landau stayed for a few weeks longer, and I had the opportunity of ascertaining that Gamow's representation of the situation was only exaggerated to the extent usually conceded to artistic fantasy. (Rosenfeld, 1955, p. 70)

[814] Von Weizsäcker added the remark: 'Our result that the uncertainty of imaging quantum-theoretically will not be larger than $\frac{\lambda}{\sin\varepsilon}$, cannot be guaranteed for $\lambda = \lambda_0 \sim \frac{h}{\mu c}$ because of the size of the momentum transfer in the Compton effect.' (von Weizsäcker, 1931, p. 130)

Rosenfeld's reference was to the investigation, entitled '*Erweiterung des Unbestimmtheitsprinzips für die relativistische Quantentheorie* (Extension of the Indeterminacy Principle to Relativistic Quantum Theory),' which Lev Landau and Rudolf Peierls had completed in late January and would eventually submit in early March 1931 to *Zeitschrift für Physik* (Landau and Peierls, 1931). They started with the observation that the application of wave-mechanical methods to relativistic problems led to several 'senseless' results: first, the negative energy states of Dirac's electron equation; second, to 'hopelessly infinite divergence' of the interaction of a charged particle with itself; and third, to 'infinite matrix elements of the energy density.' Hence, they concluded:

> It is shown that by considering possible methods of measurement that all the physical quantities occurring in wave mechanics can in general no longer be defined in the relativistic range. This is related to the well-known failure of the methods of wave mechanics in that range. (Landau and Peierls, *loc. cit.*, p. 56; English translation, p. 152)

In order to support their special claim 'that in the range considered the physical requirements of the applicability of the methods of wave mechanics are no longer satisfied,' Landau and Peierls turned to what they considered a generalization of Bohr's ideas on the concept of measurement (as presented by Bohr in his lectures at Como and Brussels, 1928a, e, respectively). In particular, they argued that according to Bohr, for every quantum-mechanical system there should exist *predictable* measurements; i.e., 'measurements such that for every result there is a state of the system in which the measurement *certainly* gives the result' (Landau and Peierls, *loc. cit.*, p. 57); hence, they also concluded: 'If the wave function of the system cannot be determined by the measurement, it can have no meaning,' or 'the existence of predictable measurements is an absolutely necessary condition for the validity of wave mechanics' (Landau and Peierls, *loc. cit.*, p. 58). Now, in Bohr's scheme, every momentum measurement in time Δt is connected with a definite change ΔP (in addition to the unknown change which restricts the accuracy of the measurement due to the indeterminacy relation), given by the relation

$$(v - v')\Delta P > \frac{h}{\Delta t}, \tag{635}$$

where v and v' denote the velocities of the particle before and after the change. In the relativistic case, $v - v'$ assumes at most the value c; hence, they found

$$\Delta P \cdot \Delta t > \frac{h}{c}, \tag{635$'$}$$

or 'the concept of momentum has a sharp meaning only for long times.' (Landau and Peierls, *loc. cit.*, p. 61) This applies, in particular, for free particles, while for charged particles emitting radiation another additional momentum uncertainty, Δp, would result; i.e.,

$$\Delta p \cdot \Delta t > \frac{e^2}{c^3}(v - v'). \tag{636}$$

Now, for electrons (where $v' - v$ is of the order c), the uncertainty is smaller than the uncertainty (635′), because then $\Delta p \Delta t > \frac{e^2}{c}$ (and the small fine-structure constant); but for macroscopic bodies Eq. (636) becomes important; hence, both uncertainties have to be combined to give the final relation

$$\Delta p \Delta t > \frac{h}{c}\sqrt{\frac{e^2}{hc}}. \tag{637}$$

Landau and Peierls thus derived, e.g., in the case of the Compton effect, an additional scattering effect, consisting of 'a further, uncontrollable radiation ... obtained when higher approximations are taken into account in the perturbation-theoretical calculation for the interaction between radiation and particle' (Landau and Peierls, *loc. cit.*, p. 62).[815]

With these preparations, Landau and Peierls proceeded to consider the measurement of electric and magnetic field strengths. For the observation of the electric field $\mathscr{E}$, they employed a body of very large mass (hence small velocity, to keep the magnetic disturbance small), whose momentum accuracy, Δp, after the measurement processes was given by Eq. (637). Then, the accuracy $\Delta\mathscr{E}$ of the measured field strength was given by

$$\Delta\mathscr{E} > \frac{1}{e\Delta t}\Delta p = \frac{\sqrt{hc}}{(c\Delta t)^2}. \tag{638}$$

Similarly, for the accuracy of the magnetic field strength $\mathscr{H}$ followed in the case of a separate measurement

$$\Delta\mathscr{H} > \frac{\sqrt{hc}}{(c\Delta t)^2}. \tag{639}$$

In the case of simultaneous measurements of both electric and magnetic field strengths, the magnetic field of the charged test body had to be considered as well,

[815] In the case of the Compton effect, though, this extra radiation became quite negligible due to the smallness of $\frac{e^2}{\hbar c}$ (the fine-structure constant), as Landau and Peierls noted (Landau and Peierls, 1931, p. 62).

yielding an additional inaccuracy; thus, finally, there followed the relation

$$\Delta\mathscr{E} \cdot \Delta\mathscr{H} > \frac{hc}{(c\Delta t)^2} \frac{1}{(\Delta l)^2}, \tag{640}$$

where Δl was the distance between the test body and the magnetic needle (measuring the magnetic field strength $\mathscr{H}$). From Eqs. (638) to (640), Landau and Peierls concluded 'that for $\Delta t = \infty$, the measurement can be made arbitrarily accurate for both $\mathscr{E}$ and $\mathscr{H}$'; hence:

> Thus static fields can be completely defined in the classical sense.... In the quantum range, on the other hand, the field strengths are not measurable quantities. (Landau and Peierls, *loc. cit.*, p. 63)

That is, neither light-quanta nor material particles (such as electrons) could be measured—this impossibility then might explain also the well-known difficulties with energy conservation in beta-decay, Landau and Peierls argued at the end of their paper.

The investigation by Landau and Peierls caused considerable stir, not only in Copenhagen.[816] After some time, Pauli raised objections; in a letter to Peierls, he wrote:

> Obviously, it is wrong that the radiation energy $\frac{e^2}{c^3}\frac{(v-v')^2}{\Delta t}$ represents an *uncertain* energy change.... It may be that the radiation energy also contains some uncertainty in time development, but in the first approximation the radiation energy certainly represents a definite change. Hence the equation [(636)] is certainly wrong as an uncertainty relation. This is already clear from the fact that it does not contain h [Planck's constant] and, if correct, would postulate a fundamental uncertainty of charged particles in the classical theory. (Pauli to Peierls, 3 July 1931, in Pauli, 1985, p. 91; English translation in Bohr, 1996, p. 10)

However, in January 1933, when he read the proofs of his *Handbuch* article on wave mechanics, Pauli admitted the validity of the indeterminacy relations (638) and (639), while still denying Eq. (636). (See Pauli's letter to Heisenberg, dated 18 January 1933, in Pauli, 1985, especially, p. 150.) In his published *Handbuch* treatise, Pauli wrote:

> At this point, however, the argument of Landau and Peierls contains an essential gap, since the emitted-radiation momentum and the emitted-radiation energy can be measured accurately. The change of energy and momentum of the charged [test] body caused by them therefore cannot be regarded just as an indeterminate change. Because of this the further consequences are connected with an essential uncertainty, and the question of the field-strength measurement must be considered to be one that *has not yet been clarified.* (Pauli, 1933c, p. 257)

[816] See, for instance, the letters of Heisenberg to Peierls and Landau, dated 26 January 1931, and Heisenberg to Pauli, dated 12 March 1931, in Pauli, 1985, pp. 53–54, 66–67.

Pauli, in his letter to Heisenberg mentioned above, claimed that Bohr also considered Eqs. (638) and (639) to be correct. At that time, however, the Copenhagen team—now consisting of Bohr and Rosenfeld—had nearly completed their own investigation on the subject, leading to quite different conclusions. We do not know exactly—not even from Rosenfeld's recollections which we have quoted earlier—when they really began their work actively. It might have been already rather early, i.e., soon after Rosenfeld's arrival in March 1931, because the latter also recalled: 'My first task was to lecture Bohr on the fundamentals of field quantization; the mathematical structure of the commutation relations and the underlying physical assumptions of the theory were subjected to unrelenting scrutiny.' (Rosenfeld, 1955, p. 71) But it is clear that the main results were in hand on 2 December 1932, when Bohr presented them to the Danish Academy, although the finally published paper was signed only in April 1933.[817] In any case, Rosenfeld reported that Bohr took over the lead 'after a short time' and then 'he was pointing out to me essential features to which nobody had yet paid sufficient attention,' especially:

> His first remark, which threw decisive light on the problem, was that field components taken at definite space-time points are used in the formalism as idealizations without immediate physical meaning; the only meaningful statements of the theory concern averages of such field components over finite space-time regions. This meant that in studying the measurability of field components we must use as test bodies finite distributions of charge and current, and not point charges as has been loosely defined so far. The consideration of finite test bodies immediately disposed of Landau and Peierls' argument concerning the perturbation of the momentum measurements by the radiation reaction; it is easily seen that this reaction is so much reduced for finite test bodies, as to be always negligible. (Rosenfeld, 1955, p. 71)

However, the problem of constructing and using test bodies proved to be a long story which began with a quick result, namely, the case given by Heisenberg's Eq. (634)—in fact, the only case written by anybody—'was one in which unlimited accuracy had to be expected from the correctly integrated commutation law.' On the other hand, the correct relativistic treatment of extended bodies presented many difficult situations, especially when they were investigating whether relativity implied further restrictions to the measurability of momentum. 'This necessitated a much more detailed analysis of the measuring process than one was wont [to carry out] in an ordinary quantum mechanics,' recalled Rosenfeld, and: 'Bohr succeeded in showing that the measurement of the total momentum can even be performed in such a way that the displacements of the elements, though uncontrollable within a

[817] See the report in *Overs. Dan. Vidensk. Selsk. Virks Juni 1932–Maj 1933*, p. 35: '*Niels Bohr gav en Meddelelse: Om den begroensede Maarlelighed af elektromagnetiske Kraftfelter*'; or the announcement in *Nature* **132**, 75 (1933): 'Dec. 2, Niels Bohr: The limited measurability of electromagnetic fields of force. An investigation in collaboration with L. Rosenfeld proves the existence of a limitation of the measurability of electromagnetic field components, conforming with the tentative rational formulation of quantum electrodynamics, and analogous to the characteristic complementary limitations of the mechanical quantities, which secures the consistency of quantum mechanics.' (Reprinted in Bohr, 1996, p. 54)

finite latitude Δx, are equal, and that the determination of the total momentum is only limited by the uncertainty of the common displacement Δx to the extent $\hbar/\Delta x$, indicated by the indeterminacy relation.' (Rosenfeld, *loc. cit.*, p. 75) 'The reading of the fourteen or so successive proofs only took about one more year,' in which a final great trouble had to be resolved, namely, the role played by the field fluctuation in the logical structure of the theory. (Rosenfeld, *loc. cit.*, p. 77)

Bohr and Rosenfeld embarked upon their fundamental paper, '*Zur Frage der Meßbarkeit der elekromagnetischen Feldgrößen* (On the Question of the Measurability of the Electromagnetic Field Quantities),' with the firm conviction that 'the quantum theory of fields should be viewed as a consequent, correspondence-like reformulation of the classical electrodynamic theory, just as quantum mechanics constitutes a reshaping of classical mechanics corresponding to the existence of the quantum of action' (Bohr and Rosenfeld, 1933, pp. 3–4). In dealing with the topic properly, the fact had to be considered that the quantum-electrodynamical formalism did not depend *per se* on the atomic constitution of matter; hence, the effects of retardation—which played an essential role in the earlier investigations—could be neglected by choosing suitably extended test bodies (i.e., large compared to atomic dimensions) having an approximately constant charge distribution. Further, 'the field quantities are not represented by genuine point-functions but by functions of space-time regions, which correspond formally to the average values of the idealized field components over the regions under investigation' (Bohr and Rosenfeld, *loc. cit.*, p. 5). In relativistic field theory, an essential complication of measurement arose, because 'when comparing field averages over different space-time regions, we cannot speak generally in a unique manner about a time sequence of measuring processes, but already the interpretation of single results of a field measurement requires a still greater caution than in the case of usual [i.e., non-relativistic] quantum-mechanical measurement problems,' Bohr and Rosenfeld emphasized, and then sketched the main aspects of their treatment as follows:

> For measurements of field quantities, each result measured is well defined on the basis of the classical field concept; the limited application of the classical field theory for describing the unavoidable electromagnetic field actions of the test bodies in the measurements leads, as we shall see, to the consequence that those field actions influence to a certain extent the very result of the measurement in an uncontrollable manner. A closer study of the principally statistical character of the consequences from the quantum-electrodynamical formalism, however, demonstrates that this influence of the measuring process on the measured object does not restrict the possibilities to check such consequences in any way; it must rather be considered to constitute an essential feature of the intimate fit (*innige Anpassung*) of the quantum theory of fields to the problem of measurability. (Bohr and Rosenfeld, *loc. cit.*, pp. 6–7)

With these ideas, Bohr and Rosenfeld attacked their problem, emphasizing at once, however, that they would leave out completely the discussion of the well-known difficulties of quantum electrodynamics, primarily the infinite self-energy. This meant that they were able to deal in their programme entirely with the charge-free theory.

In that approach, which had been prepared several years earlier by Pascual Jordan and Wolfgang Pauli (1928), the commutation relations (see Section II.7) between the electromagnetic field components at the space-time points 1 and 2 assumed the form,

$$\left.\begin{aligned}
&[\mathscr{E}_x^{(1)}, \mathscr{E}_x^{(2)}] = [\mathscr{H}_x^{(1)}, \mathscr{H}_x^{(2)}] = i\frac{h}{2\pi}(A_{xx}^{(12)} - A_{xx}^{(21)}),\\
&[\mathscr{E}_x^{(1)}, \mathscr{E}_y^{(2)}] = [\mathscr{H}_x^{(1)}, \mathscr{H}_y^{(2)}] = i\frac{h}{2\pi}(A_{xy}^{(12)} - A_{xy}^{(21)}),\\
&[\mathscr{E}_x^{(1)}, \mathscr{H}_x^{(2)}] = 0,\\
&[\mathscr{E}_x^{(1)}, \mathscr{H}_y^{(2)}] = -[\mathscr{H}_x^{(1)}, \mathscr{E}_y^{(2)}] = i\frac{h}{2\pi}(B_{xy}^{(12)} - B_{xy}^{(21)}),
\end{aligned}\right\} \tag{641}$$

with the help of the relativistic generalizations of the Dirac δ-function,

$$\left.\begin{aligned}
A_{xx}^{(12)} &= -\left(\frac{\partial^2}{\partial x_1 \partial x_2} - \frac{1}{c^2}\frac{\partial^2}{\partial t_1 \partial t_2}\right)\left\{\frac{1}{r}\delta\left(t_2 - t_1 - \frac{r}{c}\right)\right\},\\
A_{yy}^{(12)} &= -\frac{\partial^2}{\partial x_1 \partial x_2}\left\{\frac{1}{r}\delta\left(t_2 - t_1 - \frac{r}{c}\right)\right\},\\
B_{xy}^{(12)} &= -\frac{1}{c}\frac{\partial}{\partial t_1 \partial z_2}\left\{\frac{1}{r}\delta\left(t_2 - t_1 - \frac{r}{c}\right)\right\}.
\end{aligned}\right\} \tag{641a}$$

Since Bohr and Rosenfeld considered the averages of the field quantities (denoted by bars) over a space-time region having the volume V and the time duration T, i.e., $\bar{\mathscr{E}}_x^{(I)}$, etc., only the averaged (and, therefore, regular) relativistic δ-functions, $\bar{A}_{xx}^{(I,II)}\left(= \frac{1}{V_I V_{II} T_I T_{II}} \cdot \int dt_1\, dt_2 \int_{v_I} dv_1 \int_{v_{II}} dv_2\, A_{xx}^{(12)}\right)$, etc., entered into their quantum-electrodynamical commutation relations, which they wrote explicitly,

$$\left.\begin{aligned}
\Delta\bar{\mathscr{E}}_x^{(I)} \Delta\bar{\mathscr{E}}_x^{(II)} &\sim \frac{h}{2\pi}|\bar{A}_{xx}^{(I,II)} - \bar{A}_{xx}^{(II,I)}|,\\
\Delta\bar{\mathscr{E}}_x^{(I)} \Delta\bar{\mathscr{E}}_y^{(II)} &\sim \frac{h}{2\pi}|\bar{A}_{xy}^{(I,II)} - \bar{A}_{xy}^{(II,I)}|,\\
\Delta\bar{\mathscr{E}}_x^{(I)} \Delta\overline{\mathscr{H}}_x^{(II)} &\sim 0,\\
\Delta\bar{\mathscr{E}}_x^{(I)} \Delta\overline{\mathscr{H}}_y^{(II)} &\sim \frac{h}{2\pi}|\bar{B}_{xy}^{(I,II)} - \bar{B}_{xy}^{(II,I)}|.
\end{aligned}\right\} \tag{642}$$

From the relations (642), they derived immediately 'that the averages of all field components over the same space-time region commute, and therefore can be measured independently of each other,' and further 'that the averages of two different-types of components, like $\mathscr{E}_x$ and $\mathscr{H}_y$, over arbitrary time intervals commute if the respective space regions coincide' (Bohr and Rosenfeld, 1933, p. 12). The different result, concluded in the latter cases by Heisenberg earlier, depended on his peculiar limiting procedure: He first took equal times $t_1 = t_2$, and then equal space regions, which actually led to an ambiguous result. Such an ambiguity could be avoided if one took, as Bohr and Rosenfeld insisted upon, extended test bodies.

For spatial dimensions, i.e., $L > cT$, the above results corresponded to those of the classical theory; for $L \leq ct$, peculiar fluctuations arose in quantum field theory, 'which are most intimately connected with the impossibility to visualize the light-quantum picture characterizing quantum field theory in terms of classical concepts' (Bohr and Rosenfeld, *loc. cit.*, p. 15). Bohr and Rosenfeld calculated these fluctuations explicitly and found that for field averages surpassing a critical value $\mathscr{S}$ (which was the square root of the vacuum fluctuations), the fluctuations might be neglected. From the commutation relations (642), there resulted also another critical value $\mathscr{U}$ (being about the square root of the right-hand side of Eqs. (642) for regions shifted by distances L and T); for field strengths larger than $\mathscr{U}$, all quantum-theoretical features would disappear. These critical values were given, respectively, by

$$\mathscr{U} \sim \mathscr{S} \sim \sqrt{\frac{h}{2\pi} c/(L \cdot cT)} \quad \text{for } L < cT \tag{643a}$$

and

$$\mathscr{U} \sim \sqrt{\frac{h/2\pi}{L^3 T} c} \quad \text{and} \quad \mathscr{S} \sim \sqrt{\frac{(h/2\pi) \cdot c}{L^2}} \quad \text{for } L > cT. \tag{643b}$$

Hence, in the latter case, for $L \gg cT$, where $\mathscr{U}$ becomes much larger than $\mathscr{S}$, no field fluctuations occur in the formalism.

With these background preparations, Bohr and Rosenfeld turned to their main problem, the physical measurement of the field quantities, which is based on the process of transporting momentum onto electrical and magnetic test bodies brought into the fields. Thus, for instance, to determine $\mathscr{E}_x$ by a test body of volume $V(= L^3)$, having a homogeneous electric density ρ, they used the relation

$$p_x'' - p_x' = \rho \bar{\mathscr{E}}_x V T, \tag{644}$$

if p_x' and p_x'' denoted the momentum of the test body at initial and final times, t' and t'', respectively ($t'' - t' = T$). Upon inserting the fundamental indeterminacy relation ($p_x \Delta x \sim h/2\pi$), they obtained for the uncertainty of $\mathscr{E}_x$,

$$\Delta\bar{\mathscr{E}}_x \sim \frac{h/2\pi}{\rho\Delta x \cdot V \cdot T}, \tag{645}$$

which could be made arbitrarily small by choosing the electric density ρ large enough. By selecting the particularly suitable situation $L > ct$, $\Delta\bar{\mathscr{E}}_x$ could be written as

$$\Delta\bar{\mathscr{E}}_x \sim \lambda Q, \quad \text{with } Q = \sqrt{\frac{h/2\pi}{VT}}, \tag{645$'$}$$

where λ denoted a small dimensionless factor (namely, $\frac{1}{\rho\Delta x}\sqrt{\frac{h/2\pi}{VT}}$).[818] Now, by taking into account the acceleration of the test body, the measured field received a slight change; indeed, an elementary charge as a test body would then give rise to a minimum uncertainty of the electric field strength $\mathscr{E}_x$; i.e.,

$$\Delta_m \mathscr{E}_x \sim \frac{\sqrt{(h/2\pi)c}}{c^2 T \Delta t}. \tag{646}$$

Upon this result, Bohr and Rosenfeld commented as follows:

> If one further, like Landau and Peierls, does not distinguish between T and Δt, this expression agrees with the absolute limit of measurability of a field component, on which they based their criticism of the foundations of the quantum-electrodynamical formalism. (Bohr and Rosenfeld, *loc. cit.*, pp. 24–25)

However, in the case of an extended test charge—as considered by Bohr and Rosenfeld, in contrast to Landau and Peierls and Heisenberg before—the retardation effect became much smaller, namely of the order of λ^2. Hence, the authors concluded:

> For the discussion of the measurability of [electromagnetic] field quantities, it is of fundamental importance to assume that the test bodies used [behave] like a uniformly charged rigid body, whose momentum can be determined within any given, arbitrarily small time interval with an accuracy derived from [$\Delta p \Delta x \sim h/2\pi$], complementary to the accompanying, uncontrollable shift in position. (Bohr and Rosenfeld, *loc. cit.*, p. 27)

A detailed evaluation of the test body (if split into many parts) confirmed that conclusion.

[818] Evidently $Q = \mathscr{U}$, due to Eq. (643b). For $\Delta x \ll L$, the small factor λ means that the test body carries a large number N of elementary charges ε, namely $N = \rho V/\varepsilon = \frac{1}{\lambda}\frac{L}{cT}\sqrt{\frac{h}{2\pi}c}/\varepsilon^2$.

Before proceeding to their final goal—i.e., to calculate the accuracy of field measurements—Bohr and Rosenfeld evaluated the effect of the fields on the test bodies: They found the classical result, with a fluctuation determined by $\mathscr{S}$, Eq. (643b); hence, it decreased quickly with increasing size L of the region of measurement. Now, every field component observed, say, $\mathscr{E}_x$, constituted a superposition of the corresponding field components arising from all sources (including the test bodies); hence, Eq. (644) had to be written explicitly as

$$p_x^{(I)''} - p_x^{(I)'} = \rho_{\mathrm{I}} V_{\mathrm{I}} T_{\mathrm{I}} (\bar{\mathscr{E}}_x^{(I)} + \bar{E}_x^{(I,I)}), \tag{644'}$$

where $\bar{\mathscr{E}}_x^{(I)}$ denoted the average value of $\mathscr{E}_x$ in the observed region I if no momentum measurement would be made at time t on the test body, and $\bar{E}_x^{(I,I)}$ the contribution of the latter obtained from the measurement. Thus, a minimum value followed for the uncertainty $\bar{\mathscr{E}}_x^{(I)}$, given by the relation

$$\Delta m \bar{\mathscr{E}}_x^{(I)} \sim \sqrt{h/2\pi |\bar{A}_{xx}^{(I,I)}|}\,, \tag{647}$$

which for $L_{\mathrm{I}} > cT_{\mathrm{I}}$ became identical with the critical quantity Q_I. This limit could be reduced still further by an additional mechanism (involving a spring), even to zero, apart from the inevitable field fluctuations. Hence, the accuracy of a single field measurement in quantum electrodynamics was 'restricted only by the limit of the classical description of the test body's field action' (Bohr and Rosenfeld, *loc. cit.*, p. 46), a result which appeared to be justified by the fact 'that one must deal in all measurements of physical quantities, by definition, with the application of classical conceptions, and that for field measurement any reference to a limitation of the strict applicability of classical electrodynamics would contradict the concept of measurement itself' (Bohr and Rosenfeld, *loc. cit.*, p. 47). On the other hand, this conclusion must be compensated, Bohr and Rosenfeld continued, in the complementary view, namely by 'the fact that the knowledge of the light-quantum composition of the fields [i.e., the quantum-mechanical constitution] gets lost by the field actions of the test bodies, ... the more the greater is the accuracy demanded from the measurement' (Bohr and Rosenfeld, *loc. cit.*, p. 48). That complementary feature of the theory also ensured 'that every attempt to restore the knowledge of the light-quantum composition of the field by a later measurement with any suitable apparatus would simultaneously prevent any further use of the field measurement in question' (Bohr and Rosenfeld, *loc. cit.*).

The measurement of two average values of a given field component could be carried out just as well along these lines, yielding eventually the result,

$$\Delta \bar{\mathscr{E}}_x^{(I)} \cdot \Delta \bar{\mathscr{E}}_x^{(II)} \sim \frac{h}{2\pi} |\bar{A}_{xx}^{(I,II)} - \bar{A}_{xx}^{(II,I)}| \tag{648}$$

in agreement with the first Eq. (642). Herewith, one had to consider a special feature of the relativistic field theory, notably: 'When measuring two field averages, one can only speak about a sequence of measurements if the corresponding time intervals T_1 and T_2 are completely separated.' (Bohr and Rosenfeld, *loc. cit.*, pp. 57–58) Finally, in the case of measuring two average values of different field components, Bohr and Rosenfeld calculated the indeterminacy relation

$$\Delta\bar{\mathscr{E}}_x^{(I)} \cdot \Delta\mathscr{R}_y^{(II)} \sim \frac{h}{2\pi}|\bar{C}_{xy}^{(I,II)} - \bar{C}_{xy}^{(II,I)}|\,, \tag{649}$$

where $\mathscr{R}_y^{(II)} = \bar{\mathscr{E}}_y^{(II)}$ or $\overline{\mathscr{H}}_y^{(II)}$ and $\bar{C}_{xy}^{(I,II)} = \bar{A}_{xy}^{(I,II)}$ or $\bar{B}_{xy}^{(I,II)}$, respectively. 'We therefore arrive at the conclusion mentioned already in the beginning that the quantum theory of fields represents, as far as the problem of measurability is concerned, an idealization which is free from contradictions insofar as we can forget about all restrictions created by the atomistic structure of field sources and of the measurement apparatus,' Bohr and Rosenfeld finished their long memoir (Bohr and Rosenfeld, *loc. cit.*, p. 64), for whose extensive details they excused themselves on account of the complicated character of the mathematical formalism of quantum electrodynamics which required, in addition the use of certain features not known in the nonrelativistic measurement problem.[819] Léon Rosenfeld, with whom Bohr had worked out the field-theoretical measurement problems, would become one of his favourite helpers and a long-term associate in Copenhagen.[820]

(d) The Continuation of the Debate on Causality with the Berlin Physicists (1929–1935)

In the early discussions of the causality problem immediately following Heisenberg's derivation of the uncertainty relations, we have thus far missed certain voices that one would have expected to hear from the conservative side, notably,

[819] Bohr summarized this work in the general discussion at the seventh Solvay Conference on Physics in Brussels (in *Institut International de Physique Solvay*, ed, 1934). He also returned to the problem in an unpublished manuscript, entitled 'Field and Charge Measurements in Quantum Theory' of 1937 (reproduced in Bohr, 1996, pp. 195–209), and after many further years he wrote a final paper on the topic, again with Rosenfeld, which was published in the *Physical Review* after World War II (Bohr and Rosenfeld, 1950).

[820] Léon Rosenfeld was born on 14 August 1904 at Charleroi, Belgium, and studied physics and mathematics at the University of Liège, obtaining his doctorate in 1926. He then went to the *École Normale Supérieure* and *Collège de France* (to work with Louis de Broglie), and in spring 1927 to Brussels (to work with Théophile de Donder), before he joined Max Born in Göttingen as an assistant (1927–1929). During 1929–1930, Rosenfeld worked with Pauli in Zurich, and from 1930 to 1940 he occupied positions at the University of Liège (1930–1935 as Reader, 1935–1940 as Professor), spending simultaneously longer periods at Copenhagen, assisting Bohr. From 1940 to 1947, he held a professorship in Utrecht, and from 1947 to 1958 one at the University of Manchester; then he moved to Copenhagen as professor at the newly established Nordic Institute for Theoretical Physics (NORDITA). He died on 23 March 1974 at Copenhagen.

Rosenfeld worked especially on nuclear physics and quantum field theory, principally quantum electrodynamics, and in the 1940s he became an expert on the problem of nuclear forces (on which topic he published a book in 1948). He also investigated basic problems of statistical mechanics and quantum theory, but was always attracted to work on epistemological questions; thus, in later years, he was considered one of the principal advocates and defenders of the 'true' Copenhagen interpretation of quantum mechanics and a great admirer of Niels Bohr.

those of Einstein, Planck, and Schrödinger. Of course, Einstein, between the fifth and sixth Solvay Conferences in 1927 and 1930, respectively, had tried to undermine the very cornerstone of the acausal interpretation of quantum mechanics, namely, Heisenberg's uncertainty relations, by considering clever *Gedanken*-experiments in the atomic domain; this was his contribution to the discussion.[821] Since all his efforts had failed in this direction, he would retire for some years, especially from the public debate, and rather work very eagerly on what he considered to be the big question in physics: the extension of general relativity to obtain a unified field theory of matter which would even incorporate such features as revealed by Dirac's electron theory.[822] Still, there existed a further reason for Einstein's temporary absence from the causality debate: In the years after 1928, he spent much time away from home, especially in the United States, where he finally established a new home ready to receive him after the political change in Germany drove him away from Europe. However, Berlin did not only have Einstein as a representative of the anti-Copenhagen view, but Planck and Schrödinger also belonged to the same group of critics, and they expressed themselves several times in the 1930's, though expounding different reasons individually.

On 4 July 1929, Erwin Schrödinger—who had been appointed as Max Planck's successor in the chair of theoretical physics at the University of Berlin in fall 1927—delivered his inaugural lecture as a member of the Prussian Academy of Sciences. After sketching the scientific development within the Viennese scientific community (due to Ludwig Boltzmann, Franz Exner, and Fritz Hasenöhrl) and indicating his own field of interest in theoretical physics, he turned to 'the most burning questions' of the theory in those days, namely, 'whether along with classical mechanics its method had to be given up as well, i.e., the fundamental theorem (*Grundsatz*) that definite laws together with accidental initial conditions determined the natural processes in each single case: it is the question of the usefulness (*Zweckmäßigkeit*) of the infallible postulate of causality' (Schrödinger, 1929d, p. CI). Schrödinger recalled how he had learned already in Vienna (through Exner and Hasenöhrl) that the strictly deterministic view of nature might not be upheld because of the practical impossibility to fix the state of a body consisting of millions of atoms, and he pointed out that the recent development of quantum theory seemed to demand even more, namely, the abandonment altogether of the possibility of determining the initial state of an atomic system. However, he continued:

> I do not believe that [the causality problem] will ever be answered in this way. In my opinion, this question does not decide about the real property of nature (*wirkliche Beschaffenheit der Natur*) as we are confronted with, but about the suitability and

[821] For details, see Section II.6.

[822] He worked on this topic especially with the Austrian mathematician Walther Mayer, focusing on a five-dimensional theory of what they called 'semi-vectors' (Einstein and Mayer, 1931; 1932a, 1932b). These efforts, had they succeeded, would have opened vistas beyond the limitations of the existing quantum mechanics (removing also, in particular, the unwanted statistical foundation).

convenience of one or the other view in our thinking about nature. Henri Poincaré has stated that we may be allowed to apply to real space the Euclidean as well as non-Euclidean geometry without fearing to be contradicted by facts. The physical laws which we discover, however, are functions of the geometry applied, and it may happen that one geometry leads to complicated and the other to simpler physical laws. Then one geometry turns out to be convenient, the other inconvenient, and the words "right" or "wrong" should not be used. The situation may be similar with the postulate of strict causality. There may hardly be [any] imaginable facts of experience which will finally decide whether a process of nature is absolutely determined or partially determined in reality, but at the most they will decide whether one or the other view allows a simpler survey of the facts observed. Even to reach this decision a long time will pass. Because also with respect to the geometry of the world we have become less sure, since we grasped with Poincaré our freedom of choice. (Schrödinger, *loc. cit.*, pp. CI–CII)

Schrödinger had expressed a similar view already several years earlier in a letter to Hans Reichenbach, dated 25 January 1924 (but published only in 1932). After calling the causality conclusions 'nothing but a tautology,' he had added:

However, it perhaps still appears that our idea of causality has something to do with *realism*. Just because we consider our surrounding as something real which persists for a certain while, we can go as far as giving this reality the *property* of being causally connected. Of course, behind this concept of a "relatively continuous reality (*relativ beständigen Realen*)" is hidden only what has been asked originally: why can past experience state something about future experience? Namely, [we say] now: just because of this the organizing property of reality, which has to be imagined as being eternally durable. (Schrödinger, 1932a, p. 66)

Then, he further emphasized that he did not, 'in fact, believe this organizational property [of reality],' as was evident already from his inaugural lecture at the University of Zurich in 1922 (Schrödinger, 1929a).[823] Evidently, also in 1929, Schrödinger had not moved away much farther from his earlier, uncommitted point of view with respect to causality, as was felt clearly by Max Planck, who responded to Schrödinger as follows:

I cannot resist the temptation to express here some words in favor of strictly causal physics, even with the danger of appearing to you to be a narrow-minded reactionary.... The question whether the lawful connections (*Gesetzmäßigkeiten*) which we encounter in nature all possess basically only an accidental character, i.e., are of a statistical type, can also be formulated thus: should we search for an explanation of the actually ever present uncertainty and accuracy, connected with every single observation, always only in the peculiar properties of the case under investigation, say, in the complex structure of the observed object or the incompleteness of the measuring apparatus including our senses; or should we trace back the uncertainty still further back into the formulation of the fundamental laws of physics? (Planck, 1929b, p. C II)

[823] Indeed, Schrödinger enclosed in the letter to Reichenbach of 1924 a copy of the earlier Zurich lecture, which was eventually published only in 1929.

Certainly, Planck admitted, the problem constituted to some extent one of the usefulness (*Zweckmäßigkeit*); but he also emphasized that 'the scheme [of physical theories], in any case, needs a solid basis, ... and if the postulate of strict causality fails to serve anymore such a basis, then the question arises about the basis of "acausal physics."' The older Planck did not consider the situation in quantum mechanics—namely, the fact that 'the conditions which determine a process causally cannot always be experimentally realized up to a, in principle unrestricted, degree of accuracy'—to present a new experience in the history of science. But science must be taken as a whole enterprise based on the causal law—e.g., 'in biology the real science starts only once the causal law has been introduced' (Planck, *loc. cit.*, p. C III)—and he (Planck) rather hoped that Schrödinger's own work on wave mechanics—'which has first demonstrated how the space-time processes in an atomic system can indeed be formulated as strictly [causally] determined' (*loc. cit.*, p. CIV) would make it possible to restore strict causality again in atomic theory.[824]

Both Planck and Schrödinger participated also in the causality debate of the early 1930's with their younger colleagues in Germany by developing and expounding partly on the viewpoints mentioned so far. Thus, Planck delivered on 17 June 1932, the Seventh Guthrie Lecture on 'The Concept of Causality' (Planck, 1932a); later, he elaborated on the topic in a brochure entitled '*Der Kausalbegriff in der Physik* (The Concept of Causality in Physics)' (Planck, 1932b). There Planck admitted that the strict causality entering into the world view of the classical theories (including the one describing Brownian motion) failed *vis a vis* quantum mechanics, in particular, Heisenberg's uncertainty relations, but he also claimed that a 'final refutation of the causal law ... rested on a confusion of the world view (*Weltbild*) with the world of senses (*Sinnenwelt*),' which he called a 'premature step' because:

> A different, more obvious way out of the difficulties exists, which has often served in similar situations rather well: it consists in the assumption that the question asking for simultaneous values of the coordinates and momenta of a material point makes no physical sense at all. The impossibility to answer a meaningless question, however, should not be held against the causal law *per se* but rather against the assumptions leading to ask the question, hence in the present about the assumed structure of the physical world view (*Weltbild*). Now, since the classical world view has failed, it must be replaced by another one. (Planck, 1932b, pp. 13–14)

The concept of matter waves, which described atomic particles by a wave packet, in Planck's opinion admitted—though it satisfied Heisenberg's relation—as considering the same determinism to be at work as in classical point mechanics. Of

[824] In a lecture on '*Zwanzig Jahre Arbeit am physikalischen Weltbild* (Twenty Years of Work on the Physical World View),' which Planck gave at Leyden on 18 February 1929, he had addressed the problem of causality in modern physics in some detail and argued that the wave-mechanical description provided a 'different determinism' from the one existing in classical physics: It now determined just the matter waves (Planck, 1929a, especially, p. 220).

course, now the conventional world of senses (*Sinnenwelt*) deviated from the world view (*Weltbild*) of the quantum physicist, about which Planck did not worry but preferred to insist upon ‘retaining determinism first of all in the world view (*Weltbild*)’ (Planck, *loc. cit.*, p. 15). Even the fact that the wave function did not yield the values of the coordinates as functions of time but only the probabilities that the coordinates possess at a given time ‘somehow given values’ would not disturb him (Planck). There still existed ‘the saving way out,’ namely, the assumption that the question about the meaning of a given symbol of the causal quantum-physical *Weltbild*, say, of a matter wave, makes ‘no definite sense as long as one does not simultaneously say in which state the peculiar measuring apparatus is used to translate the symbol into the *Sinnenwelt*’ (Planck, *loc. cit.*, p. 17). The latter argument raised then (by Bohr, Heisenberg and others) might be refuted perhaps by referring to ‘indirect test methods which have yielded good results in many cases, where the direct ones have failed’ (Planck, *loc. cit.*, pp. 16–17).

In a word, Planck—who initiated quantum theory in the first place—was not prepared to succumb to the central argument of the ‘indeterminists’ stating: Since the wave function in quantum physics is a probabilistic quantity, also strict causality must be necessarily abandoned; all that remains to understand is how strict laws, such as Coulomb’s law for electric forces, can arise. Planck rather expounded his credo as follows:

> The determinist thinks quite the opposite about all these points. He declares the Coulomb law to have the satisfactory character of a completely final law: on the other hand, he recognizes the wave function as a probabilistic quantity only as long as one can forget about the measuring apparatus by which the wave is analyzed; and he searches for strict theoretical relations between the properties of the wave function and the processes in the measuring apparatus. To achieve this purpose, he must first turn the measuring apparatus, like the wave function, into an object of research: he must not only translate the total experimental setup creating matter waves—say, the high-voltage battery, the heated wire, the radioactive probe—but also the registering apparatus—say, the photographic plate, the ionization chamber or the Geiger counter—plus the processes occurring in them into his physical *Weltbild*, and must deal with all these objects together as a single object, as a closed unit. But the problem would not be finished even then, as it has rather become more complex, because: since the total object must neither be cut into parts nor be subject to external actions for otherwise it would lose its characteristics, hence a direct test cannot be made at all. However, now it would be possible to establish new hypotheses concerning the internal processes [within the total object] and then to test their consequences. (Planck, *loc. cit.*, p. 20)

After all of these complications, Planck frankly admitted that ‘only future will tell us’ whether one might really be able to proceed successfully on the path indicated (Planck, *loc. cit.*, pp. 20–21). But with respect to the causality problem, Planck remained optimistic, provided one would assume the following interpretation:

> The causal law is neither right nor wrong; it is rather a heuristic principle, a path-indicator (*Wegweiser*)—in my opinion the most valuable indicator we have at

hand—for us to find our way in the colourful jumble of events and to indicate the direction in which physical research must go on to reach final results. Just as it occupies from the very beginning the awakening spirit of a child and puts into its mouth the never-fatiguing question "why?" it also guides the scholar throughout his life presenting to him unceasingly new problems. Indeed, science does not mean resting leisurely in the possession of cognition already obtained, but it means restless work and steadily progressive development. (Planck, *loc. cit.*. p. 26)

In presenting this 'deterministic' world view (*Weltbild*) of quantum theory, Max Planck certainly followed his previous line of arguments, especially the stand he had taken since 1908 against the philosophical attitude of Ernst Mach.[825] In Planck's opinion, physical theories should not be restricted to represent an economical connection of sensations or observational data, but had to follow ideal guidelines—in the first place, the causal law. To support this view, Planck referred to that form of modern atomic theory which he favoured, namely, Schrödinger's wave mechanics. The wave-mechanical scheme indeed seemed to provide the best chance of retaining the causal principle formulation, which was similar to that of the classical theories. The opponents of the causal interpretation of quantum mechanics, on the other hand, stuck (in Planck's view) too much to the ancient concept of a mass point. Max von Laue, Planck's former student (and later, his colleague and friend in Berlin), agreed in this opinion when he published a note '*Zu den Erörterungen über Kausalität* (About the Discussions on Causality)' in the *Naturwissenschaften* (von Laue, 1932b). In it, he wrote:

The present forms of quantum mechanics attempt to rescue the life of the "mass point" [of the old Newtonian theory]. Then they immediately arrive, because of those wave motions [as found in wave mechanics], necessarily at the uncertainty relations; from the latter, they conclude further that physics must renounce the causal interpretation of the individual [atomic] process and restrict itself to state [only] statistical laws. We do not wish to reproach this procedure; *for the moment* it may represent the best way out. (von Laue, *loc. cit.*, p. 916)

However, von Laue continued, history may decide for a different method and eventually return to the older conceptions. 'Hence in the case of the quantum riddle it is possible that time is not yet ripe for a [definitive] solution,' he claimed. In any case, he concluded: '*These difficulties cannot force anybody to change his epistemological point of view, whatever it may be*.' (von Laue, *loc. cit.*) That is, like Planck, von Laue favoured the causal point of view.

The third senior Berlin theoretician, Erwin Schrödinger, also pondered in those years about the consequences arising from quantum mechanics. Having studied in some detail the derivation of the uncertainty relations, especially for relativistic mechanics (Schrödinger, 1930), he declared in a popular talk on '*Indeterminismus*

[825] In a way, Planck's lecture at Leyden, referred to in footnote 824, constituted a modernized version of his previous talk at Leyden in 1908.

in der Physik (Indeterminism in Physics)' two years later that the (uncertainty) relations themselves contained an internal conceptual contradiction if applied to a mass point (Schrödinger, 1932b, first essay). Since a mass point in mechanics has to be defined by position, velocity and mass, he now argued, the statement that position and velocity cannot be determined simultaneously with arbitrary accuracy would dissolve the very concept. Evidently, he agreed with Planck and von Laue in hoping for a satisfactory solution of the quantum riddle by applying the purely wave-mechanical description.

As Planck noted, in the beginning of the 1930's, the majority of the quantum physicists believed in the violation of the causality principle, while only a small minority protested. Was this perhaps the matter of the generational difference, since even a scientist like Paul Ehrenfest, friendly to the young revolutionaries, became worried that he might not understand the *unanschauliche* (non-intuitive) trends taken by the later developments?[826] However, Planck, von Laue, and Schrödinger certainly did not adhere to old classical theories; they did not wish to renounce any of the achievements of the modern relativity and quantum theories, but only complained about the Copenhagen interpretation of quantum mechanics and proposed to retain more '*Objektivierkeit* (objectifiability)' in the sense accepted since centuries by scientists in many different fields. Bohr and Heisenberg, the spokesmen of the Copenhagen *Weltbild*, saw the situation quite differently and they criticized the Berlin 'conservatives.' Especially, Heisenberg argued that the causal principle did not belong to the old traditions of science: The physicists had accepted it only since about 150 years as an 'important consequence of the postulate of *Objektivierbarkeit* of the observed facts,' he said in a lecture on '*Atomtheorie und Naturerkenntnis* (Atomic Theory and Understanding of Nature)' presented on 22 November 1933, at Munich (Heisenberg, 1934b). Immanuel Kant had initially expressed this consequence in his *Kritik der reinen Vernunft* (*Critique of Pure Reason*) of 1781, and strict determinism had sneaked into the classical theories since the early 19th century; the development of quantum mechanics and its interpretation in the mid-1930s had then shown 'that the requirements of perception to be *objectivierbar* (objectifiable) and of connections being describable by mathematical equations do not depend on each other,' but:

> Rather the requirement of clarity—and more is not attempted by the application of mathematics—can be retained absolutely, even in a field of science, in which *Objektivierbarkeit* (objectifiability) of perceptions ceases to be possible. (Heisenberg, *loc. cit.*, p. 13)

In his talk, Heisenberg stated a little later: 'For the indivisible constituents of matter, i.e., for the lightest bodies, every irradiation, or every act of observation at all, constitutes a remarkable perturbation (*Eingriff*) which changes the behaviour

[826] See Paul Ehrenfest's '*Erkundigungsfragen* (scientific queries) (1932),' which we shall discuss in the next section.

of the observed body decisively.' (Heisenberg, *loc. cit.*, p. 14). These and similar arguments were reproached by Max von Laue in a further note, '*Über Heisenbergs Ungenauigkeitsbeziehungen und ihre erkenntnistheoretische Bedeutung* (On Heisenberg's Uncertainty Relations and Their Epistemological Meaning,' published in June of the following year (von Laue, 1934). He wrote:

> It seems to me altogether doubtful to derive from the present status of physical knowledge too far-reaching conclusions concerning the theory of cognition. Quite apart from the fundamental doubt to abandon the principle that nature can be experienced (*Prinzip der Erforschbarkeit der Natur*), because one is not able to apply it so far completely, one must at least start from a foundation which is logically firm and does not contain contradictions. This cannot be said of the present physics. (von Laue, *loc. cit.*, p. 440)

Here, von Laue pointed to the fact that the concept of smallest particles followed only from the most recent experiments if interpreted according to the old corpuscular view of matter, while wave mechanics and its relativistic extensions rather spoke of extended electrons and the like. Again, he repeated: '*The uncertainty relations limit in my opinion every corpuscular mechanics but not every physical cognition*.' (von Laue, *loc. cit.*, p. 441) Since he considered causality as the key to any physical cognition, von Laue hoped that 'the uncritical pessimism'—which seemed to him 'in spite of all the given physically spurious arguments (*Scheinargumente*), to be a result of that deep general cultural pessimism forming the fundamental tendency of our times'—might soon be overcome (von Laue, *loc. cit.*).[827]

'Cultural pessimism' or 'positivism'—these were the accusations directed against the Bohr-Heisenberg interpretation of quantum mechanics, though the originators did not really feel to be victims of such verdicts.[828] No, the successful Heisenberg of those days—who had recently explained the structure of atomic nuclei (see Section IV.3 below) and was about to deal with cosmic-ray phenomena

[827] In contrast to the other critics of the Copenhagen interpretation, von Laue did not worry about the '*Unanschaulichkeit*' of quantum phenomena, arguing (as Heisenberg also did): 'What one calls non-visualizable, depends on time. A theory which forces us to give up the usual conceptions to describe the external world, seems to the witnesses of its origin always necessarily *unanschaulich*, mostly even to the originators themselves.' (von Laue, 1934, pp. 440–441)

[828] Heisenberg was even less worried about an argument raised by the Viennese Karl Popper against the validity of the indeterminacy relations (Popper, 1934). Popper claimed that 'for 'non-prognostic' measurements, e.g., to determine the momentum of a particle when arriving at an exactly given space point,' the relations would not apply; he proposed to demonstrate this point by a *Gedanken*experiment involving the crossing of an electron-ray A and an X-ray B, with both rays representing 'pure cases' (i.e., a monochromatic parallel beam of electrons interacting with a monochromatic spherical X-ray). Heisenberg let his student Carl Friedrich von Weizsäcker analyze the experiment and demonstrate that nothing was wrong with the relations. Von Weizsäcker rather concluded:

> The uncertainty relations cannot be applied to "non-prognostic measurements" because of the only reason: the theorems stating their results do not contain statements about physically possible measurements; on the other hand, conclusions about the past obey the same accuracy as those about the future, due to the symmetry of quantum-mechanical laws with respect to the time direction. (von Weizsäcker in Popper, 1934, p. 808)

(see Section IV.5)—could hardly be accused of being influenced by any feeling of cultural pessimism.

Moreover, Heisenberg's *Weltbild* also did not follow any philosophical doctrine, such as positivism, as we have mentioned earlier in this section. He rather developed his own epistemological conclusions from the quantum-mechanical revolution, which he embedded into the grand historical schemes of physical descriptions in the talk entitled '*Wandlungen der Grundlagen der exakten Naturwissenschaft in jüngster Zeit* (Recent Changes in the Foundations of Exact Science)' and delivered on 17 September 1934, at the Hanover *Naturforscherversammlung* (Heisenberg, 1934f). Heisenberg spoke in this programmatic lecture about the alterations in the physical concepts achieved by the modern relativity and quantum theories, which showed the limitations of the previous theories, and then stated:

> Modern physics has rather purged classical physics from some obscurities connected with the assumption of their unlimited applicability and shown that the single parts of our science—mechanics, electricity, quantum theory—constitute schemes, closed in themselves and being rationally penetrable to their limits, which probably represent the corresponding laws of nature for all future times. (Heisenberg, *loc. cit.*, p. 701)

Such 'closed systems' then do not contradict but rather complement each other, as Heisenberg explained in more detail in the talk on '*Prinzipiellen Fragen der modernen Physik* (The Fundamental Questions of Modern Physics),' given on 27 November 1935, at the University of Vienna (where Moritz Schlick taught). Classical physics, he said there, is built 'on a system of sharply formulated axioms whose physical content is determined by the fact that through the choice of words appearing in the axioms their application to nature is uniquely prescribed,' he began his remarks (Heisenberg, 1936a, p. 91). That means, classical physics rested on the range of its concepts, like mass, velocity, and force. The modern theories, first relativity and then quantum mechanics, had restricted the range of the systems of classical concepts. The difficulty in understanding the results of modern theories arose from the necessity to leave 'the domain of the daily human experience,' while one had simultaneously to continue using the concepts of those classical theories which can be regarded as the limiting cases of the modern theories. That is, 'the classical concepts remain still an indispensable part of the scientific language, without which one cannot speak at all about scientific results,' Heisenberg concluded the introductory part of his lecture. (Heisenberg, *loc. cit.*, p. 95)

The necessity to go beyond the classical theories had grown out of the experimental observations of new phenomena; e.g., the new experience that 'no signals can be transmitted with velocities faster than light,' led to new systems of axioms and concepts which allowed one to formulate new laws describing new experiences. For the physicist, 'even the mathematically formulated statements of physics are so-to-speak only "pictures in words (*Wortgemälde*)" through which we try to interpret our experiences about nature for us and other people in a definite and understandable way' (Heisenberg, *loc. cit.*, pp. 97–98), but one always had to transcend the conventional concepts in essential aspects, for example:

> Thus relativity theory and classical theory constitute the first decisive steps from the region of visualizable concepts into a more abstract land, and the character of the here discovered connections leaves no doubt that these steps can never be taken back.... Actually, the discovery of a new system of concepts means nothing else but the discovery of a new way of thinking which as such can never be taken back. (Heisenberg, *loc. cit.*, p. 98)

That is, the hope expressed by some people that one might return finally to the classical concepts must be given up. Especially, unless the results of quantum mechanics were proven to be wrong, the statistical character of the theory would remain *final*. Further, in treating arbitrary experiments in quantum mechanics, Heisenberg continued, a 'cut (*Schnitt*)' must be introduced between the measuring apparatus and the physical system observed; while this cut can be chosen largely at an arbitrary point, it is responsible for the statistical behaviour of the quantum-mechanical laws. That is, any possible deterministic reformulation of quantum mechanics would have first to remove the cut, which appears to be quite an impossible task; hence, any revision of the present atomic theory must move away further from the classical theory. Perhaps, the 'hole theory' of Paul Dirac might open the way to understand the properties of electrons and even the strength of the electromagnetic coupling constant, Heisenberg argued at the end of his paper, and further continued:

> Quite generally, one may say in conclusion: the assumption that even the concepts of modern physics will have to be revised should not be taken as skepticism [or even "cultural pessimism"]; quite the contrary, it is just another expression for the conviction that the extension of our range of experience will bring to light new harmonies of nature. (Heisenberg, *loc. cit.*, p. 102)

Returning to the topic of causality discussed in this section, we should finally mention the attempt of a young student of philosophy, Grete Hermann from Østrupgaard (Denmark), who discussed in her doctoral dissertation of 1935 the 'natural-philosophical foundations of quantum mechanics' (Hermann, 1935a, b). The contents of her work, which she carried out in Leipzig and Copenhagen (staying in close contact with Heisenberg and Bohr), may be derived from a review written by Carl Friedrich von Weizsäcker:

> The present memoir is perhaps the first work from the philosophical side, which provides a positive and incontestable contribution to derive the epistemological consequences of quantum mechanics. She [Grete Hermann] achieves her goal by pursuing a single problem to its depth. [On the one hand,] quantum mechanics claims the impossibility of [arriving at] certain results. On the other hand, because our experiences are not closed, it is always possible to search for the causes of an observed phenomenon as long as they are not yet known. Hence, does not quantum mechanics, when stating the *impossibility* of a causal description of nature determining all events,

> exceed its competence? The author [Grete Hermann] provides an answer, which on first inspection sounds paradoxical but hits the point exactly: finding real, still unknown causes is impossible because quantum mechanics already provides the causes for a given event in any case completely. The impossibility of [making] certain predictions is not based on the fact that a causal chain investigated turns out to be interrupted somewhere, but rather on the fact that the different causal chains cannot be organized to form a unified picture embracing all aspects of the process, thus it rather remains to the whim (*Willkür*) of the observer which of the different "virtual causal chains" has been realized. (von Weizsäcker, 1936c, p. 527)

The physicists might not be tempted to embed their results too strictly into any philosophical school—as Grete Hermann did by appealing strongly to the traditions of Immanuel Kant, Herbert Fries and Leonard Nelson—von Weizsäcker noted, and concluded that 'a fruitful discussion on the topic could not be opened, at any rate, in a clearer and more pertinent manner.' (von Weizsäcker, *loc. cit.*, p. 528)

To complete the story in the words of Grete Hermann herself, a few sentences from the summary of her work might be quoted. In particular, she wrote:

> The difficulties, in which the partisans of causality are placed by the discoveries of quantum mechanics, seem in proper light not to arise from the causality principle itself. They rather emerge from the tacit assumption connected with it that the physical cognition grasps natural phenomena adequately and independently of the observational connection (*Beobachtungszusammenhang*). This assumption is expressed in the prerequisite that every causal connection between processes yields a calculable action due to the cause, even more, that the causal connection is identical with the possibility of such a calculation.
>
> Quantum mechanics forces us to dissolve this mixing of different principles of natural philosophy, to drop the assumption of the absolute character of the cognition of nature, and to use the causal principle independently of the latter. By no means has it disproved the causal law, but it has clarified its status and freed it from other principles which must not be combined with it necessarily. (Hermann, 1935b, p. 721)

When Grete Hermann wrote her dissertation, the debate among the quantum physicists on causality and the prerequisites for cognition of nature had reached a new climax in Albert Einstein's new attack on the question of the completeness of quantum mechanics.

IV.2 The Debate on the Completeness of Quantum Mechanics and Its Description of Reality (1931–1936)

(a) Introduction

Expressed in whichever formulation, quantum mechanics offered even to experienced experts puzzling features to ponder about. Thus, Paul Ehrenfest, since 1906 an active contributor to the theory of quanta, wrote in summer 1932 '*Einige* *die Quantenmechanik betreffende Erkundigungsfragen* (Certain Queries Concerning Quantum Mechanics)' and submitted them to *Zeitschrift für Physik* (Ehrenfest, 1932). In particular, he listed the following queries: A. The (role of the) imaginary unit in the Schrödinger equation and the Heisenberg–Born commutation relations. B. The limitation of the analogy between photons and electrons. C. The convenient accessibility of the spinor calculus. He concentrated there on what he thought might be called by most quantum physicists as being 'senseless questions (*sinnlose Fragen*)'; e.g., why did Schrödinger, in formulating wave mechanics, start from a real wave function but soon introduce the complex notation, for it seemed to be more convenient, and never returned later to the real formulation; or, how to express the analogy between photons and electrons in a differential equation formulation, and not in the formulation of a non-local integral equation (as suggested by Lev Landau and Rudolf Peierls, 1930)?

Wolfgang Pauli soon replied to these 'senseless questions' of his senior friend in some detail, first by letters exchanged from October to December 1932 (Pauli, 1985), and then openly in a paper published also in *Zeitschrift für Physik* (Pauli, 1933d). Concerning query A, he pointed out that it was the assumption of a positive normalized probability which demanded the imaginary unit, especially: 'The imaginary unit enters into the search for an expression for the probability density W, which satisfies the requirements and does not contain the time derivatives of [the wave function] ψ.' (Pauli, *loc. cit.*, p. 576) This probability density then depended quadratically on the wave function $\psi_\rho(\boldsymbol{x}, t_0)$ at a given instant of time t_0 and could be expressed both in nonrelativistic and relativistic cases only with complex wave functions. With respect to query B, the photon–electron analogy, Pauli proposed to distinguish between 'large fields (*große Felder*) Ψ_ρ and $\mathbf{E}$, $\mathbf{H}$' describing many electrons and photons, on the one hand, and 'small fields (*kleine Felder*)' ψ_ρ and $\boldsymbol{e}$, $\boldsymbol{h}$ describing single photons and electrons, on the other hand. In the latter case, the photon would not possess a four-current satisfying a continuity equation and having positive-definite density; hence, the electromagnetic fields $\boldsymbol{e}$, $\boldsymbol{h}$ of a photon could not be associated with a local space-time density $W(\boldsymbol{x}, t)$ for a particle. Moreover, in the photon situation, particles with positive energies could always be kept in the processes of interaction, while in the electron situation negative-energy particles might result. The large-field case also revealed differences: When many photons were present, the $\mathbf{E}$, $\mathbf{H}$ constituted classically measurable fields (though the number of quanta N did not commute with $\mathbf{E}$ and $\mathbf{H}$); however, the Ψ_ρ field could not be measured like a classical field.

Ehrenfest had mentioned another problem of the quantum theory that bothered him: 'If we recall what an *uncanny theory of action-at-a-distance* is represented by Schrödinger's wave mechanics, we shall preserve a healthy nostalgia for a *four-dimensional theory of action by contact*.' (Ehrenfest, 1932, p. 557, footnote 1) To that, Pauli replied in detail in §3 of his paper. He noted that already in classical electrodynamics action-at-a-distance forces formally occurred, but the situation could be easily reformulated in the action-by-contact language when introducing the differential equation (div $\mathbf{E} = 4\pi\rho$) which the electrostatic field obeyed. Similarly, he argued, the Coulomb force in the Schrödinger equation (mentioned by Ehrenfest) might be replaced by an action-by-contact (Pauli, 1933a, pp. 584–586).

For several years, Ehrenfest had been bothered by his lack of understanding as he reflected about the decisive features of the modern development.[829] Somehow, he still felt attracted by the nostalgic arguments of his friend Albert Einstein. Already at the fifth Solvay Conference in Brussels in fall 1927, Einstein had criticized the point of view that 'quantum mechanics is considered to be a complete theory of individual [atomic] processes,' and stated: If a particle, somehow described by the absolute square of the Schrödinger function $|\psi|^2$, 'is localized, a peculiar action-at-a-distance must be assumed to occur which prevents the continuously distributed wave in space from producing an effect at *two* places on a screen' (Einstein, 1928, p. 255). In the early 1930's, Einstein continued to worry about this particular problem, as Ehrenfest (with whom he conferred in those times quite regularly) in his paper on the *Erkundigungsfragen* (queries) mentioned that 'certain thought experiments, designed by Einstein but never published, are particularly suited for [clarifying] that purpose' (Ehrenfest, 1932, p. 557). The answers given by Pauli to Ehrenfest did not satisfy Einstein (see the discussion in Jammer, 1974, pp. 117–119), and the *Gedanken*experiments recalled by Ehrenfest in 1932 would finally lead to the paper containing the famous 'Einstein-Podolsky-Rosen (*EPR*) paradox,' as Max Jammer concluded from an examination of Einstein's correspondence between 1927 and 1935 (partly supported by a letter which Einstein wrote to Paul Epstein later, on 10 November 1945). Jammer summarized Einstein's steps on the way to this decisive paper as follows:[830]

> The point of departure is Einstein's well-known photon-box experiment which he presented at the sixth Solvay Conference in October 1930 in Brussels in order to disprove the Heisenberg energy-time uncertainty relation.... Although defeated, Einstein continued to ponder about this argument and understood that in order to eliminate the unwanted gravitational effect only horizontal motion should be admitted. As described in his [later] letter to Epstein, he thus designed the following modification. He imagined an ideally reflecting box B which contains a clock operating a shutter V and a quantum of radiation of unknown frequency; the box is assumed to be movable in a horizontal direction along a frictionless rail which serves as a reference system K, but can also be rigidly connected with K. At one end of the rail an absorbing screen or reflecting mirror can be mounted. An observer sitting on top of the box B and in possession of all measuring devices releases the shutter at a precisely determinable moment to emit a photon in the direction of the screen. Thereupon the observer can *either* immediately connect B with K, read the position of B and predict the time of arrival of the photon at the screen *or* he can measure the

[829] Paul Ehrenfest occasionally mentioned to his friends and colleagues that he would have to vacate his university chair for another, more capable, person. It is difficult to say how much such feelings may have contributed to his suicide on 25 September 1933.

[830] Besides Max Jammer (1974, 1985), especially, Arthur Fine (1986, 1993) has worked on the historical reconstruction of the *EPR* paper.

momentum p of B relative to K by means of the Doppler effect with arbitrarily low frequency and the recoil formula $p = \frac{h\nu}{c}$ and predict the energy of the photon arriving at the screen. As stated in the letter, Einstein already at that time conceived the idea that the light-quantum, after leaving the box, represents a "real state of affairs (*einen realen Sachverhalt*)" which can hardly be thought to depend on what kind of measurement is being performed with B. Hence any property of the light-quantum, found by a measurement on B, must also exist if the measurement would not have been performed at all. The light-quantum must consequently possess a definite position as well as a definite colour, a situation not describable in terms of a wave function. Hence a description in terms of wave functions cannot be a complete description of the physical reality. It is clear that the scenario of this thought-experiment is the same as that of the Brussels photon-box experiment apart from being, so to say, rotated into the horizontal direction. But it intends to show not the inconsistency but rather the incompleteness of the theory. And to this end the additional feature of introducing the idea of a "real state of affairs" was imperative. It vaguely foreshadowed what became later known as the "Einstein separability principle." (Jammer, 1985, pp. 133–134)

Thus, after a preparation of several years, Albert Einstein—with Boris Podolsky and Nathan Rosen—finally sent a paper to the *Physical Review* (where it was received on 25 March 1935); it was entitled 'Can Quantum-Mechanical Description of Physical Reality be Considered Complete?' This *EPR*-paper concluded with a bold statement:

While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question whether or not such a description exists. We believe, however, that such a theory is possible. (*EPR*, 1935, p. 780)

The *EPR*-paper, which appeared in the *Physical Review* (issue of 15 May 1935), aroused an abundant response, first in America, then in Europe (especially from Niels Bohr in Copenhagen). It initiated an extended debate among the physicists on what 'physical reality' was all about. In the fall of 1935 the *EPR*-arguments were supported by Erwin Schrödinger, who in the context of a review on '*Die gegenwärtige Situation in der Quantenmechanik* (The Present Situation in Quantum Mechanics),' also developed his famous 'cat paradox' (Schrödinger, 1935a). The response of the Copenhagen representatives, especially, Niels Bohr and Werner Heisenberg—as well as certain philosophical supporters—mingled with the political situation in Germany. The debate on 'What is Real?' in physics and whether quantum mechanics is, or ever could be, able to provide a complete description of nature has been going on till the present day.[831]

[831] We shall later briefly indicate the development of this debate during the past several decades in the Epilogue.

(b) From Inconsistency to Incompleteness of Quantum Mechanics: The *EPR* Paradox (1931–1935)

In the early 1930's Albert Einstein, besides becoming deeply involved in a programme on the development of quantum field theory (Einstein and Mayer, 1931, 1932a, b), addressed the problem of the 'Knowledge of Past and Future in Quantum Mechanics' in a note written during his visit to California in the winter semester 1930–1931, together with Richard Chace Tolman (the dean of the graduate school of the California Institute of Technology) and the young Russian-born physicist Boris Podolsky.[832] In particular, they discussed 'a simple ideal experiment which showed that the possibility of describing the past path of a particle would lead to predictions as to the future behaviour of a second particle of a kind not allowed in quantum mechanics' (Einstein, Tolman, and Podolsky, 1931, p. 780). Contrary to some earlier suppositions, stating 'that the quantum mechanics would permit an exact prescription of the past path of a particle,' the authors obtained from their analysis 'an uncertainty in the description of past events which is analogous to the uncertainty in the prediction of future events.' (Einstein, Tolman, and Podolsky, *loc. cit.*)

The Einstein–Tolman–Podolsky (*ETP*) *Gedanken*experimental setup worked with a box *B* containing a number of identical particles in thermal agitation and provided with two small openings to be closed and opened by a shutter *S*, which releases for a short time particles in two directions: (i) directly toward an observer *O*, and (ii) after reflection at a wall at the point *R* to the observer on a second, larger path *SRO*. An energy measurement (by weighing the box *B*) and a time determination were to be carried out. Then, 'knowing the momentum of the particle in the past, and hence also its past velocity and energy, it would seem possible to calculate the [instant of] time when the shutter must have been open from the known time of arrival of the first particle [on the direct path *SO*], and to calculate the energy and momentum of the second particle [on the longer path *SRO*] from the known loss of the energy content of the box when the shutter opened' (*ETP*, *loc. cit.*, p. 781). This 'paradoxical result' of a prediction of exact energy and time of the arrival of the second particle could only be explained by 'the circumstance

[832] Boris Podolsky, born in Taganrog, Russia, on 29 June 1896, emigrated to the United States in 1913. After receiving a B.S. degree in electrical engineering from the University of Southern California (USC) in 1918, he served in the U. S. Army and then obtained employment in the Los Angeles Bureau of Power and Light. After further studies in mathematics at USC (M.S. in 1926) and physics at the California Institute of Technology, he received his doctorate at Caltech (under the supervision of Paul Sophus Epstein) in 1928. With a National Research Council Fellowship, he spent a year at the University of California at Berkeley, followed by a year in Leipzig as an International Education Board Fellow. In 1930, Podolsky returned to Caltech for a year and worked with Richard C. Tolman, and then spent two years at the Ukrainian Physico-Technical Institute at Kharkov, collaborating there with Vladimir Fock, Paul Dirac (who was on a visit to the U.S.S.R.), and Lev Landau. He returned to the Institute for Advanced Study in Princeton with a fellowship in 1933; from there, he moved to the University of Cincinnati in 1935 as a professor of mathematical physics, and in 1961, he changed to Xavier University in Cincinnati. He died on 28 November 1966, in Cincinnati.

that the past momentum of the first particle cannot be accurately determined as was assumed' (*ETP*, *loc. cit.*).[833] 'Finally, it is of special interest to emphasize the remarkable conclusion that the principles of quantum mechanics would actually impose limitations upon the localization in time of a macroscopic phenomenon such as the opening and closing of a shutter,' their letter stated (*ETP*, *loc. cit.*).[834]

The idea of using reflected particles entered into the next *Gedanken*experiment of Einstein, about which Paul Ehrenfest reported to Niels Bohr in a letter, dated 9 July 1931.[835] Ehrenfest wrote, in particular, that Einstein no longer intended to make use of the box experiment as an argument 'against the indeterminacy relations' but 'for a totally different purpose'; indeed, Einstein now constructed a 'machine' which ejects a projectile and considered the following situation: After the projectile had been ejected, an 'interrogator (*Frager*)' asks the 'machinist' to predict, by examining the 'machine' alone, *either* what value a quantity A *or* what value a conjugate quantity B would have if the projectile were subjected to the respective measurements after a long period of time (when the projectile returns after being reflected by a distant reflector). As Ehrenfest reported further, Einstein believed that a photon box might represent such a machine and proposed to carry out the following experiment:

1. Set the clock's pointer to time O hour and arrange that at the pointer position 1,000 hours [later] the shutter will be released for a short time interval.
2. Weigh the box during the first 500 hours and screw it firmly to the fundamental reference frame.
3. Wait for 1,500 hours to be sure that the quantum has left the box on its way to the fixed reflector (mirror), placed at the distance of 1/2 light-year away.
4. Now let the interrogator choose what prediction he wants: (α) *either* the exact time of arrival of the reflected quantum, *or* (β) the colour (energy) of it. In case (α), open the still firmly screwed box and compare the clock reading (which during the first 500 hours was affected, due to the gravitational red-shift formula) with the standard time and find out the correct standard time for the pointer position "1,000 hours"; then the exact time of arrival [of the photons] can be computed. In case (β), weigh the box again after 500 hours; then the exact energy can be determined. (Ehrenfest to Bohr, 9 July 1931; see Jammer, 1974, pp. 171–172)

'The interesting point is that the projectile, while flying around isolated on its own, must be able to satisfy totally different non-commutative predictions, without

[833] Einstein, Tolman, and Podolsky substantiated the above argument to be correct by referring to the measurement of the particle's momentum by a Doppler effect in reflected infrared light, which would lead to an uncertainty in the position of the first particle, and thus also in the exact opening-instant of the shutter.

[834] The *ETP*-paradox received some publicity, because a little later another visitor from Europe to USA, Charles Galton Darwin, concluded differently from a *Gedanken*experiment working with two shutters. In particular, he stated: 'The uncertainty principle is essentially only concerned with the future; we can install instruments which will tell us as much of the past as we like.' (Darwin, 1931, p. 653) See the discussion of this point in Jammer, 1974, p. 169.

[835] For a detailed discussion of the contents of this letter and the further development of the story until 1934, we refer to Jammer, 1974, p. 170–178, and Jammer, 1985, pp. 134–137.

knowing as yet which of the predictions will be made,' Ehrenfest concluded the description of Einstein's new *Gedanken*experiment in his letter to Bohr, and proposed that Bohr might visit Leyden in the fall to discuss the situation with Einstein (who was also expected to visit Leyden at that time). However, the meeting of Bohr and Einstein did not materialize; but on 4 November 1931, Einstein presented a talk in the Berlin colloquium entitled '*Über die Unbestimmtheitsrelationen* (On the Uncertainty Relations),' dealing with a photon-box experiment (Einstein, 1932). The aim of this talk was to point out that, whatever quantity had to be measured accurately, could be decided well after the photon had left the box.

On 4 April 1932, when Einstein was on his way back to Germany from another visit to the United States, he met Ehrenfest again in Rotterdam (where the ship docked for several days). Evidently, the two friends discussed further the *Gedanken*experiment, because the next day Einstein wrote a letter to Ehrenfest, and said:

> Yesterday you prodded me to modify the "box experiment" in such a way that it employs concepts more familiar to the wave-theoretician. This I do in the following by applying only such idealizations which, as I know, you will accept unhesitatingly. It operates as a schematized Compton effect. (Einstein to Ehrenfest, 5 April 1932)

The new experiment now suggested involved the interaction of a photon and a massive particle, and Einstein showed how either the momentum or the position of the heavy particle might be determined by observing the corresponding quantities of the photon. 'This is the reason why I find myself inclined to ascribe objective "reality" to both [observables, i.e., momentum and position],' he concluded (Einstein to Ehrenfest, *loc. cit.*). Apparently, he addressed here for the first time explicitly the question of 'reality' in quantum mechanics, and what he meant by it became clearer about one-and-a-half years later. Indeed, shortly before Einstein left Europe for good in fall 1933, he attended a lecture given by Léon Rosenfeld (who was then a lecturer at the University of Liège) in Brussels on the Bohr–Rosenfeld theory of the measurability of electromagnetic field quantities; he then expressed a certain uneasiness about the results obtained and asked Rosenfeld:

> What would you say about the following situation? Suppose two particles are set in motion towards each other with the same, very large momentum and that they interact with each other for a very short time when they pass at known positions. Consider now an observer who gets hold of one of the particles, far away from the region of interaction, and measures its momentum; then, from the conditions of the experiment, he will obviously be able to deduce the momentum of the other particle. If, however, he chooses to measure the position of the first particle, he will be able to tell where the particle is. This is a perfectly correct and straightforward deduction from the principles of quantum mechanics. (Rosenfeld, 1967, pp. 127–128)

However, Einstein considered the situation to be 'very paradoxical,' because: 'How can the final state of a second particle be influenced by a measurement performed on the first, after all physical interaction has ceased between them?' (Rosenfeld, *loc. cit.*, p. 128)

Thus, between spring and fall 1933, Einstein's *Gedanken*experiment finally took the direction toward what would be formulated in 1935 as the *EPR*-argument. It may be that the final write-up was also influenced by a paper of Karl Popper criticizing the uncertainty relations.[836] Popper had sent a copy of his note (Popper, 1934)—according to which the path of one particle determined via the conservation laws the path of its partner with which it had collided—to Einstein; and a similar situation was considered in the *EPR*-paper.[837] Still missing was only the 'completeness' argument, which could perhaps be obtained from the mathematical literature or conversations with John von Neumann (who was also at the Institute for Advanced Study in Princeton).[838] In any case, in spring 1935, the Princeton team of Einstein, Podolsky and Rosen connected the hitherto mathematical concept of completeness with the metaphysical concept of 'physical reality,' when they stated in the preamble of their paper:

> In a complete theory there is an element corresponding to each element of reality. A sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system. In quantum mechanics in the case of two physical quantities described by non-commuting operators, the knowledge of one precludes the knowledge of the other. Then either (1) the description of reality given by the wave function in quantum mechanics is not complete or (2) these two quantities cannot have simultaneous reality. (*EPR*, 1935, p. 777)

'Consideration of the problem of making predictions concerning a system on the basis of measurements made on another system that had previously interacted with it leads to the result that if (1) is false then (2) is also false,' *EPR* continued, and then sharply concluded that 'the physical description of reality as given by the wave function is not complete.' (*EPR*, *loc. cit.*)

Max Jammer, in his classic book on *The Philosophy of Quantum Mechanics*, organized the analysis of *EPR*'s four-page note (containing two sections) as follows:

> The paper contains four parts: (A) an epistemological-metaphysical preamble; (B) a general characterization of quantum-mechanical description; (C) the applciation of this description to a specific example; and (D) a conclusion drawn from parts (A) and (C). (Jammer, 1974, p. 181)

[836] We have mentioned it above in Footnote 828.

[837] For a detailed analysis of Popper's paper, see Jammer, 1974, pp. 174–178. In his reply to Popper, Einstein criticized the conclusion because it contradicted the indeterminacy relations.

[838] Max Jammer, in his detailed analysis, referred to remarks on the 'completeness of quantum mechanics' by Bohr and other physicists, and to the studies of the Polish logician Alfred Tarski (Jammer, 1985, pp. 137–139). As we have discussed in previous volumes, especially Volume 3, the concept of 'completeness' entered into the quantum-mechanical literature (Born, Heisenberg, and Jordan, 1926) quite early, and the Göttingen quantum-theoreticians took it from the mathematicians, especially, David Hilbert. Also, von Neumann, in his famous proof of 'hidden variables' (discussed in the foregoing Section III.3), made use of the same concept.

The preamble (A) started with a definition of what Einstein, Podolsky, and Rosen meant by reality, namely:

> Any serious consideration of a physical theory must take into account the distinction between the objective reality, which is independent of any theory, and the physical concepts with which the theory operates. These concepts are intended to correspond with the objective reality, and by means of the concepts we can picture this reality ourselves. (*EPR*, 1935, p. 777)

Then, they called a theory 'satisfactory' if the following two questions could be answered positively: 'Is the theory correct?' and 'Is the description given by the theory complete?'. By 'correct' they meant the 'agreement between the conclusions from the theory and human experience,' while they defined 'complete' by what they stated as a 'necessary requirement' in the summary, notably: 'Every element of the physical reality must have a counterpart in the physical theory.' (*EPR*, *loc. cit.*, p. 777) Since 'physical reality' had to be derived from experiments, a sufficient definition appeared to be the following:

> If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quality. (*EPR*, *loc. cit.*, p. 777)

In order to characterize briefly the quantum-mechanical formalism, Einstein, Podolsky, and Rosen considered a state described by the wave function ψ and its eigenvalues for a given quantity A; further, they assumed the commutation relations for canonical pairs of quantities, such as position and momentum, to be be valid, and arrived at two statements (1) and (2), as formulated in their summary (preamble) quoted above. Hence, they quickly concluded in part (B) that the usual statement, 'the wave function *does* contain a complete description of the physical reality of the system in the state to which it corresponds'—though 'at first sight entirely reasonable, for the information obtainable from a wave function seems to correspond exactly to what can be measured without altering the state'—nevertheless leads to a contradiction if one wants to preserve the above reality condition (*EPR*, *loc. cit.*, pp. 778–779).

In part (C), *EPR* constructed their *Gedanken*experiment by considering two sytems, each composed of a particle—*EPR* spoke of systems I and II—which were allowed to interact from time $t = 0$ to $t = T$, their state being known before $t = 0$ while it could be calculated for $t > T$ via the Schrödinger equation. This calculation yielded the wave functions for the combined system $\mathrm{I} + \mathrm{II}$, from which those of the separated systems were derived according to the standard quantum-mechanical process of 'reduction of the wave packet,' i.e., formally given by

$$\psi(x_1, x_2) = \sum_{n=1}^{\infty} \psi_n(x_2) u_n(x_1), \tag{650}$$

where $u_n(x_1)$ denoted the eigenfunctions of an operator A of the system (particle) I and $\psi_n(x)$ the corresponding eigenfunction of the system (particle) II. The measurement of another quantity B might lead to a different result

$$\psi(x_1, x_2) = \sum_{s=1}^{\infty} \phi_s(x_2) v_s(x_1), \tag{650'}$$

yielding afterward the states $v_s(x_1)$ and $\phi_s(x_2)$ of the systems I and II, respectively. 'Thus, *it is possible to assign two different wave functions* (in our example, ψ_k and ϕ_r) *to the same reality* ([i.e.,] the second system after the interaction with the first),' *EPR* concluded and referred to the fact that 'at the time of measurement [of A and B] the two systems [I and II] no longer interact,' hence 'no real change can take place in the second system in consequence of anything that may be done to the first system' (*EPR*, *loc. cit.*, p. 779). Now, in the special case that the physical quantities A and B were taken to be the momentum P and the position Q satisfying the commutation relations

$$PQ - QP = \left(\frac{h}{2\pi i}\right), \tag{651}$$

the following situation emerged: $\psi(x_1, x_2)$ could be written either as

$$\psi(x_1, x_2) = \int_{-\infty}^{+\infty} \exp\left(\frac{2\pi i}{h} x_1 p\right) \exp\left[-\frac{2\pi i}{h}(x_2 - x_0)p\right] dp \tag{652}$$

or

$$\psi(x_1, x_2) = h \int_{-\infty}^{+\infty} \delta(x_1 - x)\delta(x - x_2 + x_0)\, dx. \tag{652'}$$

In case (652), the associated wave functions were $u_p(x_1) = \exp\left(\frac{2\pi i}{h} x_1 p\right)$ and $\psi_p(x_2)$, corresponding to the operator P with the eigenvalues $p_1 = p$ for the particle I and $p_2 = -p$ for the particle II. In case (652′), on the other hand, the wave functions were $v_x(x_1) = \delta(x_1 - x)$ and $\phi_x(x_2) = \delta(x - x_2 + x_0)$, corresponding to the operator Q with the eigenvalues $x_1 = x$ and $x_2 = x + x_0$, respectively. 'Thus, by measuring either A or B we are in a position to predict with certainty, and without in any way disturbing the second system, either the value of the quantity P (that is p_k) or the value of the quantity Q (that is q_r),' *EPR* concluded and continued:

> In accordance with our criterion of reality, in the first case we must consider the quantity P as being an element of reality, in the second case the quantity Q is an element of reality. But, as we have seen, both wave functions ψ_k and ϕ_r belong to the same reality. (*EPR*, *loc. cit.*, p. 780)

EPR interpreted the result thus obtained as follows in part (D): Originally—i.e., in part (A)—they had argued that the situation in quantum mechanics should be described either by the assertion (1) or the assertion (2). However, now one had to argue rather:

> Starting then with the assumption that the wave function does give a complete description of the physical reality, we arrived at the conclusion that two physical quantities with noncommuting operators can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of the physical reality given by the wave function is not complete. (*EPR*, *loc. cit.*)

One might evade this consequence, *EPR* continued, by refining the definition of physical reality, say, by regarding 'two or more physical quantities as simultaneous elements of reality *only when they can be simultaneously measured or predicted*'—which would imply that 'P and Q are not simultaneously real'—*EPR* added, then 'the reality of P and Q depends upon the process of measurement carried out on the first system in any way'; however, they claimed: 'No reasonable definition of reality could be expected to permit this.' (*EPR*, *loc. cit.*) Finally, they expressed the hope which Einstein had cherished for more than a decade, namely, the firm belief that another theory may be found for the phenomena of atomic physics, such that a complete description of reality in the sense expressed above will be possible.

In his detailed study, 'The *EPR* Problem in Its Historical Development,' Max Jammer tried to single out the individual contribution of each of the three authors (Jammer, 1985). Evidently, Einstein, as he stated himself repeatedly (e.g., in his letter of 10 November 1945, quoted earlier), conceived the general idea of the *EPR*-argument.[839] Then the work on the paper was shared in equal parts, as Jammer learned especially from interviews with Nathan Rosen: *EPR* met for several weeks in early 1935 in Einstein's office to discuss the problem; then, 'Podolsky was the one who wrote the first draft,' and, as Rosen recalled, 'roughly speaking, one can say that Einstein contributed the general point of view and its implications, [and] I found the ψ-function (i.e., [the description of] the "*EPR* thought experiment"), and Podolsky composed the paper' (Jammer, *loc. cit.*, p. 142). Thus, Podolsky 'who liked to use the language of logic and was good at it' contributed an essential aspect, namely 'the completeness argument' which was previously not in the line of Einstein's thinking. (Jammer, *loc. cit.*)

The *EPR* paper, which expressed Einstein's unhappiness with the standard interpretation of quantum mechanics—or, rather expressed it most explicitly—also made his junior authors known to wider circles, especially the 26-year-old Nathan

[839] The background given above has been summarized by Jammer, 1985, pp. 141–144.

Rosen.[840] In order to prepare for the understanding of the response to the *EPR*-study, let us summarize its contents (with Jammer) as follows:

> The Einstein-Podolosky-Rosen [*EPR*] argument for the incompleteness of quantum mechanics is based ... on two explicitly formulated and two tacitly assumed—or only *en passent* mentioned—premises:
>
> 1. *The reality criterion*. "If without in any way disturbing a system we can predict with certainty ... the value of a physical quantity, then there exists an element of physical reality corresponding to this physical reality."
> 2. *The completeness criterion*. A physical theory is complete only if "every element of the physical reality has a counterpart in the physical theory."
>
> The tacitly assumed arguments are:
>
> 3. *The locality assumption*. If "at the time of measurement ... two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system."
> 4. *The validity assumption*. The statistical predictions of quantum mechanics are confirmed by experiment.
>
> ... The Einstein-Podolsky-Rosen argument then proves that on the basis of the reality criterion 1, assumptions 3 and 4 imply that the quantum mechanics does not satisfy criterion 2, that is, the necessary condition of completeness, and hence provides only an incomplete description of physical reality. (Jammer, 1974, pp. 184–185)

The various points mentioned here soon became the centre of a lively debate among the physicists, first some in the United States, and then the leading ones in Europe.

The publicity began already on Saturday, 4 May 1935—i.e., before the *EPR*-paper appeared in *The Physical Review*—when *The New York Times* carried an extensive report under the provocative headline 'Einstein Attacks Quantum Theory,' which was summarized by the sentences: 'Professor Einstein will attack science's important theory of quantum mechanics, a theory of which he was sort of grandfather. He concluded that while it [the quantum mechanics] is "correct" it

[840] Nathan Rosen, born on 22 March 1909, in Brooklyn, New York, received his education at the Massachusetts Institute of Technology (a B.S. in electrochemical engineering in 1929, and an Sc.D. in physics in 1932—with Philip M. Morse as his thesis advisor on quantum chemistry). Then, he held several postdoctoral positions, first at the University of Michigan and Princeton Foundation; from 1934 to 1936, he served as Einstein's assistant at the Institute for Advanced Study in Princeton and instructed Einstein in the details of the properties of wave functions in complex molecular situations. From 1936 to 1938, he worked as a professor of theoretical physics at Kiev State University, and then he returned to MIT; he taught for one year at Black Mountain College in North Carolina and became a member of the faculty of the University of North Carolina at Chapel Hill from 1941 to 1952. During World War II, he worked on uranium-isotope separation. In 1953, Rosen went to Israel and joined the Technion at Haifa as a professor of physics; at the Technion, he established the physics department and the graduate school and retired in 1973; in addition, he served from 1969 to 1971 as Dean of the Engineering School of the newly established Ben Gurion University of the Negev at Beersheba. He died on 18 December 1995, in Haifa.

is not "complete."' After a non-technical description of the main contents of the paper a statement attributed to Podolsky was added, namely:

> Physicists believe that there exist real material things independent of our minds and theories. We construct theories and invent words (such as electron, positron, etc.) in an attempt to explain to ourselves what we know about our external world and help us to obtain further knowledge about it. Before a theory can be considered to be satisfactory it must pass two severe tests. First, the theory must enable us to calculate facts of nature, and these calculations must agree very accurately with observation and experiment. Second, we expect a satisfactory theory, as a good image of objective reality, to contain a counterpart for every element of the physical world. A theory satisfying the first requirement must be called a correct theory while, if it satisfies the second requirement, it may be called a complete theory. (*The New York Times*, 4 May 1935, p. 11)

The article in the newspaper was followed by a report of an interview with the quantum theorist Edward Uhler Condon, then associate professor at Princeton University, who stressed that the *EPR*-argument of course depended on 'what meaning is to be attached to the word "reality" in connection with physics' but concluded that, in spite of Einstein's criticism of quantum-mechanical theories, 'I am afraid that thus far the statistical theories have withstood criticism.'

The public stir in *The New York Times* was completed by the strong statement of Einstein himself in the issue of 7 May (p. 21), who pointed out that 'any information upon which the article "Einstein Attacks Quantum Mechanics" in your issue of 4 May is based was given without authority.' The newspaper affair also terminated the previously friendly collaboration between Einstein and the young Russian-American theoretician Boris Podolsky, who left Princeton shortly thereafter. In a later essay (which will be discussed below), Einstein gave a few indications where his view deviated from that of the unauthorized spokesman (Einstein, 1936). In any case, as Einstein wished, from then on the discussion on the topic was carried on 'only in the appropriate forum' of scientific journals.[841]

(c) The Response of the Quantum Physicists, Notably, Bohr and Heisenberg, to *EPR* (1935)

The very first discussion of the *EPR*-argument occurred properly in the *Physical Review*, which published a letter of the Harvard theoretician Edwin C. Kemble, that was dated 25 May 1935, and appeared in the issue of 15 June (Kemble, 1935a). Kemble, a senior and experienced quantum physicist (who wrote a standard textbook on the subject: Kemble, 1937), expressed the opinion that 'the argument is not sound'; he had in mind especially the *EPR* assertion that the sys-

[841] The story of Einstein's dissatisfaction with Podolsky, and further details of the early response to the *EPR*-paper, can be found in Jammer, 1974, pp. 189–194, and especially in Jammer, 1985, pp. 144–146.

tem II 'cannot be affected by [the observation of I] and must in all cases constitute "the same physical reality"' Kemble, 1935a, p. 973). Kemble argued that 'here lies a fallacy, however, for whenever two systems interact for a short time there is a correlation between the subsequent behaviour of one system and that of the other,' and he claimed that the whole question had already been properly treated in 'the interpretation of quantum mechanics as a statistical mechanics of assemblages of like systems,' as had been 'most clearly formulated by Slater [1929a]' who had invoked the assumption 'that the wave functions of the Schrödinger theory have meaning primarily as descriptions of the behaviour of (infinite) assembleges of identical systems similarly prepared' (Kemble, *loc. cit.*, p. 974). Kemble then showed how to phrase the *EPR* argument correctly according to this interpretation and concluded: 'There seems no reason to doubt the completeness of the quantum-mechanical description of atomic systems within the frame of our present experimental knowledge.' (Kemble, *loc. cit.*)

The second response to the *EPR* paper also came from an American author: Arthur E. Ruark's letter dated 2 July was published in the *Physical Review* issue of 1 September 1935. Ruark principally attacked the conclusion of *EPR* that the quantities corresponding to both P and Q possess reality, because one should prefer to say 'that P and Q could possess reality only if *both* A and B (not merely one or the other) could be simultaneously measured' (Ruark, 1935, p. 466). He continued:

> Whereas Einstein, Podolosky and Rosen say it is not reasonable to suppose the reality of P and Q can depend on the process of measurement carried out on system I, an opponent could reply: (1) that it makes no difference whether the measurements are direct or indirect; (2) that system I is nothing more than an instrument, and the measurement of A makes this instrument unfit for the measurement of B. Such an opponent will feel that the ingenious method of measurement discussed by Einstein, Podolosky and Rosen suffers from all the essential difficulties common to measurements which result in disturbing system II. (Ruark, *loc. cit.*, p. 466)

Ruark closed his letter by saying: 'It seems to the writer that in the present state of our knowledge the question cannot be decided by reasoning based on accepted principles,' and added: 'The arguments which can be advanced on either side seem to be so far from conclusive, and the issue involved appears to be a matter of personal choice or of definition.' (Ruark, *loc. cit.*, p. 467) The latter opinion was not shared at all by his European colleagues Bohr, Heisenberg and Pauli.

Léon Rosenfeld, Niels Bohr's closest collaborator in the 1930s, recalled that the *EPR* paper first 'came down upon us as a bolt from the blue' (Rosenfeld, 1967, p. 128). Previously, the quantum physicists in Copenhagen had been quite used to Einstein's attacks on quantum mechanics (since 1927, see our discussion in Section II.6). 'The situation changed radically, however, on the publication [of this paper],' wrote Jørgen Kalckar in introducing the 'last battle' between Bohr and Einstein on the interpretation of quantum mechanics:

> Not only did it attract the attention of many physicists, but the ensuing discussions aroused interest in the more philosophical aspects of quantum physics far outside the physics community. (Kalckar, in Bohr, 1996, p. 250)

Looking at the 'Copenhagen theorists' in more detail, one may recognize two different general attitudes. On the one hand, especially Pauli and Heisenberg—and, to some extent, Bohr himself—were greatly surprised that Einstein had published statements which appeared to contain just the old and, at times, even 'stupid' arguments. On the other hand, Bohr still became rather worried, as Rosenfeld recalled several decades later:

> We were then in the midst of groping attempts at exploring the implications of the fluctuations of charge and current distributions, which presented us with riddles of a kind we had not met in electrodynamics. A new worry could not come at a less propitious time. Yet, as soon as Bohr heard my report of Einstein's argument, everything else was abandoned; we had to clear up such a misunderstanding at once. We should reply by taking up the same example and showing the right way to speak about it. In great excitement, Bohr immediately started discussing with me the outline of such a reply. Very soon, however, he became hesitant. "No, this won't do, we must try all over again.... we must make it quite clear ..." So it went on for a while, with growing wonder at the unexpected subtlety of the argument. Now and then, he would turn to me: "What *can* they mean? Do *you* understand it?" There would follow some inconclusive exegesis. Clearly, we were farther from the mark than we first thought. Eventually, he broke off with the familiar remark that he "must sleep on it." The next morning, he at once took up the dictation again, and I was struck by a change in the tone of sentences: there was no trace in them of the previous days sharp expression of dissent. As I pointed out to him that he seemed to take a milder view of the case, he smiled: "That's a sign," he said, "that we are beginning to understand the problem." And, indeed, the real problem now began in earnest: day after day, week after week, the whole argument was patiently scrutinized with the help of simpler and more transparent examples. Einstein's problem was reshaped and its solution reformulated with such precision and clarity that weakness in the critic's reasoning became evident. (Rosenfeld, 1967, pp. 128–129)

On 29 June 1935, Bohr wrote a letter to the British journal *Nature*, in which just before, in the issue of 22 June, a note signed by H. T. F. (i.e., H. T. Flint from the University of London) had drawn attention to the *EPR* paper—and sketched his answer to the 'criterion of physical reality' of Einstein, Podolsky, and Rosen; in particular, he wrote:

> I would like to point out, however, that the named criterion contains an essential ambiguity when it is applied to the problems of quantum mechanics. It is true that in the measurement under consideration any direct mechanical interaction of the system and the measuring agencies is excluded, but a closer examination reveals that the procedure of measurement has an essential influence on the conditions on which the very definition of the physical quantities in question rests. Since these conditions must be considered as an inherent element of any phenomenon to which the term "physical

> reality" can be unambiguously applied, the conclusion of the above-mentioned authors [*EPR*] would not appear to be justified. A further development of this argument will be given in an article to be published in the *Physical Review*. (Bohr, 1935a, p. 65)

This article of Niels Bohr was indeed received by the *Physical Review* on 13 July 1935, and published in the 15 October issue of the same year (Bohr, 1935b).

Unlike Bohr, Pauli and Heisenberg took the *EPR*-arguments with much less worry, as was revealed by their correspondence. Thus, Pauli wrote in a letter to Heisenberg, dated 15 June 1935, about 'two pedagogical problems where you could perhaps interfere publicly,' addressing with the first an idea of the Italian theorist Gian Carlo Wick on the origin of the proton's magnetic moment, and with the second, especially:

> Einstein has once again made a public statement about quantum mechanics, and even in the issue of *Physical Review* of May 15 (together with Podolsky and Rosen, not a good company by the way). As is well known, that is a disaster whenever it happens. "Because, thus he concludes most sharply nothing can exist if it ought not to exist. (*Weil, so schließt er messerscharf, nicht sein kann was nicht sein darf.*)"
>
> Still I would grant him that if a student in one of his earlier semesters had raised such objections, I would have considered him quite intelligent and promising. Since through this publication there exists a certain danger of confusing the public opinion—notably in America—it might perhaps be advisable to send an answer to the *Physical Review* which I would like to persuade *you* to undertake. (See Pauli, 1985, p. 402; English translation in Bohr, 1996, pp. 251–252)

Pauli then outlined in his letter to Heisenberg 'the facts demanded by quantum mechanics which cause particular mental troubles to Einstein,' namely essentially 'the connection of two systems in quantum mechanics.' After outlining the results obtained by calculation of the systems 1 and 2, he characterized the *EPR* interpretation as follows:

> Now comes the "deep feeling" which tells you: "Since the measurement of 2 does not disturb the particle 1, there must be something called 'physical reality,' namely the state of particle 1 *per se*—independently of which measurement one has performed at 2." It would be absurd to assume that particle 1 is changed by measurements at 2, i.e., it is transformed from a [given] state into another. In reality, the quantum-mechanical description must attribute characteristics to the particle 1 which contain already all those properties of 1 which—*after* possible measurements of 2 which do not disturb 1—can be predicted with certainty. (Pauli, *loc. cit.*, p. 403)

Now, the pedagogical response on this argumentation, which Pauli expected Heisenberg to formulate, had to clarify in particular the difference between two different situations: '(a) Two systems 1 and 2 have no interaction at all (i.e., the interaction energy is missing)—in that case the observation of all quantities of 1 yield *the same time evolution as if there were no system 2*,' and (b) 'The total system [1 + 2] is in a state where the partial systems 1 and 2 do not depend on each other (separation of an eigenfunction into a product of two eigenfunctions)—in that case the expectation values of the quantities F_1 of 1 remain, after the performance of measurement of an arbitrary quantity F_2 at 2 with known numerical result $F_2 = (F_2)_0$, the same as without performing a measurement at 2.' According to Pauli, Einstein felt correctly that the composition and separation of systems should play a greater role in considering the foundations of quantum mechanics; since this point happened to be closely connected with Heisenberg's 'considerations about [the quantum-mechanical] cut and the possibility to shift it arbitrarily [as he had emphasized it in his talk at the Hanover *Naturforscherversammlung* (Heisenberg, 1934f)],' Pauli requested Heisenberg to '*present* [the situation] *once in a short* [article], not in *a popular language but with the use of formulae*,' and emphasized:

> One must distinguish different levels of reality (*Schichten der Realität*): one R containing all interactions which one can obtain by measurements of 1 and 2, another r (deducible from R) which contains only interactions obtainable by measurements at 1 alone. Then one must show how from the statement (*Bekanntgabe*) of a measurement's result at 2 a discontinuous change of r ($r \rightarrow r_A$ or $r \rightarrow r_B$, etc.) follows (unless the systems of particles were independent); and that necessarily contradictions would arise if one tried to describe these changes without referring to 2—say, by "hidden properties" of 1 in a classical or semi-classical manner. (Pauli, *loc. cit.*, p. 404)

In any case, Pauli hoped that Heisenberg would contradict in his answer to the *EPR* paper the idea which 'haunted elderly gentlemen like [von] Laue and Einstein' that the present quantum mechanics was incomplete and must be '*completed by statements it does not [yet] contain*,' such as 'hidden variables'; he (Heisenberg) should especially 'make it obvious in an authoritative manner that such a supplement to quantum mechanics is impossible without changing its contents' (Pauli, *loc. cit.*).

Heisenberg took Pauli's request seriously and soon got down to work on the proposed paper. Meanwhile, he had heard from Copenhagen about Bohr's considerations in response to the *EPR*-argument; therefore, he concentrated on his manuscript, entitled '*Ist eine deterministische Ergänzung der Quantenmechanik möglich*? (Is a Deterministic Extension of Quantum Mechanics Possible?),' very much on the '*Schnitt* (cut) problem' and the supposed 'incompleteness of quantum theory' (Heisenberg to Pauli, 2 July 1935, in Pauli, 1985, pp. 409–418).[842] As Heisenberg would report to Bohr, 'the essay was perhaps intended for publication in *Naturwissenschaften* … and thought to contain an answer to von Laue and Schrödinger, especially since I heard from [Arnold] Berliner that soon a similar essay would appear [in that journal] written by Schrödinger'; and further: 'In it I

[842] It is not certain whether Heisenberg enclosed already the above-mentioned manuscript in his letter of 2 July to Pauli, because he did not mention its existence even in his later letter to Bohr, dated 14 July 1935. We assume that Heisenberg composed it later in July or August; in any case, he sent a copy of the type-written manuscript on 22 August in a letter to Bohr.

have—in order not to write the same as you, because I still cannot do so as well—I have emphasized a little more the formal and logical side of the problem.' (Heisenberg to Bohr, 22 August 1935) That is, Heisenberg, in his paper, mainly tried to reply to the peculiar question addressed by von Laue (1932b, 1934) and also now by Einstein, Podolsky, and Rosen (1935) 'whether quantum mechanics may not later, due to new physical experiences, be so supplemented as to become a deterministic theory.'[843] Notably, he wrote:

> Such a consideration in general assumes, *vis-a-vis* the experimental successes of quantum mechanics, as a prerequisite that quantum mechanics provides [at present] a *correct* description of nature. It connects this prerequisite, however, with the hope that the later research will uncover behind statistical connections of quantum mechanics a hitherto hidden net of causal connections—just as behind the temperature and entropy concepts of heat theory classical mechanics lies hidden. These causal connections should not at all necessarily concern the visualizable (*anschaulichen*) classical properties of physical systems; rather one concludes from the validity of the indeterminacy relations that the classical concepts do not allow an adequate description of atomic phenomena, that therefore new concepts must be formed which are associated perhaps with the hitherto unknown physical properties of atomic systems. (See Heisenberg's manuscript, reproduced in Pauli, 1985, pp. 409–410)

Heisenberg, in the considerations in his manuscript, wished to demonstrate 'that such a deterministic addition to quantum mechanics is impossible, and that one can therefore cherish the hope for a deterministic description of nature only if one considers the most important successes of quantum mechanics to be accidental' (Heisenberg, in Pauli, *loc. cit.*, p. 410). He then demonstrated this claim in three sections, emphasizing at the same time that his manuscript did not contain anything new beyond what could be found in the earlier publications of Bohr, von Neumann, Pauli, and himself.

In Section 1, Heisenberg addressed, in particular, 'the noteworthy schism (*Zwiespalt*)' of the quantum-mechanical description of nature: 'On the one hand, it assumes the task of physics to be the lawful description and synopsis of visualizable, objective processes in space and time; on the other hand, it uses for a mathematical representation of physical processes those wave functions in multidimensional configuration spaces which in no way can be regarded as representative of the objective happenings in space and time such as, say, the coordinates of a mass point in classical mechanics.' (In Pauli, *loc. cit.*, pp. 410–411) This schism, Heisenberg continued, leads to a certain 'arbitrariness in applying quantum mechanics': i.e., either the observed atomic system is described by quantum mechanics and the apparatus used for observation obeys the laws of classical physics, *or* also the apparatus is described by wave functions and only 'the observation of the measuring apparatus, e.g., the observation of a line on the photographic plate'

[843] Heisenberg's manuscript was found in the Pauli *Nachlaß* and has been published in Pauli, 1985, pp. 409–418, following Heisenberg's letter to Pauli dated 2 July 1935.

obeys classical laws. This so-called 'cut' or 'gap' (*Schnitt*) between the descriptions of quantum mechanics and classical theory could thus be placed arbitrarily, that is: '*The quantum-mechanical predictions concerning the result of any experiment do not depend on the position of the cut in question*' (Heisenberg, in Pauli, *loc. cit.*, p. 411), as Heisenberg proved explicitly in an example: He took an atomic system A, and considered the existence of several measuring apparatuses B, C, ... (which provide the observer the final observation) which is treated by quantum mechanics and by classical theory, respectively.

Clearly, the process A must be described by the time-dependent wave function $\psi_A(q_A, t)$, yielding the probability $|\psi_A(q'_A, t')|^2$ for the coordinate to assume at time $t = t'$ the value q'_A as registered by the apparatus B, C, etc. in accordance with the classical laws. On the other hand, if $A + B$ were treated quantum-mechanically, an application of the time-dependent Schrödinger equation (with H_A, H_B, and H_{AB} denoting the Hamiltonian operators of the systems A and B and the interaction energy, respectively),

$$\left(\frac{h}{2\pi i}\frac{\partial}{\partial t} + H_A + H_B\right)\psi(q_A, q_B, t) = -H_{AB}\psi(q_A, q_B, t), \tag{653}$$

provided the wave function $\psi(q_A, q_B, t)$ which—since H_{AB} deviated from zero only for the value $q_A = q'_A$—might be expressed as

$$\psi(q_A, q_B, t) = \psi_A(q_A, t)\psi_B(q_B, t) + \psi_A(q'_A, t')\phi(q_A, q_B, t, t'), \tag{654}$$

where $\phi(q_A, q_B, t, t')$ was independent of the behaviour of the system A before $t = t'$. Now, the probability of the system B to undergo a change from the original state—i.e., $\psi_B(q_B, t)$—was given by the integral over the absolute square of the interaction term on the right-hand side of Eq. (654) in the variables q_A and q_B, $\int dq_A dq_B |\psi(q'_A, t')\phi(q_A, q_B, t, t')|^2$; hence, it became proportional to $|\psi_A(q'_A, t')|^2$ and did not depend on the prehistory of the system A. As a consequence, the quantum-mechanical result in case the cut is transferred beyond B turned out to be the same as before; similarly, one could transfer the cut beyond C, etc.

Thus, Heisenberg stated that the characteristic property of the application of the wave-mechanical description of the measuring apparatus was the fact that the interaction with the atomic system (to be measured) resulted only in transitions of the coordinate q_B at a fixed value, q'_A, of the coordinate q_A of the atomic system: and then q'_B was just changed to q''_B, or 'the total wave function then appears (for a short time after switching on the interaction) as a product of two factors, one of which being given by the wave function of the observed system A at the moment the interaction is switched on, while the other represents the reaction of the measuring apparatus B.' (In Pauli, *loc. cit.*, p. 413) This result came out of the peculiar properties of the quantum-mechanical formalism, and, as a consequence, 'the causal connections of the classical theories used in the measuring apparatus can be reproduced in quantum mechanics only with that degree of accuracy as the visualizable classical characteristics of the measuring apparatus are represented in wave optics'—but 'the fundamental indeterminacy created in this way of formulating causal connections is in all practical cases much smaller than the practical uncertainty that must be taken into account for every—even the best—measuring device.' (In Pauli, *loc. cit.*) Heisenberg concluded the more technical Section 1 with two remarks: (i) the cut cannot be shifted so arbitrarily that certain measuring devices operating like atomic systems (e.g., nuclear systems measuring the neutron flux) are described by classical theory; (ii) since the wave-mechanical formalism *per se* operates with respect to a causal behaviour like classical theory, and the statistical aspect enters only via the cut, the whole measurement process can represent causal connections in a restricted sense.

In Section 2, Heisenberg investigated 'the assumption that the physical systems described statistically by quantum mechanics carry up-to-now unknown physical properties which determine so far only the statistically known behaviour uniquely' (Pauli, *loc. cit.*, p. 414); contrary to the expectations of von Laue and *EPR*, he showed that 'this assumption contradicts the statements of quantum mechanics, especially not only its statistical results but also the definite conclusions derived' (in Pauli, *loc. cit.*, p. 415). This impossibility proof of 'hidden variables' to establish a causal behaviour was based on the premise that quantum mechanics determined uniquely *all* properties of the system left of the cut, i.e., either of A, or $A + B$, or $A + B + C$, etc. Hence, if extra properties had to be assumed for A in order to turn the statistical statements of the measurement into definite results, also changes of the properties of $A + B$, or $A + B + C$, etc., must arise, and 'every statement about A which was not already contained in the quantum-mechanical connection $A + B$ [or $A + B + C$, etc.] can contradict the conclusions from this connection' (in Pauli, *loc. cit.*), thus also the above premise. Heisenberg then illustrated this situation in an example, where he tried to obtain information about complementary quantities of the system A. 'For a supplement of the quantum-mechanical statements, the only suitable place was that of the "cut,"' he found, 'but this place cannot be fixed physically, since it is rather the arbitrariness in the choice of the position of the cut that is responsible for the [consistent] application of quantum mechanics'; hence, 'Any physical properties so far unknown that must be connected necessarily with a physical system therefore could not serve in principle to supplement quantum-mechanical statements.' (In Pauli, *loc. cit.*, p. 416) After illustrating this result in the case of the radioactive α-decay (by applying the complementary particle- and wave-pictures, respectively), Heisenberg closed Section 2 with two comments:

> It is a decisive feature of quantum mechanics that it permits via its formalism to connect the physical domains foreign, in principle, to our visualization in an organic way with the macroscopic, visualizable domain, such that the results from the formalism can be expressed by visualizable (*anschauliche*) concepts.
>
> However, quantum mechanics, explicitly presupposes—like the argumentation presented here—that at the same place we are finally able to turn our interactions into objective entities (*unsere Wechselwirkungen zu objektivieren*), i.e., allow us to speak about objects and events. Classical physics proves that this can be done for a

> large domain [of experience], and all of experimental science rests on this possibility. (Heisenberg, in Pauli, *loc. cit.*, p. 417)

In Section 3, Heisenberg argued that the philosophical explanations of these conclusions must be traced in the very essence of Nature, or—as stated by Grete Hermann (1935a)—'that a deterministic supplement of quantum mechanics fails because quantum mechanics already allows us to give completely the causes for the occurrence of a given result of measurement.' (Heisenberg, in Pauli, 1985, p. 417) This situation involves the problem to search for the particular feature of nature which forbids us to derive from the uniquely connected—one might even say, causal—formalism of quantum mechanics all (possible) results of measurement, and which creates the statistical connections at the cut. A quantum-mechanical state, Heisenberg said, is given uniquely by a wave packet moving with a certain velocity at a fixed space-point *plus* 'further statements about the size and shape of the wave packet, *for which there exist no analogues in the classical theory*' (Heisenberg, in Pauli, *loc. cit.*, p. 418); he called such a description a '*Beobachtungszusammenhang* (context of observation)' and emphasized that 'the same visualizable events may correspond to different contexts of observation,' a situation that was not known to occur in classical physics. 'The experimental conclusion formulated by quantum mechanics has shown that the observation of a system in general leads from one *Beobachtungszusammenhang* into another,' Heisenberg explained, and noted:

> The causal connection can be followed within a definite context of observation, while in the discontinuous transition from one [situation] to the other (especially to a "complementary" [one] in the sense of Bohr) only statistical predictions are possible. Hence the possibility of different, complementary contexts of observation, unknown in the classical theory, becomes responsible for the occurrence of statistical laws. (Heisenberg, in Pauli, *loc. cit.*)

Finally, Heisenberg questioned whether a future modification of quantum mechanics might give rise to a deterministic supplement, but he firmly claimed that experimental evidence so far provided no hint that 'the future description of nature will fit again into the narrow classical scheme of a visualizable and causal description of objective processes in space and time' (in Pauli, *loc. cit.*).

While no written comment of Pauli on Heisenberg's manuscript has survived among the available documents, Niels Bohr, in a letter dated 15 September 1935, to Heisenberg, asked for a few clarifications of complementary situations, which Heisenberg tried to provide in his letter of 29 September. Bohr further criticized that he placed too much emphasis on the 'shift of the cut,' to which Heisenberg replied as follows:

> Why the possibility of shifting the "cut" is so particularly important in my opinion, I can most simply explain thus: You say correctly that "all elements of description are defined classically and yet the classical theory leaves no room for quantum-mechanical

> laws." This statement appears to physicists used to think formally as a plain contradiction, as I know for instance from talking to *Herr* von Laue. Hence I thought it to be important to stress the property of the formalism which ensures that no contradiction arises here, and this, it seems to me, lies in the possibility to shift the cut. If this were not so, simply two categories of physical systems—classical and quantum-mechanical ones—would exist, and one could *never* apply classical concepts to the latter. That's how von Laue sees the situation. I believe that then it might be very difficult to argue against the hope of a later causal supplement. (Heisenberg to Bohr, 29 September 1935)

In any case, Heisenberg believed that the most direct way to understand why the quantum-mechanical formalism did not at all need new concepts, totally different from the classical ones, was to make effective use of the possibility to shift the 'cut.' He hoped to be able to discuss these questions in greater detail with Bohr in October in Copenhagen, especially the latter's arguments against the 'more formal manner of treating quantum theory' and promised not to submit his manuscript for publication prior to these discussions.[844]

We shall now discuss the official response given by Niels Bohr to the *EPR* argument, published in the *Physical Review* issue of 15 October 1935, which—unlike Heisenberg's manuscript—worked with very little formalism in the style to which Bohr had become accustomed in the previous 15 years. His answer was contained especially in the comment which he added after he had summarized the conclusion of the *EPR*-paper, and which read:

> Such an argumentation, however, would hardly seem suited to affect the soundness of quantum-mechanical description which is based on a coherent mathematical description covering automatically any procedure of measurement like that indicated.* The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails—because of the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose—the necessity of a final renunciation of the classical idea of causality and a radical revision of our attitude towards the problem of physical reality. In fact, as we shall see, a criterion of reality like that proposed by the authors [i.e., *EPR*] contains—however cautious its formulation may appear—an essential ambiguity when it is applied to the actual problems with which we are here concerned. In order to make the argument to this end as clear as possible, I shall first consider in some detail a few simple examples of measuring arrangements. (Bohr, 1935b, pp. 696–697)

[844] We do not know the results of the discussions in Copenhagen in October 1935 on this subject, as they were not mentioned in Heisenberg's letter to Bohr, in which he thanked the latter for the 'fine time' in 'your circle' and the 'wonderful mixture of leisure and serious thinking.' One reason for not sending his manuscript for publication may also have been the more difficult situation—which Heisenberg soon experienced—that existed towards modern theoretical physics; in particular, he did not wish to attack people like Planck or von Laue [who were also under attack from Nazi partisans and representatives of 'German Physics (*Deutsche Physik*)'].

From these lines of argument, the style of Bohr's answer may be recognized. He intended to continue the previous discussions with Einstein (at the fifth and sixth Solvay Conferences of 1927 and 1930, respectively) by referring to the particular *Gedanken*experiments which could be worked out with a minimum of mathematical formalism. To characterize the subordinate position given here to mathematical argumentation, as compared to Heisenberg's procedure, Bohr put the entire formal apparatus essentially into a single footnote, marked by an asterisk (*) (attached to the first sentence in the above quotation). Having emphasized the mathematical completeness of the quantum-mechanical scheme by a sentence, he went on quickly to describe an atomic system consisting of two partial systems (1) and (2), interacting or not, by two pairs of canonical variables, (q_1p_1) and (q_2p_2), which satisfy the commutation rules,

$$\left.\begin{aligned}&[q_1,p_1]=[q_2,p_2]=\frac{ih}{2\pi},\\&[q_1,q_2]=[p_1,p_2]=[q_1,p_2]=[q_2,p_1]=0.\end{aligned}\right\}\tag{655}$$

A canonical transformation by a simple orthogonal transformation yielded new pairs of conjugate variables, (Q_1,P_1) and (Q_2,P_2), defined by the equations

$$\left.\begin{aligned}&q_1=Q_1\cos\theta-Q_2\sin\theta,\ p_1=P_1\cos\theta-P_2\sin\theta,\\&q_2=Q_1\sin\theta+Q_2\cos\theta,\ p_2=P_1\sin\theta+P_2\cos\theta,\end{aligned}\right\}\tag{656}$$

with the angle of rotation θ. The analogous commutation relations, with the transformed Q's and P's replacing the original q's and p's in Eq. (655), implied that in the description of the combined system definite values could not be assigned to both Q_1 and P_1, but certainly one could assign such values to Q_1 and P_2, etc.—i.e., all variables which commute. Further, from the expressions Q_1 and P_2, namely,

$$Q_1=q_1\cos\theta+q_2\sin\theta,\ P_2=-p_1\sin\theta+p_2\cos\theta,\tag{657}$$

one derived that a subsequent measurement of either q_2 or p_2 would allow one to predict the value of q_1 or p_1, respectively. Eqs. (655) to (657) provided all the quantum-mechanical formalism needed by Bohr, who put all his efforts in the discussion of the following *Gedanken*experiment.

Bohr began by considering the passage of an atomic particle through an arrangement of diaphragms with parallel slits which allow either to detect the position or the momentum of the object accurately—in the first case the diaphragms have to be fixed rigidly, in the second case not rigidly—as was known from previous discussions. Bohr commented: 'My main purpose in repeating these simple ... considerations, is to emphasize that in the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrificing other such elements, but a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum.' (Bohr, *loc. cit.*, p. 699) On the one hand, there was the 'freedom of handling the measuring instruments, characteristic of the very idea of experiment'; on the other hand, quantum theory, because of 'the impossibility of accurately controlling the reaction of the object to the measuring apparatus, i.e., the transfer of momentum in case of position measurements, and the displacement in case of momentum measurements,' implied 'the renunciation in each experimental arrangement of one or the other of the two aspects of the description of physical phenomena—the combination of which characterizes the method of classical physics.' (Bohr, *loc. cit.*) Bohr continued:

> Just in this last respect any comparison between quantum mechanics and ordinary statistical mechanics ... is essentially irrelevant. Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way. (Bohr, *loc. cit.*)

After these preliminary remarks, Bohr reproduced the *EPR Gedanken*experiment on the interaction of two particles:

> at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction; while of course the conjugate quantities, i.e., the difference of the components of their momenta, and the sum of the positional coordinates, are entirely unknown. (Bohr, *loc. cit.*)

At this point, Bohr added a footnote which explained how the experiment thus proposed was theoretically described by the transformation of the variables according to Eqs. (656) with the particular rotational angle $\theta = \pi/2$; further he emphasized that the wave function (652) of *EPR* corresponded 'to the special choice of $P_2 = 0$ and the limiting case of two infinitely narrow slits' (Bohr, *loc. cit.*, footnote). 'In this arrangement it is therefore clear that a subsequent single measurement either of the position or of the momentum of one of the particles will automatically determine the position or momentum, respectively, of the other particle with any desired accuracy,' he continued and further admitted: 'As pointed out by the named authors [i.e., *EPR*], we are therefore faced at this stage with a completely free choice whether we want to determine the one or the other of the latter quantities by a process which does not directly interfere with the particle concerned.' (Bohr, *loc. cit.*, p. 699) However, Bohr interpreted this 'freedom of choice' just as '*a discrimination between different experimental procedures* [*in quantum mechanics*] *which allow of the unambiguous use of complementary classical concepts*,' (Bohr, *loc. cit.*, p. 700) and then went on to explain the well-known situation in atomic theory which required a quite different interpretation than proposed by *EPR*. In particular, he summarized the quantum-theoretical position as follows:

> From our point of view we now see that the wording of the … criterion of physical reality proposed by Einstein, Podolosky and Rosen contains an ambiguity as regards the meaning of the expression "without in any way disturbing the system." Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system*. Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that the quantum-mechanical description is essentially incomplete. (Bohr, *loc. cit.*, p. 699)

Quantum mechanics rather '*may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory*,' Bohr emphasized (Bohr, *loc. cit.*, our italics)—i.e., only the recognition of this fact in atomic physics, in his opinion, 'provides room for new physical laws' characterized by 'the notion of *complementary* aims' (Bohr, *loc. cit.*).

In the discussion of Bohr's experiment, the time played only a secondary role, but certainly also the consideration of the time and energy measurements which had been emphasized by *EPR* could be discussed according to the rules of the fundamental quantum-mechanical complementarity. To Bohr, the essential point seemed to be 'the necessity of discriminating in each experiment between those parts of the physical system considered which constitute the objects under investigation'; their necessity 'may indeed be said to form *a principal distinction between classical and quantum and quantum-mechanical descriptions of physical phenomena*,' Bohr concluded, explaining:

> While, however, in classical physics, the distinction between object and measuring agencies does not entail any difference in the character of the description of the phenomena concerned, its fundamental importance in quantum theory, as we have seen, has its root in the indispensable use of classical concepts in the interpretation of all

> proper measurements, even though the classical theories do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics. (Bohr, *loc. cit.*, p. 701)

Hence, 'there can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules ... which have found their general expression through the transformation theorems.' These theorems secured the correspondence of quantum mechanics with the classical theory and excluded 'any imaginable inconsistency in the quantum-mechanical description, connected with a change of the place where the discrimination is made between object and measuring agencies.' Bohr concluded his paper by announcing in a footnote a further study 'where the writer will in particular discuss a very interesting paradox suggested by Einstein concerning the application of gravitation theory to energy measurements, and the solution of which offers an especially instructive illustration of the generality of the argument of complementarity,' and further: 'On the same occasion a more thorough discussion of space-time measurements in quantum theory will be given with all necessary developments and diagrams of experimental arrangements, which had been left out in this article, where the main stress is laid on the dialectic aspect of the question at issue.' (Bohr, *loc. cit.*, pp. 701–702) However, this detailed paper intended to extend the complementarity philosophy further never appeared.

(d) Erwin Schrödinger Joins Albert Einstein: The Cat Paradox (1935–1936)

Unlike Albert Einstein, Erwin Schrödinger had regularly published since 1927 his thoughts about quantum mechanics and its interpretation (e.g., Schrödinger, 1928; 1929b, c; 1932b). From the very beginning, he had shared with Einstein the uneasiness, first concerning certain results—such as the uncertainty or indeterminacy relations—and later the 'unvisualizable (*unanschauliche*)' consequences of quantum mechanics. In fact, he often discussed these questions with Einstein when they were together in Berlin, and they both left after the Nazis took over the government of Germany. While Einstein, after spending several months in Europe (in the remote and secluded *Villa 'Savoyarde'* in Le Coq-sur-mer, the resort town near Ostende on the Belgian coast), settled down for good at the Institute for Advanced Study in Princeton, Schrödinger first went in summer 1933 as a Fellow of Magdalen College at Oxford, and did not really know whether he should stay in England in the following years. On 17 May 1935, he wrote to Albert Einstein: 'The feeling grows that I hold no position and depend on the generosity of others,' and added, 'When I came here I thought I could do something valuable for teaching, but one did not care about that here. And further, I think that in truth I must tell myself that in reality I am staying here for a very nice old man [Augustus Love] to die or become disabled and that one calls upon me to be his successor.' He therefore hoped, as he reported to Einstein further, to obtain a position in

Austria, namely, the chair of Professor Michael Radaković in Graz.[845] Three weeks later, Schrödinger took up his correspondence again with Einstein, and entered into a lively discussion of the contents of the paper of Einstein, Podolsky, and Rosen:[846]

> Dear Einstein,
> I have much rejoiced that in your just published paper in *Physical Review*, you have publicly gotten to the heart (*öffentlich beim Schlaffitchen erwischt hast*) the dogmatic quantum mechanics, about which we have discussed so much in Berlin. May I add a few things to it? At first they look like objections; but they concern only points which I wish had been formulated more clearly. (Schrödinger to Einstein, 7 June 1935)

Schrödinger thus began his letter to Einstein, in which he analyzed the procedure of proof in the *EPR* paper. In particular, he argued: 'In constructing a contradiction, it does not suffice in my opinion that for the identical preparation of a pair of systems the following may occur: *one* definite single measurement of the first system yields for the second a certain value A, another a certain value B, and the simultaneous reality of A and B is excluded because of general reasons.' Identical preparation would not always lead to the same result, but may yield in one case the value A' for the quantity A, in a second one the value A'' for the same quantity, and in a third the value B' for the different quantity B. Thus, in order to establish a genuine contradiction, one should rather require for a pair of systems the existence of two quantities A and B whose reality is mutually excluded, and further:

1. *One* method of measurement exists which yields for the quantity A for a wave function *always* a sharply defined (though not always the same) value, hence I can say without actually performing the experiment: in case of the given wave function, A possesses reality, independently of its value.

[845] Officially, Schrödinger was at Oxford on leave of absence from the University of Berlin, which was extended until he requested his *Emeritierung* in early 1935. In Austria, where he had looked for a permanent position since summer 1933 (and also asked for the restoration of his Austrian citizenship), it took until September 1936 when the Schrödingers could move to Graz. Two years later, after the annexation (*Anschluß*) of Austria with the *Third Reich*, Schrödinger lost this position and—after a transitory period again at Magdalen College in Oxford—he received in December 1938 a professorship at the University of Ghent in Belgium. Upon the outbreak of World War II, Schrödinger had to leave Belgium (having become officially a 'hostile alien'). The Irish politician and prime minister, Eamon de Valera, who had always been an amateur mathematician and had hoped to establish an 'Institute for Advanced Studies' in Dublin, invited Schrödinger (who had taken refuge at the Pontifical Academy of Sciences at the Vatican) to meet with him in Geneva, Switzerland (where he, de Valera, was attending a meeting of the League of Nations); as a result of their meeting, de Valera advanced the schedule of the founding of the Institute for Advanced Studies in Dublin, and invited Schrödinger to join it as a Senior Professor of Theoretical Physics in October 1939 (being made the Director of the Theoretical Physics Division in November 1940).

[846] The Einstein–Schrödinger correspondence between 1935 and 1947 on the interpretation of quantum mechanics has not been included in the correspondence collection edited by Karl Przibram (Schrödinger *et al.*, 1963). We thank Robert Schulmann for providing us with the contents.

2. *Another* method of measurement should at least *occasionally* give the quantity B a sharp value (always for the same wave function, of course). (Schrödinger to Einstein, *loc. cit.*)

In general, Schrödinger continued, there exists only *one* way of expanding a function of two variables (or groups of variables) in a bilinear series,

$$\psi(x_1, x_2) = \sum_{n=1}^{\infty} c_n \psi_n(x_2) u_n(x_1), \tag{658}$$

such that both $u_n(x_1)$ and $\psi_n(x_2)$ form a normalized orthogonal system. Now, if two of the coefficients c_n assume identical absolute value, the expansion (658) ceases to be uniquely defined, and in the *EPR* case, all c_n were taken to be equal: 'Hence you can rotate [by a canonical transformation of the quantum-mechanical system] in an arbitrary manner, even from the "*Q*-position" into the "*P*-position."' Apart from suggesting this sharper formulation, however, he agreed with the *EPR* conclusions and considered the unsatisfactory situation as arising from the inability of 'the orthodox scheme [of quantum mechanics] to describe the separation process' of the two systems.

Einstein replied to Schrödinger's letter on 19 June, being 'very pleased' with this support. 'The real situation lies in the fact that physics is a kind of "metaphysics,"' he wrote, and further: 'Physics describes "reality," but we do not know what "reality" is, as we know it only through physical description!' The latter might be 'complete' or 'incomplete,' as he explained—leaving out the 'erudition' of Podolsky (who had redacted the paper but spoilt, in Einstein's opinion, the presentation of the argument)—in the example of two boxes having collapsible lids and a sphere which may be found by 'observation,' i.e., by opening the lid of a box:

> Now I describe a state as follows: *The probability to find the sphere in the first box is 1/2*. Is this a complete description? [Answer] *No*. A complete description is: the ball *is* in the first box (or it is not there). This must look like the characterization of a complete description. [Answer] *Yes*. Before I open the lid, the ball is not in either of the two boxes. Its being in a certain box comes about only by opening the lid. In this way, only the statistical character of the experienced world, or the empirical structure of its law (*Gesetzlichkeit*) arises. The state before opening [the lid] can be *completely* characterized by the number 1/2, whose meaning manifests itself in the process of observation only as a statistical statement. The statistics arises only by introducing insufficiently known factors, foreign to the system considered, through the observation. (Einstein to Schrödinger, 19 June 1935)

Einstein then argued that one might not be able to distinguish between the two conclusions mentioned above, unless one called for help upon an 'additional principle,' the 'principle of separability,' and stated explicitly: 'The second box plus everything concerning its contents is independent of what happens in the first box ([both are] separated partial systems).' Thus, 'if one sticks to the principle of separability, one excludes the second [he called it "Schrödinger-like"] interpretation and retains only the first ["Born's"], according to which the above description of the state, however, is an *incomplete* description of *reality*, or the real state, respectively.' (Einstein to Schrödinger, *loc. cit.*)

Einstein admitted that his example represented the quantum-mechanical situation only in an imperfect manner, although it stressed the 'essential feature' of whether the normalized wave function ψ can be uniquely associated with the real state of the system, and the statistical character of the results of measurement emerges exclusively from the process of measurement. If it were so, he would call the situation a complete description of reality by the theory; if not, it would be incomplete. Schrödinger responded to Einstein on 13 July: 'Your letter shows that I completely agree with you concerning the opinion about the *existing* theory ... I now take pleasure and use your note to challenge with it the most different, intelligent people: London, Teller, Born, Pauli, Szilard, Weyl.' That is, he had asked these colleagues (representing the orthodox viewpoint of quantum mechanics) personally (if available in Great Britain, e.g., London, Teller, and Born) or by letters (Pauli, Weyl) about their stand on this question. Now, Schrödinger reported in particular that the 'most relevant' answer came from Pauli, 'who at least admits that the use of the word "state" for the ψ-function is very suspicious (*anrüchig*).' (Schrödinger to Einstein, 13 July 1935, p. 1) Evidently, he referred to a letter, in which Pauli—though claiming that 'one cannot, as the old conservative gentlemen wish to do, declare the statistical statements of quantum mechanics (wave mechanics) as *correct* and *nevertheless* put a hidden causal mechanism behind it'—had (after explaining to Schrödinger Bohr's reply to the *EPR* argument) contemplated about the question whether a 'pure case' (described by a given wave function) might be called a 'state (*Zustand*)':

> A pure case [of a system] A represents a whole situation, in which the results of certain measurements at A (to the maximal extent) can be predicted with certainty. If one calls this a "state," I do not mind—but then it *does* follow that a change of the state A—i.e., of what is predictable about A—lies also, differently from the influence of a direct perturbation of A itself—i.e., also *after* the isolation of A—, in the *free choice* of the experimentalist. (Pauli to Schrödinger, 9 July 1935, in Pauli, 1985, p. 420)

'The great difficulty to reach an understanding with the orthodox people,' Schrödinger went on to write in his letter to Einstein on 13 July 1935, 'has induced me to try to attempt an analysis of the present situation of the interpretation *ab ovo* [i.e., from the very beginning]. Whether and what I shall publish of it, I do not know; but for me this is the best way to clarify matters for myself.' (Schrödinger to Einstein, 13 July 1935, p. 2)

In this analysis, he wrote to Einstein, several points in the current foundations of quantum mechanics occurred to him as 'strange (*komisch*).' The first such point was that the new quantum theory, which deviated so strongly from the previous one by the statements of indeterminacy, acausality, and many more specific ones, had not changed at all in one peculiar aspect, namely, the fact that: 'The only real thing in the world [of science], the result of the measurement, can be explained by it *only* totally classically just as the measurement of a property in a classical model.' Hence, differently from the situation in electrodynamics, where the new (Maxwell) theory had created new concepts (e.g., the field strengths, etc.) to be measured, in quantum mechanics, 'one so-to-say measures happily further (*angeblich lustig weiter*) the same concepts as before [in the classical theory], because supposedly our language is not able at all to grasp something else.' (Schrödinger to Einstein, *loc. cit.*) Even the totally new 'probability' statements in the quantum-mechanical calculation referred in Schrödinger's opinion just to classical concepts instead of dealing with the new properties of the atomic systems.[847] Second, to determine the obviously continuous ψ-function by a finite or discrete set of 'suitably chosen and ideally accurate measurements' appeared to be quite an unbelievable 'hocus-pocus' (Schrödinger to Einstein, *loc. cit.*, p. 3). Third, he strongly criticized a statement of Paul Dirac's, according to which 'canonical variables may have as eigenvalues all real numbers, from minus infinity to plus infinity' as being unbelievable and practically not verifiable by measurements. This point had been clearly noticed already by John von Neumann (in his 1932 book on the mathematical foundations of quantum mechanics) when he declared his own description of the quantum-mechanical measurement process as 'at least for the moment, the mathematically most practicable.' 'I believe that here our Johnny has already indicated sharply (*den Meißel angesetzt*) where a reformulation is needed,' Schrödinger commented and added:

> One had actually *lost* the classical model. One did not find a new one but hit upon the biggest difficulties opposing [the construction of] any model at all. Hence one says: Hey, we just retain the classical one, declare that all its properties are measurable in principle, and add in a wise, philosophical manner that these *measurements* represent the only reality, and everything else is metaphysics. Then the monstrosity of our statements *concerning the model* does not disturb us. We do have recanted it—and therefore we are allowed to use it all the more happily. The mistake [of this standpoint] is the following: *if* one wants to adopt this highly philosophical viewpoint, one must declare really feasible measurements, or idealizations of these, to be the "only reality." (Schrödinger to Einstein, *loc. cit.*, pp. 4–5)

Einstein replied to Schrödinger on 8 August 1935, and said: 'You are practically the only person with whom I like to argue, because all the other fellows (*Kerle*) do not view the theory from the facts but only view the facts from the theory; they cannot escape from the once adopted net of concepts but can only toss about it nicely (*possierlich darin herumzappeln*).' He immediately proceeded to stress the difference in their respective criticisms of the quantum-mechanical situation ('We represent the sharpest contrasts, he noted.'). While Einstein himself

[847] At this point, Schrödinger evidently forgot about the spin property, certainly a nonclassical concept.

preferred to have the ψ-function describe not the state of one system but an ensemble of systems, Schrödinger did consider ψ as *the* representation of reality (and he also wished, as we have discussed above, to abolish the connection with the concepts of ordinary mechanics). However, Schrödinger's interpretation would fail to describe the macroscopic experience, as Einstein now illustrated by the example of a pile of gun powder in a chemically labile state. Schrödinger disagreed: 'Since long I have left the stage behind me that the ψ-function can somehow be viewed as the description of reality,' he wrote back immediately and reported:

> In a longer essay, which I have just written, I discuss an example very similar to your exploding powder barrel. I just put the emphasis there to bring into play an uncertainty which according to our present understanding is really of the "Heisenberg type" and not of the "Boltzmann type." A Geiger counter is enclosed in a steel chamber connected with a tiny amount of uranium—so little that in the next hour *one* atomic decay is as probable as improbable. An amplifying relay makes sure that the first atomic decay crashes a little retort containing hydrocyanic acid. This and—cruelly—a cat are contained in the steel chamber. After an hour, then, in the ψ-function of the total system—*sit venia verbo* (excuse my words)—a living and dead cat are smeared out in equal parts. (Schrödinger to Einstein, 19 August 1935)

Although he did not follow the mathematical details of Schrödinger's letter, Einstein was quite pleased with the example of the cat, which showed 'that we agree completely with respect to the character of the present theory,' because:

> A ψ-function, in which a living and a dead cat enter [simultaneously], cannot just be considered to describe a real state. This example precisely hints at the fact that it is reasonable to attribute the ψ-function to a statistical ensemble, which embraces equally well a system with a living cat as well as a dead one. (Einstein to Schrödinger, 4 September 1935)

Schrödinger, on the other hand, had submitted his essay already around 12 August to the German journal *Naturwissenschaften*.[848] It was entitled '*Die gegenwärtige Situation in der Quantenmechanik* (The Present Status of Quantum Mechanics),' and Schrödinger organized in it in a quite detailed manner his ideas in 15 sections, which were published in three issues of the journal between 29 November and 13 December 1935 (Schrödinger, 1935a).

Schrödinger started his essay by explaining the nature of a 'classical model' with its 'determining characteristics (*Bestimmungsstücke*)'—i.e., the 'model con-

[848] Schrödinger had previously often published in this journal on various topics, including epistemological questions (1929a). As he wrote to Einstein on 19 August 1935, he had previously exchanged letters with Arnold Berliner, the long-time editor of *Naturwissenschaften*. Just recently, Berliner had informed him that he was fired as the editor and was only allowed to serve as an advisor, but he had requested Schrödinger still to send him papers for 'his' journal. Contrary to his prior intentions, Schrödinger let the paper appear in Germany—against Einstein's protest—as his last contribution until the end of the *Third Reich*. When Berliner was ordered years later to leave his home in Berlin, he committed suicide on 22 March 1942.

stants,' such as energy, momentum, angular momentum, etc., of which a complete set fixed the physical state of the model (§1). 'The turning point of today's quantum mechanics constitutes a dogma ... stating that models with characteristics which are determined uniquely, like the classical ones, do not correspond to nature,' Schrödinger stated at the beginning of §2 on 'The Statistics of Model Variables in Quantum Mechanics' (Schrödinger, 1935a, p. 808). The new models referred to the classical ones but emphasized restrictions—namely, the 'mutual determination'—in the following way: (i) The classical concept of state is lost, since at most half of the complete set of characteristics can be associated with fixed numerical values, while the others remain completely indeterminate (in certain cases, like the Rutherford atomic model, *all* of them appear to be uncertain, i.e., restricted by the indeterminacy relations). (ii) As not all the variables can be determined at a given instant of time, they won't be determined also at a later instant; hence, the principle of causality fails. That is, quantum mechanics replaces the causal relations by a particular statistics: It allows one to compute, from the maximal number of completely determined characteristics, the 'statistical distribution' of every variable at a given instant of time and at any later instants.

While the new theory thus declared the classical model as being unable to represent the *mutual connection of the characteristics* (*Bestimmungsstücke*)—thus renouncing the very reason why the model was invented—it assumed, on the other hand, that the classical model still remained a suitable tool to inform us as to which type of *measurements* can be carried out in principle on a given object in nature. 'This would seem to those who invented the picture [i.e., the classical model] as an unprecedented overstraining of their paradigm (*Denkmodell*), a frivolous anticipation of the future development,' Schrödinger concluded (Schrödinger, *loc. cit.*, p. 809).

The probability predictions of quantum mechanics, Schrödinger explained in the next section (§3), were quite sharp, even 'sharper than any real measurement could ever provide'; but the classical concepts (like angular momentum or energy) were used only 'to force the contents with some effort into the Spanish boots of a probability statement' or: 'According to the wording [of quantum mechanics], all statements refer to the classical model; but the valuable statements connected with it are little visualizable, and its visualizable characteristics possess only little value.' Thus:

> The classical model plays the role of *Proteus* in quantum mechanics. Each of its determining characteristics may, under suitable circumstances, become the object of interest and gain a certain reality; but all of them can never do so—once there are certain characteristics, next time there are others, especially at most always *half* of a complete set of dynamical variables provide a clear picture of the instantaneous state of the system under consideration. (Schrödinger, *loc. cit.*, p. 810)

The question now arose about the 'reality' of the other—the uncertain—variables, and Schrödinger discussed two alternatives. One alternative endowed all of them with reality but did not permit a simultaneous knowledge (of all), similar to the statistical description of molecular systems in the late nineteenth century (§4). Schrödinger then demonstrated in several examples of quantum-mechanical variables that they could not be described by 'ideal ensembles': 'At no instant of time, an aggregate of classical model states exists described by the ensemble of quantum-theoretical results.' (Schrödinger, *loc. cit.*, p. 811) The other alternative, namely, the assumption that the undetermined characteristics possessed no—or just a 'washed out (*verschwommene*)'—reality, seemed to be acceptable only at first inspection (§5). One may, of course, use the tool of the ψ-function to describe as clearly as in the classical case the degree of the 'washing-out' of all variables; however, 'serious doubts arise if one realizes that the indeterminacy seizes coarsely touchable and visible objects where the concept of washing-out simply turns out to be wrong' (Schrödinger, *loc. cit.*). For example, in dealing with the radioactive α-decay, it was possible to describe the interior of the atom by washed-out variables; yet the observation of the emitted α-rays revealed definite tracks in a Wilson cloud chamber or clear scintillation spots on a screen. 'One can even construct quite burlesque cases,' Schrödinger continued, such as:

> A cat is captured in a steel chamber together with the following infernal machinery (which one must protect from the direct grip of the cat): in a Geiger counter there exists a tiny amount of a radioactive substance, so little that in the course of an hour perhaps one atom decays, and with equal probability it does not decay; if it decays, the counter clicks and operates via a relay a small hammer such that it shatters a little retort containing hydrocynic acid. On leaving the system to itself for an hour, one may still say that the cat is still alive if no atom has decayed meanwhile; the very first decay would have poisoned it. Then the ψ-function of the total system would describe the situation by claiming that it contains the living and the dead cat mixed or smeared out in equal parts. (Schrödinger, *loc. cit.*, p. 812)

The typical feature of such examples was that an indeterminacy in the atomic domain caused an indeterminacy which might be sensed macroscopically (or '*grob sinnlich*'); this fact 'hinders us in accepting in such a naive manner a "washed-out model" as a picture of reality,' Schrödinger said in conclusion of §5 of his essay.

As the lesson to be derived, Schrödinger opened §6 on 'The Conscious Change of the Epistemological Point of View;' one could adopt the ruling dogma of the quantum theorists, namely:

> One tells us that no difference has to be made between the real state of an object of nature and what I know about it, or better, what I can learn to know about it with [all] efforts. One says that only perception, observation, measurement are actually *real*. Thus, once I have obtained at a given instant the best possible knowledge about the state of the physical object that can be achieved according to the laws of nature, I may refute any question about the "real state" which goes further as *lacking in sense* (*gegenstandslos*), if I am convinced that no additional observation can enlarge my knowledge—at least not without reducing it by the same amount (namely, by changing the state). (Schrödinger, *loc. cit.*, p. 823)

Consequently, only observations had to be considered real, and all our physical cognition was based on measurements that might be performed in principle, or it was the theory which determined where nature posed the 'ignorabimus limit'—i.e., the limit beyond which we can never proceed to know. However, Schrödinger did not like this limitation really and therefore went on to analyze the quantum-mechanical situation and to suggest ways out of the ruling dogma.

The ψ-function, he argued in §7, acts as 'a catalogue of expectation.' Quantum mechanics told us that, although its time-evolution occurs by a partial differential equation according to the 'classical causal model,' any measurement causes 'a peculiar, rather sudden change,' 'a break with the naive realism' and the causal law. Consequently, a quantum-mechanical theory of measurement (§8) arose stating: 'A variable possesses in general no determined value before I measure it,' or 'the [act of] measuring does *not* mean to determine the value which it *possesses*,' but rather:

> *An interaction between two systems* [*called*] *measured object and measuring device, achieved on a given plan, is called measurement of the first system if the value of a directly perceptible variable property of the second system* (*a pointer position*) *in an immediate repetition of the process* (*with the same measured object which should not be affected meanwhile by other influences*) *will always be reproduced within certain limits of error.* (Schrödinger, *loc. cit.*, p. 824)

Therefore, in quantum mechanics, one had to distinguish between two types of statistics, the error statistics of the measurement and the theoretically predicted statistics.

The ψ-function evidently described the state of a system insofar as 'different ψ-functions denote different states' and 'the same ψ-function describes the same state of the system,' Schrödinger noted in § 9 (Schrödinger, *loc. cit.*, p. 825). Now, he tried to construct (in §10) a new theory of measurement, based on an 'objective description of the interaction between the measured object and the measuring instruments' (Schrödinger, *loc. cit.*, p. 826). He then noted the result (already reported above as the consequence of Heisenberg's unpublished manuscript): 'The best possible knowledge of the whole [system] does not necessarily imply the same knowledge about its parts.' Hence, he concluded the '*insufficiency of the ψ-function as a substitute of the model*' (Schrödinger, *loc. cit.*, p. 827). Instead, the following result was obtained for the observed object: '*An organized catalogue of expected data of the object has been split into a conditional disjunction of catalogues of expectation values*. (*Der Erwartungskatalog des Objektes hat sich in eine konditionale Disjunktion von Erwartungskatalogen aufgespalten*.') (Schrödinger, *loc. cit.*) Finally, Schrödinger concluded that before one *inspects* the result of the measurement, the discontinuous jump characterizing quantum mechanics occurs: The original ψ-function then disappears and a new one reappears (connected with the former by a discontinuous change). Actually, the interaction between two systems (or bodies) 'correlates (*entangles*)' the expectation catalogues of data of the individual systems, as he found in §11. Then, in §12, he discussed the *EPR* case, which he generalized in the next section §13 by considering—besides the measurement of momentum and position—also that of the other variables, such as $p^2 + q^2$ or $p^2 + a^2q^2$ (with a an arbitrary positive constant). He arrived at the unsatisfactory conclusion:

> But how the numerical values of all these variables of *one* system are mutually connected, we do not know at all, though the system must possess for each of them a quite definite readiness [or acceptability], because we may, if we wish, get to know it [i.e., the numerical value] exactly at the auxiliary [i.e., second] system and always find it substantiated by direct measurement. (Schrödinger, *loc. cit.*, p. 846–847)

Evidently, this situation—where one does not know about the relations between the values of the variables—did not exist in classical mechanics. But quantum mechanics still exhibited another peculiarity, which Schrödinger discussed in §14: The correlations or entanglements of the system are connected with a 'sharply defined time.' Such a distinction of time, however, seemed to Schrödinger to be quite inconsistent, because 'the numerical value (*Maßzahl*) of time is like that of every other variable the result of an observation' and one may ask the question why 'one is permitted to attribute to the measurement with a clock an exceptional position' (Schrödinger, *loc. cit.*, p. 848). The exceptional role of the time measurement would especially create difficulties with the relativistic formulation of quantum mechanics (§15). He finally wrote at the end of his comprehensive analysis:

> Perhaps the simple procedure which the nonrelativistic [Schrödinger called it "unrelative"] theory possesses [for describing the quantum-mechanical correlations] is as yet only a comfortable trick which has however obtained, as we have seen, an immensely large influence on our fundamental view towards nature. (Schrödinger, *loc. cit.*, p. 849)

Although Schrödinger indicated certain hints as to how relativistic quantum mechanics might eventually change the situation again, he was not really able to offer a solution of the problem of interpretation; still, he hoped that the situation presented by quantum mechanics would not be the final word in this question.

(e) Reality and the Quantum-Mechanical Description (1935–1936)

The responses in the scientific literature following the articles of Einstein, Podolsky, and Rosen (1935), Bohr (1935b), and Schrödinger (1935a) showed mainly that these authors followed, as Schrödinger would say, the usual '*Lehrmeinung* (dogma).' Thus, Wendell Hinkle Furry of Harvard University, in a 'Note on the Quantum-Mechanical Theory of Measurement,' submitted in November 1935, analyzed more general examples than the one treated by *EPR* with the methods of measurement theory (Furry, 1936a). He put his finger on the point where *EPR* and the orthodox quantum theorists—represented especially by Heisenberg, von Neumann, and Pauli—disagreed: The former assumed the interaction to exist only at an instant of time and then applied the usual probability evaluation (method A); the latter, however, made use of the full quantum-mechanical formalism (method B). By investigating the position and momentum measurement of a heavy atomic particle (say, a proton) by the process of scattering it with a lighter one (say, an electron), Furry concluded that 'assumption A is consistent with quantum-mechanics,' especially:

> Both by mathematical arguments and by discussion of a conceptual experiment, we have seen that the assumption that a system when free from mechanical interference necessarily has independently real properties is contradicted by quantum mechanics. This conclusion means that a system and the means used to observe it are to be regarded as related in a more subtle and intimate way than was assumed in classical theory. (Furry, *loc. cit.*, p. 399)

In a later letter, dated 2 March 1936, and published soon afterward also in *Physical Review*, Furry addressed Schrödinger's examples and his discussion of measurement theory and rejected the latter (Furry, 1936b).[849]

Thus, among the quantum physicists, Einstein and Schrödinger appeared indeed to be 'lone wolves' who defended epistemological views that deviated from those of the community of experts in modern atomic theory. But to what amounted their views which they had expressed in the discussion of their *Gedanken*experiments discussed above? Many analyses of the science-theoretical and cognition-theoretical contents have been published since 1935, especially in the decades after 1950 when the subject of quantum-theoretical interpretation would receive renewed interest by the stimulating efforts of David Bohm and others.[850] The main idea brought into play in 1935 seems to have been what Einstein and Schrödinger called 'realism.' Toward the end of his life, Einstein characterized it in a letter as follows:

> It is basic for physics that one assumes a *real world independently of any act of perception* [our italics]. But this we do not *know*. We take it only as a programme in

[849] Henry Margenau of Yale University and Hugh C. Wolfe of the City College of New York arrived at similar conclusions in contributions submitted in November and December 1935 to the *Physical Review* (Margenau, 1936; Wolfe, 1936). Margenau especially wanted to abolish a postulate usually assumed in quantum mechanics, i.e.: 'When a measurement is performed on a physical system, then immediately after the measurement the state of the system is known with certainty.' (Margenau, 1936, p. 241) He claimed that it was unnecessary to assume this. Einstein, to whom Margenau sent a copy of the manuscript, pointed out 'that the formalism of quantum mechanics requires inevitably the postulate: "If a measurement performed upon a system yields a value *m*, then the same measurement performed immediately afterwards yields again the value *m* with certainty."' (Margenau, 1958, p. 29) This exchange with Einstein entered into Margenau's later paper (Margenau, 1937), where he distinguished between 'state preparation' and 'measurement' (see Jammer, 1974, p. 224 ff.).

[850] We shall return to this discussion later in the Epilogue. Here, we just wish to refer to two quite detailed accounts of the problem of which we have made some use below, namely, Max Jammer's book *The Philosophy of Quantum Mechanics* (1974, Chapter 6, pp. 181–251) and Arthur Fine's book *The Shaky Game: Einstein's Realism and the Quantum Theory* (1986; second edition, 1996).

> our scientific endeavours. This programme is, of course, prescientific and our ordinary language is already based on it. (Einstein to M. Laserna, 8 January 1955; quoted in Fine, 1986, p. 95)

Einstein had addressed the connection between physics and the ‘real world’ quite early in the quantum-mechanical discussion, but he placed the first detailed statements on this issue in a paper entitled ‘*Physik und Realität* (Physics and Reality),’ which appeared in print in the March 1936 issue of the *Journal of the Franklin Institute* (Einstein, 1936). In this article, Einstein also amended certain formulations of the *EPR*-paper and explained the proper meaning of its contents.[851]

In Einstein’s opinion, science was a refinement of everyday thinking of the ‘real external world’; while the latter rests exclusively on the sense impressions, science must proceed further in the ‘setting of a “real world.”’ He wrote:

> The first step is the formation of the concept of bodily objects of various kinds.... The second step is to be found in the fact that, in our thinking (which determines our expectation), we attribute to this concept of the bodily object a significance, which is to a high degree independent of the sense impression which originally gives rise to it. This is what we mean when we attribute to the bodily object “a real existence.” The justification of such a setting rests exclusively on the fact that, by means of such concepts and mental relations between them, we are able to orient ourselves in the labyrinth of sense impressions. (Einstein, *loc. cit.*; English translation, pp. 349–350)

Having established the criterion of a ‘real external world,’ Einstein demanded ‘its comprehensibility’ by assuming the existence of relations between the concepts: such special relations, namely, the theorems expressing ‘statements about reality’ constituted the laws of nature (Einstein, *loc. cit.*, p. 352). ‘Science concerns the totality of primary concepts, i.e., concepts directly connected with sense experiences, and theorems connecting them,’ Einstein continued, and added: ‘The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim by *the use of a minimum of primary concepts*.’ (Einstein, *loc. cit.*) He then talked about several stages in the development of science: The ‘first layer’ retains the primary concepts and relations; a ‘secondary system’ also involves concepts of the ‘secondary layer’ which are not directly connected with sense experiences, but it is logically more complete, as it possesses a ‘higher logical unity.’ ‘Thus the story goes on until we have arrived at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which are still compatible with the observations made by our own senses,’ Einstein concluded these general historical comments and stated: ‘We do not know whether or not this ambition will ever result in a definite system.’ (Einstein, *loc. cit.*, p. 353) He rather thought that the answer was negative, ‘however,

[851] Max Jammer called this paper ‘Einstein’s *credo* concerning the philosophy of physics’ (Jammer, 1974, p. 230).

one will never give up the hope that this greatest of all aims can really be attained to a very high degree' (Einstein, *loc. cit.*).

With this background preparation, Einstein proceeded 'to demonstrate what paths the constructive human mind has entered, in order to arrive at a basis of physics which is logically as unified as possible' (Einstein, *loc. cit.*, p. 354). Thus, he first discussed in some detail 'mechanics and the attempts to base all physics on it' in §2 of the paper, 'the field concept' of electrodynamics (in §3), and the theory of relativity (in §4). In §5, he turned to 'quantum theory and the fundamentals of physics,' which he introduced by saying:

> The theoretical physicists of our generation are expecting the erection of a new theoretical basis of physics which would make use of fundamental concepts greatly different from those of the field theory considered up to now. The reason is that it has been found necessary to use—for the mathematical representation of the so-called quantum phenomena—new sorts of methods of consideration. (Einstein, *loc. cit.*, p. 371)

Einstein then outlined what he considered to be the essence of wave mechanics (emphasizing limiting connections with classical mechanics) and stressed its wide application to 'such a heterogeneous group of phenomena of experience' and added:

> In spite of this, however, I believe that the theory is up to beguile us into error in our search for a uniform basis for physics, because, in my belief, it is an *incomplete* representation of real things although it is the only one which can be built out of the fundamental concepts of force and material points (quantum corrections to classical laws). The incompleteness of the representation is the outcome of the statistical nature (incompleteness) of the laws. (Einstein, *loc. cit.*, 374)

Einstein supported his opinion concerning the incomplete representation of quantum theory by asking the particular question whether the ψ-function describes 'a real condition of a mechanical system' (Einstein, *loc. cit.*). For that purpose, he selected a periodic system which, according to quantum mechanics, possessed discrete energy states E_1, E_2, etc. Now, if the system in the lowest state (E_1) were perturbed during a finite time by a small force, the wave function could be written as

$$\psi = \sum_{r=1}^{\infty} c_r \psi_r, \tag{659}$$

with $|c_1|$ being nearly unity and $|c_2|$, $|c_3|$, etc., very small quantities. But, he argued, that ψ cannot 'describe a real condition of the system,' because this should have an energy exceeding E_1 by a small amount; hence, it would lie between E_1 and E_2, which is excluded by quantum theory. 'Our -function ... represents rather a statistical description in which the c_r represent probabilities of the individual energy values,' Einstein continued and suggested: 'The ψ-function does not in any way describe a condition which could be that of a single system; it relates rather to many systems, to "an ensemble of systems" in the sense of statistical mechanics.' (Einstein, *loc. cit.*, p. 375) In Einstein's opinion, such an interpretation removed the 'paradox recently demonstrated by myself and two collaborators,' but 'what happens to a single system remains ... entirely eliminated from the representation by the statistical manner of consideration' (Einstein, *loc. cit.*, pp. 376–377). 'But now I ask,' he continued:

> Is there really any physicist who believes that we shall ever get an inside view of these important alterations in the single systems, in their structure and their causal connections, and this regardless of the fact that these single happenings have been brought so close to us, thanks to the inventions of the Wilson [cloud] chamber and the Geiger counter? To believe this is logically possible without contradiction; but it is so very contrary to my scientific instinct that I cannot forego to search for a more complete conception. (Einstein, *loc. cit.*)

Quantum mechanics, he admitted, 'has seized hold of a beautiful element of truth,' and he did not doubt 'that it will be a test stone for any future theoretical basis.' 'However, I do not believe that quantum mechanics will be the *starting point* in the search for this basis,' as it seemed to Einstein 'entirely justifiable seriously to consider the question as to whether the basis of all field physics cannot by *any* means be put into harmony with the facts of quantum theory' (Einstein, *loc. cit.*, p. 378).[852]

Like Einstein in America, Schrödinger in England also continued to think about the interpretation of quantum mechanics beyond his essay to the *Naturwissenschaften*. In two papers, sent in August 1935 and April 1936 (communicated by Max Born and Paul Dirac, respectively) to the *Proceedings of the Cambridge Philosophical Society*, he investigated the 'probability relations between separated systems,' which the *EPR*-paper had shown to constitute a central point at which the classical and the quantum-mechanical treatments differed (Schrödinger, 1935b; 1936). Indeed, Schrödinger called 'the characteristic trait of quantum mechanics, the one that enforces its entire departure from the classical lines of thought,' the following:

> When two systems, of which we know the states by their respective representatives, enter into temporary physical interaction due to known forces between them, and when after a time of mutual influence the systems separate again, and then they cannot any longer be described in the same way as before, viz. by endowing each of them with a representative of its own. (Schrödinger, 1935b, p. 555)

That is, the ψ-function describing the two systems became entangled by the interaction such that afterward only an experiment, rather than any previous knowl-

[852] The suggestions made by Einstein in the direction of bringing 'the basis of field physics into harmony with the facts of quantum theory' in §6 (entitled 'Relativity Theory and Corpuscles') did not go beyond some indication of how to obtain a singularity-free representation of electric corpuscles.

edge of the states can disentangle them; however, this measurement then also exhibited the strange features revealed by the *EPR*-analysis. In particular, Schrödinger showed that the 'paradox' stated by Einstein, Podolsky, and Rosen was 'the rule and not the exception' in quantum mechanics by proving a general mathematical theorem: The function $\Psi(x, y)$ representing the state of the composite system after the two subsystems have separated again is not a product of two functions containing only the variables x and y of the individual systems separately. However, if one performs a measurement on the second system yielding the state $f_n(y)$, then $\Psi(x, y)$ becomes

$$\Psi(x, y) = \sum_n c_n g_n(x) f_n(y), \tag{660}$$

with the function $g_k(x)$ and the probability coefficient c_k determined by the equations

$$\int g_k^*(x) g_k(x)\, dx = 1 \tag{661}$$

and

$$c_k g_k(x) = \int f_k^*(y) \Psi(x, y)\, dy. \tag{662}$$

In general, the $g_k(x)$ thus obtained will not be orthogonal to each other, but under suitable conditions for the f_k, namely, that they satisfy the homogeneous linear integral equation,

$$f(y) = \lambda \int K(y, y') f(y')\, dy', \tag{663}$$

with the eigenvalue λ and the Hermitean kernel $K(y, y')$,

$$K(y, y') = \int \Psi^*(x, y') \Psi(x, y)\, dx, \tag{663a}$$

they will be orthogonal.

The 'biorthogonal development' of $\Psi(x, y)$ due to Eq. (660) then provided Schrödinger the 'true insight' into the difficult problem of quantum-mechanical entanglement, as he found:

> If there are no coincidences among the $|c_k|^2$ (excluding also the case that more than one of them vanish) the relevant f_k's form a well determined and complete set and so do the g_k's. Then one can say that the entanglement consists in that one and only one variable (or set of commuting variables) of one system is uniquely determined by a

> definite observable (or set of observables) of the other system. This is the general case. We shall now turn to the opposite extreme, which is the Einstein-Podolsky-Rosen case. It could be characterized by *all* $|c_k|^2$ being equal and *all* possible developments being biorthogonal. *Every* observable (or set, etc.) of one system is determined by an observable (or set, etc.) of the other one. (Schrödinger, *loc. cit.*, p. 558)

Since a difficulty arose with normalizing the sum $|c_k|^2$ to unity in the latter, degenerate case, Schrödinger chose a different procedure there. He considered two systems, denoted by position and momentum variables x_1, p_1 and x_2, p_2, and observed that the following variables x and p of the total system Ψ,

$$x = x_1 - x_2 \quad \text{and} \quad p = p_1 + p_2, \tag{664}$$

commuted; hence, they satisfied

$$x\Psi = x'\Psi \quad \text{and} \quad p\Psi = p'\Psi, \tag{665}$$

with eigenvalues x' and p', respectively. Consequently, the value of the variable x_1, namely, x_1', could be deduced from measuring the value x_2' of the variable x_2 and, similarly, the value p_1' from p_2'. Further, Schrödinger claimed that more knowledge might be obtained about system 1 from measurements in system 2, say, from the measurement of energy or any other variable. Thus, 'the *two families* of observables, relating to the first and the second system, respectively, are linked by at least *one* match between two definite members, one of either family,' where 'the word *match* is short of stating that the *values* of the two observables in question determine each other uniquely and therefore (since the actual labelling is irrelevant) can be taken to be equal' (Schrödinger, *loc. cit.*, p. 563).

In his second paper on the probability relations between separated systems, Schrödinger tried to avoid the 'match' linking the two families of observables of systems 1 and 2, i.e., the conclusion that 'the experimenter even with the indirect method, which avoids touching the system [1] itself, controls its future state in very much the same way as it is well known in the case of direct measurement.' (Schrödinger, 1936, p. 446) This match, which Einstein and he had demonstrated as characterizing the standard quantum mechanics, seemed to him to be the greatest hindrance toward a more satisfactory theory describing what both (he and Einstein) meant by physical reality. To achieve this purpose, Schrödinger now became involved in a detailed discussion of quantum-mechanical mixtures, obtaining the result 'that *in general* a sophisticated experimenter can, by a suitable device which does also involve measuring non-commuting variables, produce a non-vanishing probability of driving the system into any state he chooses, whereas with the ordinary direct measurement at least the states orthogonal to the original ones are excluded' (Schrödinger, *loc. cit.*). In particular, he described the case of two systems as a special example of a mixture, and after the corresponding calculation was performed, he concluded:

> If the wave function of the whole system is known, either part is in the situation of a mixture, which is decomposed into *definite* constituents by a *definite* measuring programme to be carried out on the *other* part. All the conceivable decompositions (into linearly independent constituents) of the first system are just realized by all measuring programmes that can be carried out on the second one. *In general every* state of the first system can be given a finite chance by a suitable choice of the programme. (Schrödinger, *loc. cit.*, p. 452)

In fact, Schrödinger hoped to eliminate the experimenter's influence on the state of the system which he does not measure by an additional assumption, notably, by assuming:

> that the knowledge of the precise relation between the complex constants c_k [occurring in the wave function $\Psi(x, y)$ of the combined systems according to Eq. (660)] has been entirely lost in consequence of the process of separation. This would mean that not only the parts, but the whole system, would be in the situation of a mixture, not a pure state. It would not preclude the possibility of determining the state of the first system by suitable measurements of the second one or *vice versa*. But it would utterly eliminate the experimenter's influence on the state of that system which he does not to touch. (Schrödinger, *loc. cit.*, p. 451)

Schrödinger agreed that the description thus proposed was 'very incomplete,' but he called it 'a possible one, until I am told either why it is devoid of meaning or with which experiment it disagrees' (Schrödinger, *loc. cit.*, pp. 451–452). For the moment, he remained convinced that the conclusions 'unavoidable within the present theory but repugnant to some physicists including the author, are caused by applying nonrelativistic quantum mechanics beyond its legitimate range' (Schrödinger, *loc. cit.*, p. 452).[853]

Having reported about the efforts of the 'conservative' Einstein–Schrödinger camp, let us now shift to the opposite camp and report about the further development of the arguments, especially those of Niels Bohr and Werner Heisenberg. In the analysis of Bohr's reply to the *EPR*-argument, Mara Beller and Arthur Fine have emphasized the fact that Niels Bohr had turned around the original complaint—that quantum mechanics was incomplete because it did not endow the two quantities, position and momentum, with equal reality—and rather argued that this 'deficiency' spoke in favour of the consistency and theoretical soundness of the new quantum theory; in particular, they claimed that Bohr had overlooked two extra assumptions made by Einstein, Podolsky, and Rosen, namely, first, 'that the same "reality" pertains to the unmeasured component [i.e., variable] of the two-particle systems,' and second, the assumption of 'a principle of separation according to which, after the two particles are far enough apart, the measurement of particle 1 does not effect the reality that pertains to particle 2' (Beller and Fine,

[853] As Jammer has pointed out, the study of Furry (1935a) proceeded along with involving much the same mathematical steps as Schrödinger used in his paper (1936), though he arrived at rather different conclusions.

1994, p. 8).[854] Was Bohr only following a positivistic attitude when he tried to work out ambiguities in the arguments of his opponents and thus hoped to persuade them about his own standpoint, and did he even apply the 'improper' assumption and interpretation of the *EPR*-arguments (as Beller and Fine claimed)? The simplest historically substantiated answer is Yes, to a certain extent Bohr accepted positivistic arguments, as the brief correspondence between him and Philipp Frank (quoted by Beller and Fine, *loc. cit.*, pp. 19–20) showed. But one can also easily notice differences in the opinions between Bohr, the author of complementarity, and Frank, the positivist from Prague, who presented their respective views quite clearly at the 'Second International Congress for the Unity of Science (*Zweiter Internationaler Kongress für Einheit der Wissenschaft*),' held in Copenhagen from 21 to 26 June 1936, where both spoke on the same day (22 June). Bohr's talk, entitled '*Kausalität und Komplementarität* (Causality and Complementarity)' addressed the problem in quite general terms (Bohr, 1937a). He argued that the mathematical formalism of quantum mechanics allowed an unambiguous representation of the experimental facts but did not admit the classical causal representation of the quantum phenomena; rather:

> The renunciation of the causal ideal in atomic physics is founded conceptually alone on the fact that we were not able, because of the inevitable interaction between experimental objects and measuring devices ... anymore to talk about the independent behaviour of a physical object. Finally, an artificial word like "complementarity" which does not belong to the concepts of daily life and therefore cannot be attributed any visualizable content with the help of the usual concepts, just serves to remind us of the completely new epistemological situation in physics. (Bohr, *loc. cit.*, p. 298)

Frank, on the other hand, spoke explicitly on '*Philosophische Deutungen und Mißdeutungen der Quantentheorie* (Philosophical Interpretations and Misinterpretations of Quantum Theory),' (Frank, 1937). He especially identified as 'the essential misinterpretation' what he called 'the passage through the "real" metaphysical world (*Durchgang durch die "reale" metaphysische Welt*).' (Frank, *loc. cit.*, p. 306). By analyzing the situation in quantum mechanics, he identified several misinterpretations as arising from the use of classical concepts to describe atomic phenomena and stated:

> Quantum mechanics talks neither about particles, whose position and velocity exist but cannot be observed accurately, nor about particles with indefinite position and velocity, but about measuring devices, in the description of which the phrases "position of a particle" and "velocity of a particle" cannot be used simultaneously ... Measuring devices, of which one is described by the expression "position of a particle" and the other by "velocity," or more accurately "momentum," are called *complementary* descriptions.

[854] Beller and Fine referred to the fact that Furry (1936a) had noticed these extra assumptions and had answered them properly in Bohr's sense.

> If one sticks to this terminology, one will never run tinto the danger of a metaphysical conception of the physical complementarity. Because here it is evident that nothing has been said about a "real world," neither about its nature, nor about the possibility to recognize it. (Frank, *loc. cit.*, pp. 308–309)

In their lectures at Copenhagen in 1936, Bohr and Frank did not talk explicitly about positivism, although Frank clearly stated a positivistic formulation of the principle of complementarity while Bohr said nothing of that sort. The same cautious use of philosophical doctrines was made in the manuscript communicated by Moritz Schlick on '*Quantentheorie und Erkennbarkeit der Natur* (Quantum Theory and the Perceptibility of Nature,' which was read after his sudden death at the Congress (Schlick, 1937). Schlick, the founder of the Vienna Circle, especially emphasized in his last contribution the fact that the restriction enforced by the new theory on the classical concepts like 'position' and 'momentum,' and also on the causal description of natural phenomena, or on the physical description of biological phenomena, would not lead to a limitation in principle of our cognition of nature. 'The whole question constitutes a beautiful example of the important fundamental theorem of the consequential empiricism as represented by the Vienna school, namely that nothing in the world cannot be recognized in principle,' he concluded, and added:

> There exist, though, many questions which may never be answered because of practical, technical reasons; however, in principle, a problem does not yield a solution in just a single case, namely that it's no problem at all, i.e., one is dealing with a wrongly posed question. The limit of cognition exists where there is nothing which can be grasped. Where quantum theory places a limit on causal experience, it does not mean that the further, still existing laws must remain unknown; it rather means that further laws do not exist and cannot be found, because the quest for them would make no sense. (Schlick, *loc. cit.*, p. 326)

Bohr would return to the problem of reality and completeness of quantum mechanics also in later years, especially in the short address on 'The Causal Problem in Atomic Physics,' which he delivered at the *Conference of the Institute of Intellectual Cooperation*, held from 30 May to 3 June 1938, in Warsaw (Bohr, 1939a). On that occasion, as on previous and later ones, he embedded the topic deeply into his views on complementarity, as he replied to a remark of John von Neumann in the discussion of his talk—namely, that these views might be elegantly phrased in the language of formal logic:[855]

> We must also notice that the question of the logical forms which are best adapted to quantum theory is in fact a practical problem, concerned with the choice of the most

[855] A logical formulation, different from the one given by von Neumann, was given in 1936 by Max Strauß of Berlin (1936a, b).

> convenient manner in which to express the new situation that arises in this domain. [Personally he (Bohr) compelled himself] to keep the logical forms of daily life to which actual experiments were necessarily confined. The aim of the idea of complementarity was to allow of keeping the logical forms while procuring the extension necessary for including the new situation relative to the problem of observation in atomic physics. (Bohr, *loc. cit.*, pp. 38–39)

Bohr may have argued that the same applies to any philosophical doctrine, be it positivism or realism: It was fine as long as it accounted appropriately for the empirical facts. But Einstein's realism would not do, though he repeated it at various times:

> 1930: Physics is an attempt at the conceptual construction of a model of the *real* world, as well as its lawful structure.
> 1940: Some physicists, among them myself, cannot believe that we must abandon, actually and forever, the idea of direct representation of physical reality, in space and time; or that we must accept the view that events in nature are analogous to a game.
> 1950: Summing up we may characterize the framework of physical thinking ... as follows: There exists a physical reality independent of substantiation and perception. It can be completely comprehended by a theoretical construction which describes phenomena in space and time.... The laws of nature imply complete causality.... Will this *credo* survive forever? It seems to me that a smile is the best answer. (See Fine, 1986, p. 97, for the selection of quotations from Einstein)

Again and again the 'modern' quantum theorists would argue with Einstein about it, and we shall return to some aspects of these discussions in the Epilogue.

In the second half of the 1930's, Heisenberg did not seem to be involved in any arguments with Einstein and Schrödinger on the principles of the physical interpretation of quantum mechanics. In fact, he had to survive far less intellectual than rather serious political attacks on his own person and defend simultaneously all modern theories, not just his own, against some dangerous enemies in Germany. On 13 December 1935, Johannes Stark spoke at the inauguration ceremony of the '*Philipp Lenard Institut*' at the University of Heidelberg. Stark, the previous pioneer of quantum physics and Nobel laureate of physics in 1919, strongly criticized 'the conception and methods of the "Einstein physics" which are most widely spread in Germany,' and stated explicitly:

> Upon the sensation and advertisement of Einstein's relativity then followed the matrix theory of Heisenberg and the so-called wave mechanics of Schrödinger, one as obscure as the other. (See Menzel, 1936, p. 27)

Stark denounced these theories as 'Jewish' or 'degenerate' physics, in contrast to what his political ally Lenard called 'German' or 'Aryan' physics and would define in his programmatic book *Deutsche Physik* only vaguely opposed to the 'peculiar physics of the Jews,' characterized by its 'internationalism' and lack of 'understanding of truth' (Lenard, 1936, pp. ix–x). Both, the retired old professor Lenard and the younger, active president of the *Physikalisch-Technische Reichsanstalt* Stark, continued to conduct a malicious battle especially against Heisenberg. In particular, Stark influenced (and probably wrote himself) in July 1937 the article '"*Weiße Juden*" *in der Physik* ("White Jews" in Physics)' in the newspaper *Das Schwarze Korps* of the powerful Nazi organization SS (Stark, 1937). After calling the already accomplished expulsion of Jewish scientists from Germany just a 'partial victory,' the article demanded to attack those scientists who were not racially but intellectually Jews, naming Heisenberg '*Statthalter des* "*einsteinschen Geistes*" (Keeper of the Einstein Spirit) ... who, like Jews, had to disappear.' The theories which Stark wished to condemn, he described less polemically in a contribution to the *Physikalische Zeitschrift* (of which he was then an editor), entitled '*Widerspruch zwischen Erfahrung und dogmatischer Atomtheorie* (Contradiction between Experience and Dogmatic Atomic Theory)':

> If I criticize in the following the dogmatic atomic theory, I wish to refute first of all ... the accusation that I am hostile toward any theory. On the contrary, I respect greatly the realistic (*wirklichkeitgetretene*) theories which, like Maxwell's theory, represent the results of experimental research in the exact language of mathematics; similarly the theories which apply experimentally substantiated laws to special cases, as it happens in the elasticity theory and hydrodynamics. I am, however, suspicious toward dogmatic theories which build whole schemes of mathematical formulae upon not proven or arbitrary assumptions, and I reject such dogmatic theories emphatically which contradict experiences or do not represent them completely. The latter situation pertains to modern dogmatic atomic and quantum theories. (Stark, 1938, p. 190)

For a man like Heisenberg who did not wish to flee his native country and rather hoped to continue to do research in and teaching of modern physical theories in Germany, these open political attacks were extremely dangerous. He tried to obtain the assistance of colleagues, and he underwent extremely difficult interrogation in the main quarter of the frightful political police (the *Gestapo*) of the *Third Reich*. These attacks ended when Heinrich Himmler, the chief of the SS, wrote a letter to Heisenberg on 21 July 1938, informing him that he (Himmler) disapproved of the article in *Das Schwarze Korps*; and on the very same day, Himmler sent another letter to his deputy Reinhard Heydrich stating 'that one cannot afford to kill [!] Heisenberg.'[856] In this tense situation, Heisenberg could not, and would not, argue anymore with scientific opponents like Einstein, Planck, and Schrödinger, about details of the interpretation of quantum mechanics. In Germany, at least, the whole state of modern physical theories was at stake, and Heisenberg was glad to get published—though with considerable delay—an article, composed in 1940 and dealing with his positive evaluation of modern

[856] For details, see Beyerchen, 1977, Chapter 8, and Rechenberg, 1992.

theoretical physics, in the official journal of the *Reichsdozentenbund* in which he especially emphasized that:

> Relativity and quantum theory have so far been substantiated by experience. Since they are at the moment the only theories which provide precise statements about the most recent fields of physics—atomic physics, nuclear physics, cosmic-ray physics, etc.—they will also continue to remain as the basis for research in these fields, especially so long as a contradiction might show up between these theories and experience. The scientific battle against these theories could only be conducted by proving such contradictions experimentally. Philosophical essays do not change anything and tell [us] nothing about facts, hence they do not contribute to the decision about the usefulness of theories. Arguments different from scientific ones or scientific methods in a scientific conflict are not consistent with the dignity of German research. (Heisenberg, 1943a, p. 212)

By then the climate of scientific discussion had stabilized to the point that Heisenberg had to be satisfied to publish these obvious remarks in favour of the 'obscure, dogmatic atomic theories.' The whole episode characterized quite clearly the decline of physics due to the actions of the government of the *Third Reich*.

IV.3 New Elementary Particles in Nuclear and Cosmic-Ray Physics (1929–1937)

(a) Introduction: 'Pure Theory' Versus 'Experiment *and* Theory'

In looking back on his life as a physicist, Victor F. Weisskopf complained at the Erice Summer School in 1971 that, when he began his studies in physics at the University of Vienna, he came upon the scene three years too late:

> I came to the university in 1926 after quantum mechanics was invented, and, of course, I needed a few years to learn physics. That meant that I could not start active work before 1929–1930, and all the fundamental developments in quantum mechanics were made between 1925 and 1930. . . .
>
> Those fellows [of the previous years] such as [Hans] Bethe, [Rudolf] Peierls, [Felix] Bloch, and [Walter] Heitler were lucky. Every Ph.D. thesis at that time opened a new field. Peierls worked on heat conduction and opened one part of solid-state physics. Bethe wrote his Ph.D. paper on electron diffraction of crystals and opened up another part of solid-state physics. [Walter] Heitler and [Fritz] London opened up quantum chemistry, [Gregor] Wentzel the theory of the photoeffect. (Weisskopf, 1972, pp. 1 and 4)

And yet, even Weisskopf had quite a satisfactory start as a productive physicist, because he could work on his doctoral thesis beginning in 1928 at the University of Göttingen as a member of Max Born's famous Institute of Theoretical Physics, with brilliant young scholars like Pascual Jordan, Walter Heitler, Gerhard Herzberg, and Eugene Wigner around him.[857] It was Wigner who provided him important guidance in his first steps in research. As Weisskopf recalled:

> I was especially interested in the question of radiation damping, the natural width of spectral lines. I dabbled around alone and tried to find exponential solutions to electrodynamics. I did not get far because I was too young and inexperienced. I asked the great Wigner for help.... Of course, he helped me right away; together we wrote a paper on the natural width of spectral lines, a paper that contained for the first time a divergent integral. I tried to convince Wigner that the integral could be made to vanish. Wigner said, "No, no, it is infinite." I didn't believe him, but he was right, of course. This paper, part of which later became my thesis, was the first paper in which the divergent integral appeared. They have not been resolved; they are still there after 40 years. (Weisskopf, *loc. cit.*, p. 4)

Although it would seem that Weisskopf somewhat exaggerated his own situation, for divergent integrals had already appeared in the Heisenberg–Pauli work on quantum electrodynamics,[858] the two papers which he wrote with Wigner and submitted on 2 May and 12 August 1930, to *Zeitschrift für Physik* marked quite a worthy entrance into the field of theoretical physics for a not-yet 22-year-old student of Max Born (Weisskopf and Wigner, 1930a, b). Weisskopf and Wigner departed from the previous results (of Paul Ehrenfest and others), describing the intensity $J(\nu)$ of radiation (frequency ν) emitted by an oscillator of the quantum frequency $\nu_{B'}^{A}$ in the vicinity of that eigenfrequency as

$$J(\nu)\,d\nu = \gamma_{B'}^{A}\left[\left(\frac{1}{2}\gamma^{A}\right)^{2} + 4\pi(\nu - \nu_{B'}^{A})^{2}\right]^{-1}, \tag{666}$$

[857] Victor Weisskopf was born on 19 September 1908, in Vienna, where he received his education in a gymnasium and entered the University of Vienna to study physics for the first two years under the guidance of Hans Thirring. Following Thirring's advice, he left Vienna in 1928 to continue his studies in Göttingen, where he also attended an inspiring lecture course of Paul Ehrenfest (who, in 1929, substituted for Max Born during his illness). Weisskopf matured in the company of fine fellow students, such as Max Delbrück, Maria Goeppert-Mayer, and Edward Teller, and graduated in 1931 with a thesis on the line-width of spectral lines (which he completed mostly under the guidance of Eugene Wigner). From Göttingen, he went on to Leipzig (to work with Heisenberg, 1931–1932), Berlin (to work with Schrödinger, 1932–1933), Kharkov (1933 with Landau), Copenhagen (with Bohr), and Cambridge (with Dirac), being supported in his later studies by a grant from the Rockefeller Foundation. In fall 1933, Wolfgang Pauli in Zurich hired Weisskopf as a successor to his assistant Hendrik Casimir (who had returned to Leyden after the death by suicide of his mentor Paul Ehrenfest). In spring 1936, Weisskopf visited Bohr in Copenhagen again, and there he married Ellen Tvede. Bohr assisted him in obtaining an instructorship at the University of Rochester in 1937, where (in 1940) he was promoted to an assistant professorship. As a U.S. citizen (since 1942), he joined (in 1943) the American (Manhattan) atomic bomb project in Los Alamos (under J. Robert Oppenheimer), where he assisted Hans Bethe in directing the Theory Division. In 1946, after the war, Weisskopf obtained a full professorship at MIT in Cambridge, Massachusetts; his career there was interrupted by several foreign obligations, such as the position of Director General at the European high-energy centre (CERN) at Geneva from 1960 to 1965.

Weisskopf began to work on quantum field theory from the early 1930s; in 1936, he moved on to work in nuclear physics, but in the 1950s, he returned to research on high-energy physics. (For details of Weisskopf's life and work, see Weisskopf, 1972; Rechenberg, 1978; and von Meyenn, 1985).

[858] See Heisenberg and Pauli, 1929, especially, p. 53, and Heisenberg and Pauli, 1930, p. 184.

with $\gamma^A_{B'}$, denoting the line-width in question, and $\gamma^A \left(= \sum_{B'} \gamma^A_{B'} \right)$ denoting the sum of all line-widths connected with the state A (i.e., the reciprocal of its lifetime). Now, they reproduced Eq. (666) primarily with the help of Paul Dirac's radiation theory (Dirac, 1927b, c); however, in deriving this result, an infinite integral occurred (see Weisskopf and Wigner, 1930a, Eq. (17), p. 63), which was handled in a rather handwaving manner in order to obtain a consistent result (see Weisskopf and Wigner, *loc. cit.*, footnote (*) on pp. 64–65). It was this observation which Weisskopf later remembered as the first occurrence of an infinity in quantum electrodynamics. In the course of the next few years, as we shall report in Section IV.5, Weisskopf would have to deal with more singularities in quantum field theory and become a real expert in handling them.

By 1930, relativistic quantum field theory existed in two versions, one by Heisenberg and Pauli, and the other by Enrico Fermi. Stimulated by Paul Dirac's papers of 1927, Fermi had begun to write a series of short notes in spring 1929 (Fermi, 1929a, b, c; 1930d; 1931). Moreover, he taught the theory of quantum electrodynamics—developed in these notes—in his lecture courses to his collaborators in Rome as well as abroad, e.g., in April 1929 at the *Institut Henri Princaré* in Paris and at the 1930 Summer School of Theoretical Physics at the University of Michigan in Ann Arbor, from which an extensive article resulted that was published in *Reviews of Modern Physics* (Fermi, 1932a).[858a] While the first three notes suggested a quantum-theoretical reformulation of classical electrodynamics and a subsequent application to explain interferfence fringes (Fermi, 1929a, b, c), the fourth one (Fermi, 1930d) pointed out the difference with the meanwhile published papers of Heisenberg and Pauli (1929, 1930). As Fermi stated in his later review article:

> A general theory of the electromagnetic field was constructed by Heisenberg and Pauli by a method in which the values of the electromagnetic potentials in all the points of space are considered as variables. Independently the writer proposed another method of quantization starting from a Fourier analysis of the potentials. Though Heisenberg and Pauli's method puts in evidence much more clearly the properties of relativistic invariance and is in many respects more general, we prefer to use ... the method of the writer, which is more simple and more analogous to the method used in the theory of radiation [i.e., by Dirac, 1927b, c]. (Fermi, 1932a, p. 125)

The new method actually consisted of expanding both the scalar potential V and the vector potential $\mathbf{U}$ at a given time into a Fourier series [see Fermi, 1929a, Eqs. (3) and (4)], notably,

$$V = \sqrt{\frac{8\pi}{\Omega}} c \sum_s Q_s \cos\left(\frac{2\pi \boldsymbol{\alpha}_s \cdot \mathbf{X}}{\lambda_s} + \beta_s\right) \tag{666a}$$

[858a] See the introduction to Fermi's papers on quantum electrodynamics by Edoardo Amaldi, in Fermi, 1962a, p. 305.

and

$$\mathbf{U} = \sqrt{\frac{8\pi}{\Omega}} c \sum_s \mathbf{q}_s \sin\left(\frac{2\pi \boldsymbol{\alpha}_s \cdot \mathbf{X}}{\lambda_s} + \beta_s\right), \tag{666b}$$

where Q_s and $\mathbf{q}_s$ denote the amplitudes of the scalar and vector potentials, respectively (depending on the time), $\boldsymbol{\alpha}_s$ and $\mathbf{X}$ are the vectors of the direction of the electromagnetic wave propagation (of wavelength λ_s and the space coordinates (x, y, z), and Ω is the cavity valume. Fermi expressed the form of the vector $\mathbf{q}_s$ as

$$\mathbf{q}_s = \boldsymbol{\alpha}_s \chi_s + \mathbf{A}_{s1} w_{s1} + \mathbf{A}_{s2} w_{s2}, \tag{666c}$$

where the vectors $\mathbf{A}_{s1}$ and $\mathbf{A}_{s2}$ directly described the two perpendicular directions of the polarizaed light-quanta with the amplitudes w_{s1} and w_{s2}. In order to restrict the degrees of freedom of the light-quanta, χ_s had to satisfy the equation

$$2\pi \nu_s \chi_s + \dot{Q}_s = 0, \tag{666d}$$

and Fermi noted that 'this relation is, as one immediately verifies, identical with the relation

$$\operatorname{div} \mathbf{U} + \frac{1}{c} \frac{\partial V}{\partial t} = 0\text{'} \qquad [(666e)]$$

(Fermi, 1929a, p. 886). Evidently, Fermi introduced here what was called the 'Lorentz condition' in a quantum-mechanical form. He thus avoided the difficulties, which Heisenberg and Pauli had encountered in quantizing the complete set of electromagnetic potentials as independent variables (leading to a gauge ambiguity of the potentials that appear in the Lagrangian formulation of the Maxwell equations).

Fermi's quantum electrodynamics, from which he derived certain consequences (e.g., for the electrodynamical mass of particles, in Fermi, 1931), did not receive much attention, because people considered it to be just another version of the Heisenberg–Pauli approach (see, e.g., the Pauli's *Handbuch* article, 1933c, pp. 264–267). Perhaps the active researchers at that time felt that the then fashionable second quantization had played no prominent role in it. This was different from the investigations of Vladimir Fock, who—at that time—was a relatively senior theoretician.[858b] In 1930, Fock embarked upon quite a productive period of work on field theoretical investigations, being interested originally in justifying the 'ingenious (*geistreiche*) approximation method' which Douglas R. Hartree had

[858b] Vladimir Fock, who had been born in 1898, came, like Landau, from the Leningrad school, and had contributed to quantum mechanics since he proposed in 1926 a generalization of Schrödinger's wave equation (Fock, 1926a, see Volume 5, Part 2, p. 814 ff.).

proposed earlier to deal with many-body problems (Hartree, 1928).[859] Indeed, a variational principle involving a wave-function *Ansatz* in the configuration space, $\boldsymbol{\Psi} = \psi_1(x_1)\psi_2(x_2)\ldots\psi(x_N)$, whose proper symmetry behaviour (i.e., Pauli's exclusion principle for the electrons) could, in the case of 'complete degeneracy of the term system, be approximated by the product of two determinants, i.e.,

$$\boldsymbol{\Psi} = \boldsymbol{\Psi}_1\boldsymbol{\Psi}_2, \qquad [(667)]$$

with

$$\boldsymbol{\Psi}_1 = \begin{vmatrix} \psi_1(x_1)\psi_2(x_1)\ldots\psi_q(x_1) \\ \psi_1(x_2)\psi_2(x_2)\ldots\psi_q(x_2) \\ \ldots \\ \psi_1(x_q)\psi_2(x_q)\ldots\psi_q(x_q) \end{vmatrix} \qquad [(667a)]$$

and

$$\boldsymbol{\Psi}_2 = \begin{vmatrix} \psi_1(x_{q+1})\psi_2(x_{q+1})\ldots\psi_p(x_{q+1}) \\ \psi_1(x_{q+2})\psi_2(x_{q+2})\ldots\psi_p(x_{q+2}) \\ \ldots \\ \psi_1(x_{q+p})\psi_2(x_{q+p})\ldots\psi_p(x_{q+p}) \end{vmatrix}, \qquad [(667b)]$$

where $q + p = N$.' (Fock, 1930, p. 138) Thus, he completed what was called the 'Hartree–Fock method,' one of the most powerful approximation methods in nonrelativistic systems of many Fermi particles.

In January 1931, Fock presented in the theoretical physics seminar of the University of Leningrad another detailed study dealing with the relation of the method of second quantization in nonrelativistic quantum field theory and Schrödinger's original wave equation in configuration space (Fock, 1932). He proceeded in two steps: In Part I, he established the 'second quantized' wave functions, the $\boldsymbol{\Psi}$-operators from the Schrödinger function according to the prescription of Jordan and Klein and Jordan and Pauli, respectively (see Section II.5); then he continued:

> The starting point of the considerations of Part II constitute the commutation relations between the quantized wave functions ($\boldsymbol{\Psi}$-operators). It will be shown that these relations can be satisfied by certain operators, which act on a sequence of usual wave functions for $1, 2, \ldots n, \ldots$ particles. In this way the $\boldsymbol{\Psi}$-operators are represented in the configuration space (more accurately, in a sequence of configuration spaces).

[859] We have referred to this method of the 'self-consistent field' in Section III.4.

> Further, the dependence of the $\boldsymbol{\Psi}$-operators on the time will be considered, and we shall find the form of the operator $\dot{\boldsymbol{\Psi}} = \partial\boldsymbol{\Psi}/\partial t$. Then, on the basis of the representation obtained, it will be shown that the time-dependent Schrödinger equation for $\boldsymbol{\Psi}$-operators can be written as a sequence of ordinary Schrödinger equations for $1, 2, \ldots n, \ldots$ particles. As another application of the representation obtained we present a simple derivation of the Hartree equation with exchange. (Fock, *loc. cit.*, pp. 622–623)

In particular, Fock constructed two operators $\boldsymbol{\Psi}$ and $\boldsymbol{\Psi}^+$, such that their product integral

$$\boldsymbol{n} = \int \boldsymbol{\Psi}^+(x)\boldsymbol{\Psi}(x)\,dx \tag{668}$$

yields the eigenvalues $n = 0, 1, 2, 3, \ldots$, and they satisfy the commutation relations (with $\varepsilon = +1$ and -1 for Bose and Fermi statistics, respectively)

$$\left.\begin{aligned}\boldsymbol{\Psi}(x')\boldsymbol{\Psi}^+(x) - \varepsilon\boldsymbol{\Psi}^+(x)\boldsymbol{\Psi}(x') = \delta(x - x')\\ \boldsymbol{\Psi}(x')\boldsymbol{\Psi}(x) - \varepsilon\boldsymbol{\Psi}(x)\boldsymbol{\Psi}(x') = 0\end{aligned}\right\} \tag{669}$$

and demonstrated the following: They act on a sequence of usual Schrödinger functions, $\psi(x_1)$, $\psi(x_1x_2)$, $\psi(x_1x_2x_3)$, such that $\boldsymbol{\Psi}$ leads from a function of n variables to a function of $(n-1)$ variables, and $\boldsymbol{\Psi}^+$ from a function of $(n-1)$ variable to a function of n variables. This formalism constituted what one later called the 'Fock-space representation,' with $\boldsymbol{\Psi}^+$ and $\boldsymbol{\Psi}$ denoting creation and annihilation operators, respectively. Later on, this representation would play an important role in relativistic quantum field theory. Moreover, Vladimir Fock himself soon went on to consider relativistic problems, especially in collaboration with Paul Dirac.

Dirac, who maintained close relations with several Russian physicists, especially Igor Tamm (whom he had first met in spring 1928 in Leyden), and visited the Soviet Union repeatedly after the Kharkov Conference on Theoretical Physics (which he had attended in May 1929)—thus, in September/October 1929 he passed through the USSR again upon his return from his world trip, and then again in the summers of 1930 and 1932—closely followed the work of Fock.[860] Besides exchanging ideas regularly with Tamm, who showed great interest in the negative-energy states of his relativistic equation for the electron, Dirac entered into a collaboration with Fock in summer 1932 on a new approach to quantum electrodynamics.[861] In a paper, entitled 'Relativistic Quantum Mechanics' and

[860] See Dirac's response to Fock's paper dealing with the Hartree method: Dirac, 1931b.

[861] For an account of Dirac's relations with the Russian physicists, we refer to Alexei B. Kojevnikov's annotated edition of the Dirac–Tamm correspondence between 1928 and 1933 (Kojevnikov, 1993).

submitted to the *Proceedings of the Royal Society of London* in March 1932, Dirac had criticized the foundation of the Heisenberg–Pauli relativistic quantum field theory of 1929, especially the assumption that the field could be regarded 'as a dynamical system amenable to Hamiltonian treatment and its interaction with the particles as describable by an interaction energy, so that the usual methods of Hamiltonian mechanics may be applied.' In particular, Dirac noted:

> There are serious objections to these views, apart from the purely mathematical difficulties to which they lead. If we wish to make an observation on a system of interacting particles, the only effective method of procedure is to subject them to a field of electromagnetic radiation and see how they react. Thus the rôle of the field is to provide a means for making observations. *The very nature of an observation requires an interplay between the field and the particles.* We cannot therefore suppose the field to be a dynamical system on the same footing as the particles and thus something to be observed in the same way as the particles. The field should appear in the theory as something more elementary and fundamental. (Dirac, 1932, p. 454)

In contrast to Heisenberg and Pauli (see Section II.7), Dirac assumed 'the field equations as [being] always linear;' hence, 'deep-lying connections and possibilities for simplification and unification' may be reached (Dirac, *loc. cit.*, pp. 454–455). In any case, he concluded that 'quantities referring to two initial fields, or to two final fields, are not allowed,' because they 'are unconnected with results of observations and must be removed from consideration if one is to obtain a clear insight into the underlying physical relations' (Dirac, *loc. cit.*, p. 457).

Dirac's new proposal deviated from the procedure which followed from the classical theory—such as 'assuming a definite structure of the electron and calculating the effect of one part of it on the field produced by the rest' (Dirac, *loc. cit.*, p. 457)—by taking into account the influence of both the incoming and the outgoing fields, such:

> that we may associate, say the right-hand sides of the probability amplitudes [for the quantities of the relativistic theory] with ingoing fields and the left-hand sides with the outgoing fields. In this way we automatically exclude quantities referring to two ingoing fields, or two outgoing fields and make a great simplification in the foundations of the theory. (Dirac, *loc. cit.*, p. 458)

If retranslated into the classical picture, the electromagnetic field considered corresponded to a free field (i.e., a Maxwell field in empty space), and interaction could occur only with the field of the electron ψ, or

$$F\psi = 0, \tag{670}$$

where F, neglecting spin, is

$$F = \left(\frac{ih}{2\pi}\frac{\partial}{\partial t} + eA_0\right)^2 - \left(\frac{ihc}{2\pi}\frac{\partial}{\partial x} - eA_x\right)^2 - \cdots - m^2c^4 \tag{670a}$$

(with e and m denoting the charge and mass of the electron). In the special case of interaction between two electrons, the ψ must then satisfy two equations with the respective operators F_1 and F_2 depending only on the coordinates of the first and second electron, respectively. The interaction manifested itself just in the functions ψ_1 and ψ_2, each satisfying a separate Eq. (670), but 'neither of the products $\psi_1\psi_2$ and $\psi_2\psi_1$ will satisfy both equations [(670)]' (Dirac, *loc. cit.*, p. 460). Dirac finally demonstrated in a simplified example—two electrons in one space dimension—that the usual result of (the Heisenberg–Pauli) quantum electrodynamics was also obtained in the new theory.

Dirac eagerly presented his new approach to relativistic quantum field theory—the first he had proposed since his pioneering work five years earlier on the relativistic theory in 1927—both to Heisenberg and to the other members of Bohr's Institute in Copenhagen (where he visited in April 1932). Oskar Klein, who perused the paper in Dirac's presence, recalled:

> And when I turned the first page, Dirac said, "You ought to read the paper more slowly; Heisenberg read it too fast." And then I heard that Heisenberg had objected that this was just the old theory in a new form. (Klein, AHQP Interview, 1963)

At that time, Pauli was Dirac's chief critic and he rejected Dirac's theory completely. As he wrote to Lise Meitner, the theory 'cannot be taken seriously; neither does it contain anything new, nor is it justified to speak of a "theory."' (Pauli to Meitner, 29 May 1932, in Pauli, 1985, p. 114) In writing to Dirac about his work, Pauli's judgment was no less candid:

> Your remarks about quantum electrodynamics which appeared in the *Proceedings of the Royal Society* were, to put it gently, certainly no masterpiece. After a muddled introduction, which consists of sentences which are only half understandable because they are only half understood, you come at last, in an oversimplified one-dimensional example, to results which are identical to those obtained by applying Heisenberg's and my formalism to this example ... This end of your paper conflicts with your assertion, stated more or less clearly in the introduction, that you could somehow or other construct a better quantum electrodynamics than Heisenberg and I. (Pauli to Dirac, 11 September 1932, in Pauli, *loc. cit.*, p. 115).[862]

The official published response to Dirac's work was given by Léon Rosenfeld in a paper submitted from Copenhagen to *Zeitschrift für Physik* in May 1932: 'The Heisenberg-Pauli quantum electrodynamics represents a possible formulation of the programme of relativistic quantum mechanics proposed recently by Dirac.' (Rosenfeld, 1932, p. 729) Yet Paul Dirac, though he admitted the mathematical equivalence of both theories—'The connection which you give between my new theory and the Heisenberg-Pauli theory is, of course, quite general.' (Dirac to

[862] For further details of Dirac's new electrodynamics of 1932 and the response of his scientific colleagues, see Kragh, 1990, especially, pp. 132–136.

Rosenfeld, 6 May 1932)—strongly insisted upon the physical difference and continued to think about and work upon it. When he attended the Leningrad conference on the theory of metals, organized by his friend Igor Tamm in September 1932, Paul Dirac not only mentioned it in his talk, but also discussed the problem with two other participants, Vladimir Fock and Boris Podolsky. Together, they submitted a joint paper, entitled 'On Quantum Electrodynamics,' to the *Physikalische Zeitschrift der Sowjetunion* (Dirac, Fock, and Podolsky, 1932).[863]

The Dirac–Fock–Podolsky investigation consisted of two parts, one devoted to a 'simplified proof' of the 'equivalence of Dirac's and Heisenberg-Pauli's theories,' while the other treated 'the Maxwellian case' in detail. The main aspect of the new theory of Dirac, Fock, and Podolsky lay in the fact that it allowed them to exhibit relativistic invariance more explicitly. Thus, the Heisenberg–Pauli scheme described a system consisting of two subsystems, A and B, by the Hamiltonian equation

$$\left(H - \frac{ih}{2\pi}\frac{\partial}{\partial T}\right)\psi(q_a, q_b, T) = 0, \tag{671}$$

with the Hamiltonian operator

$$H = H_a + H_b + V \tag{671a}$$

(where a and b referred to the subsystems A and B, respectively, with the position coordinates q_a and q_b and the time T). In Dirac's new scheme, Eq. (671) had now to be replaced by

$$\left(H_a^* + V^* - \frac{ih}{2\pi}\frac{\partial}{\partial T}\right)\psi^* = 0, \tag{672}$$

with

$$\psi^* = \exp\left(\frac{2\pi i}{h} H_b T\right)\psi \tag{672a}$$

and

$$F^* = \exp\left(\frac{2\pi i}{h} H_b T\right) F \exp\left(-\frac{2\pi i}{h} H_b T\right), \tag{672b}$$

[863] Fock and Podolsky had already previously studied Dirac's paper in the *Proceedings of the Royal Society* (1932). After the Leningrad conference, Dirac took a vacation for a couple of weeks in the Crimea; on his return to Moscow, he passed through Kharkov, where he agreed with Podolsky to write the joint paper (indeed, Podolsky worked out the first draft and then communicated with Fock and Dirac by letters). See Kojevnikov, 1993, pp. 61–63, especially the letter of Dirac to Tamm, dated 26 September 1932, and sent from Gaspra, Crimea.

where $F = H_a$, or V. Since H_a commuted with H_b, there followed $H_a^* = H_a$, and further

$$V^* = V(p_a, q_a, p_b^*, q_b^*). \tag{673}$$

Evidently, if the subsystem A (having dynamical variables q_a and p_a) represented the particle and $B(q_b, p_b)$ the Maxwellian field—as in Dirac's quantum electrodynamics of March 1932—the q_b^* and p_b^* satisfied the free Maxwell equations, unperturbed by the presence of the subsystem A. Moreover, Dirac, Fock, and Podolsky found that Eq. (672) might assume the form

$$\left[\sum_s (H_s + V_s^*) - \frac{ih}{2\pi}\frac{\partial}{\partial T}\right]\psi^*(r_s; J, T) = 0, \tag{674}$$

where $\sum_s H_s$ denoted the sum of the particle contributions to the free Hamiltonian H_a. The particles then interacted with the electromagnetic field, such that $V^* = \sum V_s$ represented the sum of interaction terms involving the field and the particles. In the wave function, J stood for the variables of the field and r_s for the space variables of the particles. Eq. (674) now possessed a simpler solution if one introduced '*besides the common time T and the field time t an individual time $t_s = t_1, t_2, \ldots t_n$ for each particle*' (Dirac, Fock, and Podolsky, *loc. cit.*, p. 470, our italics), namely,

$$\left(R_s - \frac{ih}{2\pi}\frac{\partial}{\partial t_s}\right)\psi^* = 0, \tag{675}$$

where

$$R_s = c\alpha_s \cdot p_s + m_s c^2 \alpha_s^4 + \varepsilon_s[\Phi(r_s, t_s) - \alpha_s \cdot A(r_s, t_s)] \tag{675a}$$

and

$$\psi^* = \psi^*(r_1 r_2 \ldots r_n; t_1 t_2 \ldots t_n; J), \tag{675b}$$

with all t_s put equal to the common time t.

Equation (675) defined what was later called the 'many-time formalism' and was used especially by the Japanese physicist Sin-itiro Tomonaga many years later to formulate renormalized relativistic quantum electrodynamics.[864] At that time, however, nobody derived any profound consequences from this formalism. Actually, in August 1933, Felix Bloch in Zurich, submitted a detailed study to the *Physikalische Zeitschrift der Sowjetunion* dealing with '*Die physikalische Bedeu-*

[864] We shall discuss this future development in the Epilogue.

tung mehrerer Zeiten in der Quantenelektrodynamik (The Physical Meaning of Many Times in Quantum Electrodynamics).' He summarized his results in the abstract as follows:

> It will be shown that the wave function of Dirac-Fock-Podolsky's quantum electrodynamics, which depends on several times, can be interpreted analogously to the usual wave mechanics as probability amplitude for such measurements which are performed at times t_s on the particles s and at time t on the electromagnetic field. One must demand, as the condition of integrability for the differential equations, that one restricts oneself to intervals of the particle times during which the particles cannot influence each other by radiation. Further, one must demand for the physical interpretation that the field measurement should also be concerned only with such space-time regions in which the field quantities existing there cannot be influenced by the radiation emitted by the particles. (Bloch, 1934, p. 301)

Indeed, in spite of his continuing dissatisfaction with the Heisenberg–Pauli quantum electrodynamics, Dirac and his collaborators were not able to change the situation effectively in the 1930's,[865] as Pauli wrote to him candidly in fall 1932 (quoted earlier): 'The end of your paper [Dirac, 1932] conflicts with your assertion, ... that you could somehow or other construct a better quantum electrodynamics than Heisenberg and I.' (Pauli to Dirac, 11 September 1932, in Pauli, 1985, p. 115).

These specific developments of quantum electrodynamics in the early 1930's illustrate that the concern with relativistic quantum field theory kept the elite among the quantum theoreticians occupied; Dirac, Heisenberg, Pauli, and others did not stop thinking about what the fundamental difficulties revealed, especially concerning the infinities arising in the calculation of certain crucial physical quantities.[866] Infinities had plagued atomic theory since the discovery of the electron; an electron of finite size seemed to contradict relativity theory, and the self-energy of a point electron became infinitely large.[867] In 1929, Heisenberg and Pauli confirmed the occurrence of the infinite self-energy of the electron also in their formulation of quantum electrodynamics (Heisenberg and Pauli, 1929; 1930), and J. Robert Oppenheimer's subsequent evaluation showed that the divergence was quadratic—which was worse than in the classical case, where it came out linearly (Oppenheimer, 1930a). For dealing with this problem, Heisenberg and Pauli in particular employed the most radical and revolutionary means. Thus, Heisenberg spoke in early 1930 for the first time about the necessity of introducing a quantization of space, i.e., to endow the three-dimensional space with a lattice structure having the universal lattice constant $L = h/Mc$ (with M denoting the mass of the proton). In a letter to Niels Bohr at that time, he wrote:

[865] For these attempts, see also the discussion in Kragh, 1990, pp. 136–139.

[866] A condensed review of the infinity problems in quantum field theory of the 1930s may be found in Pais, 1986, Chapter 16.

[867] See, e.g., Frenkel, 1925.

> I cannot report anything pleasant about physics. I now believe also that in the electrodynamics of Pauli and myself the self-energy of the particles and the Dirac transitions destroy everything. Recently I have tried to split—in a manner similar to what [was done] previously in the phase space—the real space into discrete cells of size $(h/Mc)^3$, in order to obtain a reasonable [i.e., finite] theory. Such a theory turns out to appear already qualitatively much different than hitherto considered; but I am still rather sceptical whether such a coarse method will yield many reasonable results. However, I believe one thing quite definitely, namely that a future theory will just have to exploit the freedom that lies in the uncertainty of h/Mc for all determinations of length. (Heisenberg to Bohr, 26 February 1930)

In a later letter, again to Bohr (dated 10 March 1930), Heisenberg went into some details of calculation in the field theory of a one-dimensional quantum-theoretical lattice model. He argued that it would endow an electron with a self-energy of the order of $\frac{e^2}{hc}Mc^2$, and that only slight difficulties would arise with Lorentz invariance and charge conservation.

The space-lattice model just represented the first of a series of attempts by which Heisenberg and Pauli hoped to cure the divergence problem of quantum field theory.[868] Although they shared the same final goal, usually it was Heisenberg who pushed forward with concrete proposals; by taking a more positive attitude than Pauli toward Dirac's 'hole theory' (which we shall discuss below), he hoped to connect the latter with the determination of the fine structure constant, as he remarked in a letter to Pauli:

> I have the feeling that the step from the present quantum electrodynamics to $e^2/\hbar c$ [the fine structure constant, with $\hbar = h/2\pi$] is not much bigger than that of your earlier theory of spin to Dirac's. Our field quantization was so-to-speak simply a thoughtless repetition of the familiar scheme and its application to problems to which it does not fit completely. Now only a new, formal idea is missing, and in order to establish a reasonable quantum theory of fields perhaps no new physical facts will be necessary at all. (Heisenberg to Pauli, 16 June 1934, in Pauli, 1985, p. 333)

But what the new idea should look like, Heisenberg did not know even after further efforts in the following months, which he again summarized in a letter to Pauli:

> The whole labour of calculations has strengthened my belief that there must exist a unified field theory which is characterized by a Hamiltonian function that depends quadratically on a density matrix; and in this theory the electron and the light-quantum must [emerge as] nontrivial solutions. (Heisenberg to Pauli, 22 March 1935, in Pauli, *loc. cit.*, p. 383)

While Heisenberg and Pauli's dream of a unified quantum field theory was not realized in the later 1930s, they and others obtained a host of results from purely

[868] For a condensed account of these efforts, which extended into the 1950s, see Rechenberg, 1993b.

theoretical considerations, some of which we shall report on in Section IV.5. In the early 1930's, however, a different path emerged, mainly through the discovery of new elementary particles, whose existence immediately solved old problems of quantum theory and opened new vistas in atomic, nuclear, and high-energy physics. Again, the theoreticians, notably, Dirac, Pauli, and Heisenberg, played a crucial role through prophetic predictions and ingenious applications.

In order to enter into the spirit of this most fruitful period of cooperation between experiment and theory, let us quote from a popular article which Heisenberg wrote for the Christmas 1931 issue of the widely read *Berliner Tageblatt*, dealing with 'The Problems of Modern Physics.' Heisenberg reported there about the new atomic theory, which had been developed since 1925, and its relation to the conventional understanding and natural philosophy; he discussed the problems concerning causality and visualizability that had arisen in quantum mechanics, and claimed that its necessarily more abstract concepts (compared to those of the former classical theories) 'made it possible to consider "electrons" and "protons" really as the ultimate constituents of matter.' He then continued:

> The next progress ... will consist in a more accurate experimental investigation of the atomic nucleus. The interior of the atomic nucleus thus far defies all efforts of the theoreticians to formulate the laws governing it. An extensive experimental research must first force the atomic nucleus to reveal its behaviour. It will then be possible to recognize the connections. Whether the year 1932 will already lead to this recognition, may be quite doubtful. (Heisenberg, 1931e)

With these doubts, Heisenberg evidently had in mind the insurmountable difficulties noticed up to then in applying quantum mechanics to the inner structure of the atomic nucleus (see the discussion above in Section III.7). He speculated that the procedure outlined above would occupy a number of years to come.

Heisenberg did not anticipate, however, the speed with which the progress actually occurred, and how it was achieved not by a patient study of the complex properties of nuclei but rather by a series of discoveries made by both theoreticians and experimentalists. These discoveries increased, in particular, the number of the 'ultimate constituents of matter' or 'elementary particles.' In looking back on these exciting events, the historian of science Charles Weiner remarked:

> In 1972 we celebrate the fortieth anniversary of the '*annus mirabilis*' of nuclear and particle physics. Seen from the perspective of the present, the cluster of major conceptual and technical developments of 1932 mark the "marvellous" year as a very special one. It began with Harold Urey's announcement in January that he had discovered a heavy isotope of hydrogen, which he called "deuterium." In February James Chadwick demonstrated the existence of a new nuclear constituent, the neutron. In April John Cockcroft and E. T. S. Walton achieved the first disintegration of the nucleus by bombarding light elements with artificially accelerated protons. In August Carl Anderson's photographs of cosmic-ray tracks revealed the existence of another new particle, the positively charged electron, soon to be called the "positron." And later that summer Stanley Livingston and Milton White disintegrated

> nuclei with the cyclotron, an instrument that would generate almost 5 million electron volts by the end of that eventful year.
>
> New particles, new constituents of the nucleus and powerful techniques for probing its structure—they all provided a wealth of fresh challenges and opportunities for theory and experiment. Physicists who remember the excitement of those days sometime sound as if they were relishing an excellent wine when they smile and comment: "It was a great year." (Weiner, 1972, p. 40)

Weiner singled out from these events just the one year—1932—but actually the 'miraculous year' represented only the early centre and climax of experimental contributions in a wonderful period of *theoretical and experimental* discoveries extending from 1930 to 1937. It was started with the theoretical analyses of Paul Dirac and Wolfgang Pauli between 1930 and 1931, from which they predicted the existence of two new elementary particles, later called the 'positron' and the 'neutrino.' Even before the empirical substantiation of these particles, the experimental progress set in by the construction of machines which artificially created high-energy nuclear particles, such as the Van de Graaff accelerator (in September 1931), the cyclotron, and the Cockcroft–Walton device (both in February 1932). The discovery of the neutron immediately stimulated Heisenberg's explanation of nuclear structure (from May 1932 onward), which, in turn—together with the neutrino hypothesis—paved the way for another theoretical progress: Enrico Fermi's description of the beta-decay (December 1933); still, a few weeks later, the positive beta-decay, including the emission of positrons, was discovered (January 1934). While the discovery of the positron and the electron-positron pair creation (in early 1932) in cosmic radiation provided the key to the understanding of cosmic-ray phenomena (the 'soft component'), Fermi's theory was taken as the basis for explaining *all* nuclear forces, a wrong idea although it was upheld for several years by most experts in nuclear physics. Indeed, a new theoretical idea which involved the existence of a further hypothetical particle was put forward in Japan by the end of the year 1934 to account especially for 'strong' nuclear forces; between 1936 and 1937, several groups in America and Japan observed in cosmic radiation an object which seemed to fit Hideki Yukawa's 'heavy quantum' and was named the 'mesotron.' The story of these experimental and theoretical discoveries and developments will be covered in the rest of this section.

(b) The Theoretical Prediction of Dirac's 'Holes' and 'Monopoles' (1928–1931)

Several decades after his experimental observation of 'the apparent existence of easily deflectable positives,' which he reported in early September 1932 in the American journal *Science* (Anderson, 1932b, p. 239), Carl Anderson recalled:

> It has often been stated in the literature that the discovery of the positron was a consequence of the theoretical prediction of Paul A. M. Dirac, but this is not true. The discovery of the positron was wholly accidental. Despite the fact that Dirac's

> relativity theory of the electron was an excellent theory of the positron, and despite the fact that the existence of this theory was well known to nearly all physicists, including myself, it played no role whatsoever in the discovery of the positron.[869]

Actually, Anderson's statement illuminated only the final, experimental story of one of the most fundamental concepts of elementary particle theory, the existence of anti-particles. The development began several years before 1932 as a theoretical idea whose evolution we shall now analyze in some historical detail.

Having proposed his relativistic electron equation in 1928 (Dirac, 1928a, b), Paul Dirac began to analyze the physical content of his new theory and hit upon a difficulty which he first stressed in his presentation at the *Leipziger Universitätswoche* in June (Dirac, 1928c). A few weeks later, he wrote to Oskar Klein in Copenhagen: 'I have not met with any success in my attempts to solve the $\pm e$ difficulty. Heisenberg (whom I met in Leipzig) thinks the problem will not be solved until one has a theory of the proton and the electron together.' (Dirac to Klein, 24 July 1928, quoted in Pais, 1986, p. 348) It was the difficulty of the extra solutions of the equation having apparently negative energy, which irritated Dirac and his colleagues quite a lot in those days. The unwanted solutions could neither be discussed away nor suppressed, as Klein and Yoshio Nishina demonstrated in their investigation of the Compton effect on the basis of the Dirac equation (Klein and Nishina, 1928), although a strange new paradox was thereby discovered: When electrons were reflected from a potential wall, a greater intensity was returned than was going in (Klein, 1929a).[870]

In the later months of 1928 and in early 1929, Dirac was occupied with the writing of his book on quantum mechanics (which he finally completed only in May of the following year: Dirac, 1930d). At the end of March 1929, he left Cambridge for a tour around the world, beginning with a stay of several months in the United States (Madison, Wisconsin, and Ann Arbor, Michigan), then traveling to Japan, and returning to England via the Soviet Union. Prior to leaving on this tour, he noted in a letter to Igor Tamm: 'Have you seen Weyl's book "*Gruppentheorie und Quantenmechanik*"? It is very clearly written and by far the most connected [i.e., systematic] account of quantum mechanics that has appeared, although it is rather mathematical and therefore not very easy.' (Dirac to Tamm, 3 January 1929, in Kojevnikov, 1993, p. 18) The mathematician Hermann Weyl continued—after publishing his book (Weyl, 1928b)—to work on problems of quantum physics, and he reciprocated Dirac's interest in his work. In a paper on '*Elektron und Gravitation* (Electron and Gravitation),' he addressed the relativistic electron theory directly, commenting:

> The Dirac-Maxwell theory in its present form contains only the electromagnetic potentials f_p [i.e., A_μ, $\mu = 0, 1, 2, 3$] and the wave field ψ of the *electron*. Doubtlessly,

[869] This recollection was reported in C. D. and H. L. Anderson, 1983, p. 140.

[870] We have discussed the difficulties arising from the Dirac equation in Section II.7.

the wave field ψ' of the proton must be added. In particular, in the field equations ψ, ψ' and f_p will be functions of the same four space-time coordinates, and one will not be allowed really to demand before quantization that ψ is the function of a world point (t', x', y', z') independent of the former. *It suggests itself to expect that of both component pairs of Dirac's quantity* [i.e., the four-component spinor ψ] *one is associated with the electron, and the other with the proton* [our italics]. Further, two conservation laws of electricity will have to exist which (after quantization) tell us that the number of electrons remains constant like that of protons. (Weyl, 1929b, p. 332)

When Dirac, upon his return from his world tour, resumed his teaching duties at Cambridge—the term started in the second week of October 1929—he also thought about the approach indicated earlier by Weyl. Thus, in his lecture series on the problems of quantum mechanics, given in December 1929 as a visitor at the *Institut Henri Poincaré* in Paris, he stated explicitly:

The fact that there are four components to ψ is unexpected.... The reason is that in the relativistic Hamiltonian we started from, the W [of the relativistic equation] is not uniquely determined. From this equation W, or rather $W + eA_0$, can be positive or negative. However, only positive values have a physical meaning. Half of our wave function ψ thus corresponds to states for which the electron has negative energy. This is a difficulty which appears in all relativistic theories [of the electron], in the classical as well as in ours here. In the classical theory it is not serious, because none of the dynamical variables can change in time in a discontinuous fashion....

In quantum mechanics, on the other hand, one cannot in general clearly separate a solution ψ of the wave equation into a part which corresponds to positive kinetic energy and another corresponding to negative energy. Even in special cases where this is possible, for example in the case where the field is constant, a perturbation can produce a transition from a state of positive energy to one of negative energy. (Dirac, 1931a, p. 398).

In order to resolve this problem, Dirac considered the trajectory of negative states in classical theory and found that 'the motion of an electron with negative energy is identical to that of a positive electron with charge $+e$ instead of $-e$,' a result transferable to quantum mechanics; hence, he concluded:

The negative-energy electron behaves a little like a proton, but it cannot be exactly a proton, because a proton certainly does not have a negative energy. If a negative-energy electron had a large velocity, it would have to absorb energy in order to come to rest, and we are sure that protons do not have this property.

The connection between negative-energy electrons and protons can be established in a different way. We will make the following hypothesis: almost all the negative-energy states in the universe are occupied; it is the empty places which constitute the protons. (Dirac, *loc. cit.*, p. 399)

Dirac had written about the ingenious and revolutionary ideas expressed in his Paris lectures first in a letter to Niels Bohr, who had earlier suggested (see Bohr to Dirac, 24 November 1929) that the problem of negative-energy states should be resolved by renouncing energy conservation. However, Paul Dirac preferred 'to keep rigorous conservation of energy at all costs and would rather abandon even the concept of matter consisting of separate atoms and electrons,' and introduced 'a simple way of avoiding the difficulty of electrons having negative kinetic energy':

> Let us now suppose that there are so many electrons in the world that all these most stable [negative] energy states are occupied. The Pauli principle will then compel some electrons to remain in less stable states. For example, if all the states of negative energy are occupied and also few of positive energy, those electrons with positive energy will then be unable to make transitions to states with negative energy and will therefore have to behave quite properly. The distribution of negative energy electrons will, of course, be of infinite density, but it will be quite uniform so that it will not produce any electromagnetic field and one would not expect to be able to observe it. (Dirac to Bohr, 26 November 1929)[871]

The situation thus introduced the idea of the 'filled' vacuum, which would later be termed the 'Dirac sea,' but Dirac himself described the vacant places in this sea as 'holes.' As he explained further in his letter to Bohr:

> Such a hole can be described by a wave function like an X-ray orbit [in nonrelativistic atomic theory] would appear experimentally as a thing with positive energy, since to make the hole disappear (i.e., to fill it up) one would have to put negative energy into it. Further one can easily see that such a hole would move in an electromagnetic field as though it had positive charge. These holes I believe to be protons. When an electron of positive energy drops into a hole and fills it up, we have an electron and proton disappearing simultaneously and emitting their energy in the form of radiation. (Dirac to Bohr, *loc. cit.*)

Dirac immediately published the ideas described in the letter to Bohr in a paper, entitled 'A Theory of Electrons and Protons' and communicated in early December 1929 by Ralph Fowler to the *Proceedings of the Royal Society* (Dirac, 1930a). There, he also overcame the problem of the infinite density (caused by the negative-energy electrons) by the following argument:

> It seems natural ... to interpret the [density] ρ in Maxwell's equation [i.e., $\operatorname{div} E = -4\pi\rho$] as the *departure* [our italics] from the normal state of electrification, which normal state of electrification, according to the present theory, is the one where every electronic state of negative energy and none of positive energy is occupied. This ρ will then consist of charge $-e$ arising from each state of positive energy that is occupied, together with a charge $+e$ arising from each state of negative energy that is unoccupied. Thus the field produced by a proton will correspond to its having a charge $\pm e$. (Dirac, *loc. cit.*, p. 363)

[871] Dirac's letter to Bohr has been reproduced in full in Kragh, 1990, pp. 90–91. For details of the Bohr–Dirac exchange on the whole positron story, see Kragh, 1990, Chapter 5, pp. 87–117, and the paper of Donald F. Moyer, 1981b.

By means of this revolutionary concept of the vacuum as a completely filled 'Dirac sea,' Paul Dirac solved the original paradoxes arising from his electron equation, namely: (i) the problem of violating charge conservation (when an electron makes a transition into a proton); (ii) the Coulomb repulsion between electrons and negative energy states; (iii) the decrease of (absolute) energy with increasing velocity for a negative-energy state. Of course, Dirac was quite aware of the dramatic consequences that might ensue from combining electrons and protons in *one* relativistic equation: Especially, the great dissymmetry shown by the two different particles was also disturbing as were their specific roles in forming atoms or atomic nuclei (as was assumed at that time). However, he expected that the interactions between the particles—electron and proton—would take care of these problems. 'The consequences of this dissymmetry are not easy to calculate on relativistic lines, but we hope it will lead eventually to an explanation of the different masses of proton and electron,' he argued and added: 'Possibly some more perfect theory of interaction, based perhaps on Eddington's calculation of the fine structure constant $e^2/(h/2\pi)c$, is necessary before this result can be obtained.' (Dirac, *loc. cit.*, p. 364)

The well-known Cambridge astrophysicist Arthur Stanley Eddington had earlier in 1929 published an ingenious idea on how to derive the charge-coupling constant of an electron (Eddington, 1929). Though Heisenberg (in a letter to Dirac of March 1929) and Pauli (in a letter to Klein, dated 18 February 1929) had declared Eddington's proposal to be quite unreasonable or 'romantic poetry,' Dirac assumed a more tolerant attitude toward his colleague's conceptions and used them to support his own work. The reactions to Dirac's new theory of electrons and protons fluctuated between enthusiastic approval and increasingly serious criticism.[872] Bohr, in whom Dirac first confided, raised a couple of objections, which were partially answered already in a published paper (principally, by the new definition of the vacuum). George Gamow (who witnessed the origin of Dirac's work in Cambridge) and Paul Ehrenfest brought the new theory to Germany and Russia, respectively; in Russia, Igor Tamm and Dmitrij Iwanenko at once agreed, whereas Vladimir Fock remained reserved.[873] Heisenberg, who also heard about the new paper prior to publication (from Lev Landau through Gamow), welcomed Dirac's conclusion but 'did not yet see how the ratio of the masses, etc., will come out' (Heisenberg to Dirac, 7 December 1929). A little later, he wrote again to Dirac: 'One can prove that the electron and the [Dirac] proton had to have the same mass,' and objected further: 'How can the negative-energy electron go up to the final level which is already occupied [in a process of normal

[872] For a summary of the reactions, see Kragh, 1990, pp. 94–96.

[873] For instance, Tamm wrote to Dirac on 5 February 1929: 'The idea to put the whole of negative energy upside down, and to create from the presumable difficulty a unified theory of electricity, enlightens—once one gets to know it—like a flash! I really (*innigst*) hope that you succeed in calculating the mass of the proton and thus will be able to substantiate your whole theory.' (For the original German, see Kojevnikov, 1993, p. 30)

scattering]?' (Heisenberg to Dirac, 16 January 1930).[874] Many of the objections raised by colleagues certainly rested on an incomplete knowledge of the 'hole theory,' though they were raised often years after the publication of Dirac's paper in June 1930. On the other hand, much of Dirac's 'hole' and 'sea' concepts arose from his visual inspiration from the nonrelativistic atomic theory.[875] Admittedly, the identification of the holes with protons—which were not necessarily just the negative-energy components of the naively interpreted Dirac equation, as Kragh pointed out (Kragh, 1990, p. 95)—created most problems, and this idea had finally to be abandoned.[876]

Publicly, Dirac stuck to the unified electron–proton theory during the following year. In his second paper (Dirac, 1930b), which he submitted to the *Proceedings of the Cambridge Philosophical Society* on 26 March 1930, he treated explicitly the process of 'annihilation of electrons and protons' on the basis of the hole theory, leading to the emission of *two* photons (because of energy and momentum conservation).[877] Considering this process as 'stimulated emission,' Dirac could avoid it in the calculation of the quantization of the radiation field and apply a straightforward quantum-mechanical density-matrix scheme, which he had considered earlier in connection with statistical mechanics (Dirac, 1929a). Thus, he first obtained (in §5 of his paper: Dirac, 1930b) in second-order perturbation theory the Compton effect formula of Oskar Klein and Yoshio Nishina (1929). On the other hand, the proton–electron annihilation process, described in the same order, exhibited a transition probability per unit time, (with $\gamma = \frac{v}{c}$ for electrons),

$$P_{ep\to 2\gamma} = \frac{\pi e^4}{m^2 c^3}\frac{1}{\alpha(\alpha+1)}\left\{\frac{\alpha^2+4\alpha+1}{(\alpha^2-1)^{1/2}}\log[\alpha+(\alpha^2-1)^{1/2}]-(\alpha+3)\right\}, \tag{676}$$

with

$$\alpha = \gamma - 1. \tag{676a}$$

[874] The letters of Heisenberg to Dirac are from the Dirac Papers, Florida State University, Tallahassee. The quotations are from Kragh, 1990, pp. 94–95, and from Brown and Rechenberg, 1987, p. 140.

[875] Earlier in 1929, Dirac had published on this topic, especially the Hartree method for many-electron systems (Dirac, 1929b), and in his Paris lectures, as well as in the earlier letter to Bohr, where he explicitly referred to the theory of X-ray spectra.

[876] In his 1962 AHQP Interview, Dirac claimed that originally he 'really felt that it [i.e., the mass of the hole] should be the same [as the mass of the electron],' but he did not accept it; hence, he never wrote about it before 1931.

[877] Annihilation processes resulting in energy production were a quite popular topic in those days and were especially advocated to solve the relevant problems in the theory of stars (Eddington, Jeans) and the theory of cosmic rays (Nernst, Millikan). See also Richard Tolman's paper explaining the observed expansion of the universe from an annihilation process (Tolman, 1930).

Dirac concluded:

> We cannot give an accurate numerical interpretation to our result [(676)] because we do not know whether the m there refers to the mass of the electron or of proton. Presumably it is some kind of mean. In any case the result [(676)] is much too large to agree with the known stability of electrons and protons. (Dirac, 1930b, p. 375)

Actually, the order of magnitude of the cross section turned out to be consistent with the size of electron or proton for very high energies, while it became infinite for zero velocities of the particles; hence, Dirac concluded that 'the interaction between the electron and proton, which has been neglected, very considerably reduces the collision area, at any rate for ordinary velocities' (Dirac, *loc. cit.*). Igor Tamm, with whom Dirac kept closest contact in those days, independently treated similar items and extended Dirac's dispersion theory of 1927 to the scattering of light in solids (Tamm, 1930a); in the calculation of the Compton effect according to the Heisenberg–Pauli quantum electrodynamics, he essentially confirmed the result of Klein and Nishina, as he reported to Dirac in a letter of 5 February 1930. In return, Dirac wrote to him on 21 February about his new results, which he then submitted later in March (Dirac, 1930b). Tamm wrote back on 3 March and reported his own evaluation of the annihilation problem; the result did agree with that of Dirac's. At the same time, however, Tamm pointed out two 'main difficulties': '1. If one (tentatively and approximatively) applies the formula to the case of bound electrons, one gets a ridiculously small value for the lifetime of the atoms, and 2. The frequency of the radiation emitted, when an electron drops in a hole, is of the order of magnitude of mc^2/h, where m is the mass of the electron, and that cannot be reconciled with the existence of cosmic rays.' (Tamm to Dirac, 3 March 1930, in Kojevnikov, 1993, p. 37) Dirac, of course, was content with the result, though he criticized Tamm's identification of the mass m with the electron mass (Dirac to Tamm, 20 March 1930).

Besides Igor Tamm in Russia, J. Robert Oppenheimer in the USA concerned himself with Dirac's new hole theory. After seeing Dirac's published paper on hole theory in January 1930, Oppenheimer sent a letter to the *Physical Review* on 14 February, in which he stressed 'several grave difficulties': First, he claimed that the theory would require an infinite density of positive electricity, 'otherwise the scheme proposed would not give Thomson's formula [for the scattering of electrons]'; second, the scattering of soft radiation by protons would not yield the correct Thomson result (but rather the one known for electrons); third, the mean lifetime of 10^{-10} seconds in ordinary matter could not be reconciled with experience (Oppenheimer, 1930b, especially, p. 562). He announced the detailed calculation to be given in a forthcoming paper, which he submitted in early March again to *Physical Review* (Oppenheimer, 1930c). In the case of correction for the different proton (M) and electron masses (m), the evaluation yielded the result

$$T = \frac{(m+M)^2 c^3}{64\pi^5 e^4 n_p} \sim \frac{5\times 10^{16}}{n_p}. \tag{677}$$

If n_p, denoting the number of protons per unit volume (about 10^{25}), was inserted, this gave indeed 5×10^{-10} seconds, 'an absurdly short mean lifetime for matter,' which would not be brought to agree with reality by any possible interaction between electron and proton (Oppenheimer, *loc. cit.*, p. 943).[878]

The grave difficulties mentioned by Tamm and Oppenheimer did not prevent Dirac from presenting his hole theory, *including the proton interpretation*, publicly at the Bristol meeting of the *British Association* on 8 September 1930. There, he gave a talk with the title 'The Proton,' and introduced it with a remark on the proton–electron structure of the atomic nucleus and the difficulties following for the statistics of the nitrogen nucleus; after expressing the then fashionable view that in some way the difficulty would disappear, he continued:

> It has always been the dream of philosophers to have all matter built up from one fundamental kind of particle, so that it is not altogether satisfactory to have two in our theory. There are, however, reasons for believing that the electron and proton are really not independent, but are just two manifestations of one elementary kind of particle. This connexion between the electron and proton is, in fact, rather forced upon us by general considerations about the symmetry between positive and negative electric charge, which symmetry prevents us from building up a theory of the negatively charged electrons without bringing in also the positively charged protons. (Dirac, 1930e, p. 605)

Following this *credo* about the *one fundamental particle* constituting all matter, Dirac then briefly outlined the contents of the hole theory; he showed especially how a hole can be made to disappear by having it filled by a negative-energy electron, thus, the hole must have positive energy; since it behaves like a positively charged particle (having the same absolute charge as the electron), it is 'reasonable to assert that the *hole is a proton*' (Dirac, *loc. cit.*). In referring to the known difficulties, Dirac considered the infinite-density problem with the negative-energy electrons to be solved (in Dirac, 1930a, as we have mentioned earlier), while the large annihilation probability for electron-hole pairs might be removed in future. Only the very different masses of the electron and the proton still caused him great headache. He did not believe in the way out indicated by Oppenheimer in his February letter, namely:

> Thus we should hardly expect any states of negative energy to remain empty. If we return to the assumption of two independent elementary particles, of opposite charge and dissimilar mass, we can resolve all the difficulties raised in this note, and retain the hypothesis that the reason why no transitions to states of negative energy occur, either for electrons or protons, it is that *all* such states are filled. (Oppenheimer, 1930b, p. 563)

[878] In the second part of his paper, Oppenheimer evaluated the relative probability for radiative and radiationless transitions on Dirac's new theory, obtaining an expression basically equivalent to that derived on the Heisenberg–Pauli electrodynamics.

However, such a reconciliation with obvious experimental facts, which would allow one to give the proton an arbitrary mass, contradicted Dirac's intentions who 'would like, if possible, to preserve the connection between the proton and the electron ... as it accounts in a very satisfactory way for the fact that the electron and proton have charges equal in magnitude and opposite in sign' (Dirac, 1930e, p. 606). In his talk at Bristol, Dirac rather hoped for further advances in quantum electrodynamics or a new idea to settle the problem satisfactorily.

While Dirac's unified electron–proton theory initially seemed to allow an interesting explanation of the beta-decay problem by applying a sort of Auger effect to negative-energy levels of the nucleus (Ambartsumian and Iwanenko, 1930), the opposition against it grew among some of his most respected colleagues. On 13 September 1930, Igor Tamm reported in a letter to Paul Dirac the news about the 1st Congress of Soviet Physicists in Odessa, held from 19 to 24 August.[879] In particular, he wrote:

> I met Pauli and was pleased to make his acquaintance. Pauli told us that he has rigorously proved that the system consisting of m positive electrons and n "holes" in the distribution of the negative-energy electrons has the same energy as the system consisting of m holes and n electrons, the electrons having the velocities which previously belonged to the holes and *vice versa*. Pauli concludes that on your theory of protons the interaction of electrons cannot destroy the equality of the mass of an electron and a proton. I would be very pleased to hear that Pauli is wrong. (Tamm to Dirac, 13 September 1930)

More than by the news about Pauli's calculation, Dirac was shaken by the arguments put forward by the mathematician Hermann Weyl in the second edition of his book *Gruppentheorie und Quantenmechanik* (Weyl, 1931b). Weyl, who, in 1929, had proposed the identification of the negative-energy states with protons, now wrote:

> However attractive this idea may seem to be at first, it is certainly impossible to hold without introducing other profound modifications to square our theory [of electrons and protons] with the observed facts. Indeed, according to it the mass of the proton should be the same as the mass of the electron; furthermore, no matter how the action is chosen (so long as it is invariant under interchange of right and left), this hypothesis leads to the essential equivalence of positive and negative electricity under all circumstances—even on taking the interaction between matter and radiation rigorously into account. (Weyl, *loc. cit.*, p. 234; English translation, p. 263)

To demonstrate the correctness of this claim, Weyl considered the behaviour of the terms of the action functions under substitutions interchanging the past and

[879] Dirac had visited the Soviet Union several weeks earlier and participated, at the end of June, in a small meeting at the Ukrainian Physico-Technical Institute in Kharkov; however, he had to leave Russia before 27 July because his visa had expired. (For details of this visit, see Kojevnikov, 1993, pp. 40–41).

the future (as was connected with Dirac's interpretation of the negative-energy states). Though he noticed that '*past and future play essentially different roles in the quantized field equations*' (Weyl, *loc. cit.*), he also found that 'this substitution neither affects the coordinates nor disturbs the quantized wave equations'; hence:

> In view of Dirac's theory of the proton this means that positive and negative electricity have essentially the same properties in the sense that the laws governing them are invariant under a certain substitution which interchanges the quantum numbers of the electrons with those of the protons. The dissimilarity of the two kinds of electricity thus seems to hide a secret of Nature which lies yet deeper than the dissimilarity of past and future. (Weyl, *loc. cit.*; English translation, p. 264)

This mathematical argumentation—stressed already in the introduction of Weyl's book as 'a new crisis of quantum physics' (see p. x of the English translation)—ultimately convinced Dirac to abandon his cherished theory. As he later stated:

> Weyl was a mathematician.... He was just concerned with the mathematical consequences of an idea, working out what can be deduced from the various symmetries. And this mathematical approach led directly to the conclusion that the holes would have to have the same mass as electrons. (Dirac, 1971, p. 55)

In May 1931, Dirac submitted another paper to the *Proceedings of the Royal Society*, dealing with 'Quantized Singularities in the Electromagnetic Field,' in which he explicitly withdrew the proton hypothesis (Dirac, 1931c). Referring to the arguments of Weyl (1931b), Tamm (1930b), Oppenheimer (1930b), and himself, he now drew the conclusion:

> It thus appears that we must abandon the identification of the holes with protons and must find some other interpretation for them. Following Oppenheimer [1930b], we can assume that in the world as we know it, all, and not nearly all, of the negative-energy states for electrons are occupied. A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron. (Dirac, 1931c, p. 61)

The reason why this 'anti-electron,' as Dirac baptized the new kind of particle, had not been detected before, lay, he claimed, in 'their rapid rate of recombination with electrons'—as he, Tamm, and Oppenheimer had demonstrated already since sometime. However, 'if they could be produced experimentally in high vacuum,' Dirac continued, 'they would be quite stable and amenable to observation,' and 'an encounter between two hard γ-rays (of energy at least half a million volts) could lead to the creation simultaneously of an electron and an anti-electron, the probability of occurrence of this process being of the same order of magnitude as that of the collision of the two γ-rays on the assumption that they are spheres of the same size as classical electrons' (Dirac, *loc. cit.*, pp. 61–62). However, Dirac regretted that the probability in question still appeared to be negligible with the then available intensities of γ-rays. (Dirac, *loc. cit.*, p. 62) Independently of the difficulties of producing the new particles, he now concluded: Protons must be viewed as unconnected with electrons, and both the protons and the electrons have their own negative-energy states which should be interpreted as anti-protons and anti-electrons, respectively. Thus, Dirac's paper of May 1931 expounded the concept of antimatter for particles obeying his relativistic equation.

The main content of the paper under discussion was not this conclusion, important as it was considered ever since, but 'a new idea which is in many respects comparable with this one about negative energies,' Dirac maintained (Dirac, *loc. cit.*, p. 62). Indeed, he claimed to need such a new idea in order to explain 'the reason for the existence of a smallest electric charge' that was experimentally determined by the relation

$$hc/2\pi e^2 = 137. \tag{678}$$

This reason, he argued in particular, might be recognized immediately if one connected the smallest electric charge e with 'the smallest magnetic pole,' assuming 'a symmetry between electricity and magnetism quite foreign to current views' (Dirac, *loc. cit.*). Certainly, however, he also admitted that the symmetry envisaged need not be complete, but:

> Without this symmetry, the ratio of the left-hand-side of Eq. [(678)] remains, from the theoretical standpoint, completely undetermined and if we insert the experimental value 137 in our theory, it introduces quantitative differences between electricity and magnetism so large that one can understand why their qualitative similarities have not been discovered experimentally up to the present. (Dirac, *loc. cit.*)

In order to formulate the proposed new idea, Dirac started from the fact that a wave function ψ is determined only up to a phase factor exp $(i\gamma)$, or

$$\psi = \psi_1 \exp(i\gamma), \tag{679}$$

where ψ_1 is an ordinary wave function with a definite phase at each point (x, y, z, t). Now, in the special case that γ represents a nonintegrable function of the space and time variables, the physical interpretation demanded: '*The change in phase of the wave function round any closed curve must be the same for all wave functions.*' (Dirac, *loc. cit.*, p. 63) Hence, Dirac continued, 'this phase must be independent of which state of the system is considered,' or more specifically: 'As our dynamical system is merely a single particle, it appears that the non-integrability of the phase must be connected with the field of force in which the particle moves.' (Dirac, *loc. cit.*, p. 64) The γ-factor in Eq. (679) does not really have a fixed value at any space-time point but possesses the definite derivatives

$$\boldsymbol{\kappa} = (\partial\gamma/\partial x, \partial\gamma/\partial y, \partial\gamma/\partial z) \quad \text{and} \quad \kappa_0 = \partial\gamma/\partial t. \tag{680}$$

Consequently, the change of phase round a closed curve may be written as (with (,) denoting the scalar product of the vectors involved)

$$\Delta\gamma = \oint(\boldsymbol{\kappa},\, ds) = \int(\mathrm{curl}\ \boldsymbol{\kappa}, d\mathbf{S}), \tag{681}$$

respectively, where the line integral is replaced (via Stokes's theorem) by a surface integral (actually **S** denotes a six-vector). Evidently, if ψ satisfies the usual time-dependent Schrödinger equation, ψ_1 satisfies another, in which the space and time derivatives are replaced by

$$-\frac{ih}{2\pi}\frac{\partial}{\partial x}\psi \rightarrow \left(-\frac{ih}{2\pi}\frac{\partial}{\partial x} + \frac{h}{\pi}\kappa_x\right)\psi_1 \ldots \tag{682}$$

and

$$\frac{ih}{2\pi}\frac{\partial}{\partial t}\psi \rightarrow \left(\frac{ih}{2\pi}\frac{\partial}{\partial t} - \frac{h}{2\pi}\kappa_0\right)\psi_1. \tag{682a}$$

In the case of an electron of charge $-e$ moving in an electromagnetic field ($\mathbf{A}$, A_0), Dirac identified the $\boldsymbol{\kappa}$ and the κ_0 with

$$\kappa_x = \frac{2\pi e}{hc}A_x, \ldots, \kappa_0 = -\frac{2\pi e}{h}A_0. \tag{683}$$

Thus, for the phase change round a closed loop in the three-dimensional space, he arrived at the expression

$$\Delta\gamma = \frac{2\pi e}{hc}\int(\mathbf{H}, d\mathbf{S}), \tag{684}$$

with **H** denoting the magnetic-field vector.

So far, the considerations only reproduced, as Dirac remarked, the modern formulation of the gauge-invariance principle, as had been given previously by Hermann Weyl (1929b) and Vladimir Fock (1929). But quantum mechanics allowed for much more than the conventional results, which Dirac showed in §3 of his paper (1931c). In particular, he noted (Dirac, *loc. cit.*, p. 66) that 'a further fact must be taken into account, namely that a phase [in Eq. (679)] is always undetermined to the extent of an arbitrary integral multiple of 2π,' and: 'This requires a reconsideration of the connection between the κ's and the potentials and leads to a new physical phenomenon.' In particular, Eq. (684) would be replaced (for a 'large' closed path) by

$$\Delta\gamma = 2\pi\Sigma n + \frac{2\pi e}{hc}\int(\mathbf{H}, d\mathbf{S}), \tag{684$'$}$$

where n were the integer numbers associated with 'small' closed curves making up, in a network, the 'large' one. Since the left-hand side of Eq. (684′), when applied to a closed surface, must vanish, Dirac concluded that 'Σn summed for all nodal lines [arising from the zeroth of the complex wave functions] crossing a closed surface [in three-dimensional space] must be the same for all wave functions and must equal $-\frac{e2\pi}{hc}$ times the total magnetic flux crossing the surface' (Dirac, *loc. cit.*, p. 68). He then continued: 'If Σn does not vanish, some nodal lines must have end points inside the closed surface, since a nodal line without such end point must cross the surface twice (at least) and will contribute equal and opposite amounts of Σn at the two points of crossing.' (Dirac, *loc. cit.*) Hence, a finite value for Σn would give the sum of n for all nodal lines inside the surface having end points, and this sum must be the same for all wave functions. In a physical interpretation, this result meant that '*these end points are then end points of singularity in the electromagnetic field*,' whose nature can be derived from calculating the flux of the magnetic field crossing a small surface surrounding one of the points yielding

$$4\pi\mu = nhc/e. \tag{685}$$

Or, 'at the end point there will be a magnetic pole of strength $\mu = \frac{nhc}{4\pi e}$.' (Dirac, *loc. cit.*) Dirac concluded:

> Our theory thus allows isolated magnetic poles, but the strength of such poles must be quantized, the quantum μ_0 being connected with the electric charge e by
>
> $$\frac{hc}{2\pi e\mu_0} = 2. \qquad [(686)]$$
>
> (Dirac, *loc. cit.*)

In the next section, Dirac illustrated how a magnetic monopole would act in quantum mechanics. Evidently, the electromagnetic field equations (683) were not satisfied around the magnetic pole, but he succeeded in writing the Schrödinger equation for an electron in the magnetic field of a monopole. Though he did not arrive at a solution of this equation, he observed that 'there can be no stable states for which the electron is bound to the magnetic pole' (Dirac, *loc. cit.*, p. 70).[880] Dirac concluded by noting that, although in classical electrodynamics the equations of motion can be written in a Hamiltonian form 'only when there are no isolated magnetic poles,'

> quantum mechanics does not really preclude the existence of isolated magnetic poles. On the contrary, the present formalism of quantum mechanics, when developed

[880] Immediately afterward, Igor Tamm treated the problem and obtained the general solution of Dirac's equation (Tamm, 1931).

> naturally without the imposition of arbitrary restrictions, leads inevitably to wave equations whose only physical interpretation is the motion of an electron in the field of a single pole. This new development requires *no change whatever* in the formalism when expressed in terms of abstract symbols denoting states and observables, but is merely a generalization of the possibilities of representation of these abstract symbols by wave functions and matrices. Under these circumstances one would be surprised if Nature had made no use of it. (Dirac, *loc. cit.*, p. 71)

(c) The Discovery of New Elementary Particles of Matter and Antimatter (1930–1933)

In his letter of 4 December 1930, to the 'radioactive ladies and gentlemen' assembled at the Tübingen *Gauverein* meeting of the German Physical Society, Wolfgang Pauli had proposed the existence of an electrically neutral particle of spin 1/2 (in units of $h/2\pi$) in order to solve the problem of the continuous β-emission. He called this particle the 'neutron' and attributed to it a small mass of the order of magnitude of an electron mass, certainly not much higher than 1 percent of the proton mass.[881] Pauli's 'neutron,' which he would use also to explain the wrong statistics of certain nuclei, soon came into conflict with a rather different neutron which had been proposed somewhat earlier for quite another purpose. We shall first deal with the story of the latter, while the discussion of the former will be dealt with in the next part of this section.

Apparently, the first scientist, who explicitly talked about a 'neutron,' was Walther Nernst.[882] In the fourth section of his textbook *Theoretische Chemie* of 1909, Nernst introduced a new chapter on '*Die atomistische Theorie der Elektrizität* (The Atomistic Theory of Electricity)'; starting from Hermann von Helmholtz's Faraday lecture of 1881, he assumed the existence of positive and negative elementary particles ('electrons') to describe the behaviour of chemical substances. He then continued:

> Whether also the compound of a positive and a negative electron ($\oplus\ominus$ = neutron, electrically neutral massless molecule) possesses a real existence, is evidently quite an important question. We wish to assume that neutrons may exist everywhere, just like the light-aether; and we may add that a space filled with these molecules must be imponderable, electrically non-conducting, but polarizable, i.e., it should possess properties which physics moreover claims for the light-aether. (Nernst, 1909, p. 400)

Thus, the physicochemist, Nernst, connected his 'neutron' with the electromagnetic aether, a speculation which was later revived in a different form by William Henry Bragg when he proposed to consider γ-rays and highly energetic X-rays 'to consist of neutral pairs' of positive and negative electrons (W. H. Bragg, 1907, p. 441). Several years later, Antonius Johannes Van den Broek, the codiscoverer

[881] See Pauli, 1985, p. 39, and our discussion in Section III.7.

[882] The prehistory of the neutron has been summarized in a review by Bernd Kröger (1980).

of the concept of atomic number, first thought about the existence of groups of massive neutral particles in atomic nuclei; in particular, he considered the neutral helium particles (Van den Broek, 1915). Then, William D. Harkins, a professor of physical chemistry at the University of Chicago, introduced—in a paper submitted in April 1920 to the *Journal of the Chemical Society*—the existence of 'atoms of zero atomic number' which might have 'masses of 4, 3, 2 and 1, and possibly other values,' in order to explain the recent experiments of Rutherford on the reactions of atomic nuclei (Harkins, 1920, p. 1996). More concretely, Ernest Rutherford had said in his Bakerian lecture in June of the same year:

> It seems very likely that one electron can also bind two H nuclei and possibly one H nucleus. In the one case, this entails the possible existence of an atom of mass nearly 2 carrying one charge, which is to be regarded as an isotope of hydrogen. In the other case, it involves the idea of the possible existence of an atom of mass 1, which has zero nucleus charge.... If the existence of such atoms be possible, it is to be expected that they may be produced, but probably in very small numbers, in the electric discharge through hydrogen, where both electrons and H nuclei are present in considerable numbers. (Rutherford, 1920, p. 396)

Rutherford arranged suitable experiments to be done at his Cavendish Laboratory, but he and his collaborators did not obtain any result of the kind in the 1920's that he had envisaged. Neither the neutral atoms of mass 1 (which Harkins, *en passent*, named the 'neutron': 1921, p. 331), nor the predicted isotope of mass 2 was found. The discovery of both had to wait until the early 1930s.

The final story of events leading to the discovery of Rutherford's 'neutral mass-1 atom' began with the 'artificially excited nuclear γ-rays,' observed in fall 1930 by Walther Bothe and Herbert Becker at the *Physikalisch-Technische Reichsanstalt* in Berlin. Bothe and Becker bombarded several nuclei, from hydrogen to lead, with α-rays from a polonium source; in the case of Li, Be, B, F, Mg, and Al, they registered—with a Geiger counter (point-counter tube)—emerging secondary γ-rays, which for B and Be belonged 'in order of magnitude to the hardest γ-rays observed in radioactive decays' (Bothe and Becker, 1930, p. 289). In the particular case of beryllium (where a large γ-ray intensity resulted), a strong dependence of the excitation energy as a function of the energy of the incident α-rays resulted, though the hardness (i.e., energy) of the secondary radiation was not influenced at all. Bothe and Becker made use of Gamow's α-decay model (discussed in Section III.7) to describe the situation and concluded that 'a nuclear radiation can practically occur only in connection with ionization (smashing) or excitation of the nucleus'—because then the α-particles might be either absorbed or inelastically scattered (Bothe and Becker, *loc. cit.*, p. 302). They expected to obtain more accurate information by using a stronger Po probe. Indeed, in a later letter to *Naturwissenschaften*, dated 6 August 1931, refined absorption measurements of the Be-'radiation' (with iron and lead absorbers) were reported (Becker and Bothe, 1931). Becker and Bothe now determined its energy to be 14×10^6 eV as compared to 5.2×10^6 eV of the incident α-particles; this corresponded—if the motion of the nucleus were taken into account—to roughly 3.6×10^6 eV for the newly created 'gammas.' Consequently, the original α-particle had to be absorbed by the Be nucleus, leading to a gain of the negative binding energy, and Becker and Bothe proposed the following interpretation: 'Since the Be nucleus cannot be smashed, hence no secondary corpuscular radiation is emitted, one may conclude with good reason that the process represents a simple nuclear fusion, or $Be_9 + \alpha = C_{13}$.' (Becker and Bothe, *loc. cit.*, p. 753)

The results obtained by Becker and Bothe in Berlin aroused the interest of Irène Curie in Paris. On 21 December 1931, a note of hers was presented at the *Académie des Sciences*, in which she examined the 'nuclear γ-radiation' emitted from Be and Li upon bombardment with Po α-rays more closely; she determined (with the help of the Klein–Nishina formula that had also been used by Becker and Bothe, 1931) energies up to 15 to 20 MeV, which were much too large to be credible except in cosmic rays (I. Curie, 1931). Irène Curie and her husband Frédéric Joliot then allowed the 'radiation' to pass through a very thin window in an ionization chamber; they placed paraffin wax (i.e., a substance containing hydrogen) in front of it, and observed in early January 1932 an increased ionization due to the ejection of protons from the wax (I. Curie and F. Joliot, 1932a). They interpreted the proton energy, which was up to 4.5 MeV, as energy from a Compton effect with radiation having 50 MeV energy. In a second note, communicated to the *Académie des Sciences* on 11 April, they observed the protons' tracks in a cloud chamber and confirmed the high energy (I. Curie and F. Joliot, 1932b). In the same paper, they withdrew their previous Compton-effect explanation. In between, however, James Chadwick in Cambridge, who had seen their January communication, entered upon the stage. As he recalled many years later, he was immediately quite startled by the January note of Curie–Joliot's, and:

> Not many minutes afterwards, [Norman] Feather came to my room to tell me about this report.... A little later that morning I told Rutherford.... As I told him about the Curie-Joliot observation and their view of it, I saw his growing amazement and finally he burst out: "I don't believe it." (Chadwick, quoted in Kröger, 1980, p. 190)

The Cambridge group had followed already earlier the findings of Bothe and Becker with interest. 'Mr. H. C. Webster in the Cavendish Laboratory had also been making similar experiments, and he had proceeded to examine closely the production of these radiations,' Chadwick recalled in his Nobel lecture of December 1935, and further noted: 'I suggested ... that the radiation [emitted by beryllium] might consist of neutral particles and that a test of this hypothesis might be made by passing the radiation into an expansion [cloud] chamber.' (Chadwick, 1965, p. 340) But the photographs taken in 1930 or 1931 yielded nothing spectacular (due to the weakness of the polonium source, as found later). Now, the Curie–Joliot results led Chadwick to the decisive breakthrough, which he reported in a letter to *Nature*, dated 17 February 1932:

> I made some experiments by using the valve counter to examine the properties of the radiation excited. The valve counter consists of a small ionization chamber connected to an amplifier, and the sudden production of ions by the entry of a particle, such as a proton or α-particle, is recorded by the deflection of an oscillograph. These experiments have shown that the radiation [emitted from beryllium] ejects [secondary] particles from hydrogen, helium, lithium, beryllium, carbon, air, and argon. The particles ejected from hydrogen behave, as regards range and ionization power, like protons with speeds up to about 3.2×10^9 cm per sec. The particles from the other elements have a large ionization power, and appear to be in each case recoil atoms of the elements. (Chadwick, 1932a, p. 312)

Now, the real interpretation of the Cambridge result seemed to be evident. If Chadwick assumed that the recoil protons arose from the Compton effect of γ-rays (as his predecessors had claimed), both energy and ionization power should be much lower than observed. Also, the study of the recoil nuclei in a cloud chamber (which Chadwick carried out with his student Feather) required very high energy; hence, the previous interpretation appeared to be 'very difficult,' if energy and momentum conservation applied. However, Chadwick proceeded in his letter:

> The difficulties disappear if it is assumed that the radiation [excited by α-particles in beryllium] consists in particles of mass 1 and charge zero, or neutrons. The capture of the α-particle by the Be^9 nucleus may be supposed to result in the formation of a C^{12} nucleus and the emission of the neutron. From the energy relation of this process the velocity of the neutron emitted in the forward direction may well be about 3×10^9 cm per sec. The collision of this neutron with the atoms through which it passes gives rise to the recoil atoms, and the observed energies of the recoil atoms are in fair agreement with this view. (Chadwick, *loc. cit.*)

The observation of protons in an opposite direction to that of the incoming objects (from beryllium) with much smaller range confirmed the new interpretation. On the other hand, the claim of Becker and Bothe (in August 1931) to have obtained a C^{12} nucleus could be excluded on account of the known mass defect and energy conservation.

The news from Cambridge was received with the greatest interest. For example, Franco Rasetti—then at Lise Meitner's *Kaiser Wilhelm-Institut für Chemie* in Berlin—investigated the case of beryllium both in a Wilson cloud chamber and in a coincidence experiment. From the latter, he concluded that 'the particles creating the coincidences behave like electrons having some million electron-volts of energy, which cannot be explained by the neutron hypothesis,' but quite well 'as Compton electrons of a γ-radiation *of roughly 10 million electron-volts*' (Rasetti, 1932, p. 253). Thus, he pleaded in favour of a more (complex) Be-radiation, consisting of a mixture of γ-quanta and neutrons. His letter of 15 March appeared in the *Naturwissenschaften* issue of 1 April; six weeks later, the *Naturwissenschaften* published a letter of Becker and Bothe, dated 15 April 1932, who insisted that they had observed in their own experiments only γ-radiation (Becker and Bothe, 1932a). But in the beginning of May 1932, James Chadwick presented his neutron interpretation in full detail in a paper entitled 'The Existence of a Neutron' and published in the June issue of the *Proceedings of the Royal Society* (Chadwick, 1933b).

Chadwick condensed his proof as follows:

We have

$$\mathrm{Be}^9 + \mathrm{He}^4 + \text{kinetic energy of } \alpha$$
$$= \mathrm{C}^{12} + \mathrm{n}^1 + \text{kinetic energy of } \mathrm{C}^{12} + \text{kinetic energy of } \mathrm{n}^1. \qquad [(687)]$$

If we assume that the beryllium nucleus consists of two α-particles and a neutron, then its mass cannot be greater than the sum of the masses of these particles, for the binding energy corresponds to a defect mass. The energy equation becomes

$$(8.00212 + \mathrm{n}^1) + 4.00106 + \text{kinetic energy of } a$$
$$> 12.0003 + \mathrm{n}^1 + \text{kinetic energy of } \mathrm{C}^{12} + \text{kinetic energy of } \mathrm{n}^1 \qquad [(688)]$$

or

$$\text{kinetic energy of } \mathrm{n}^1 + \text{kinetic energy of}$$
$$\alpha + 0.003 - \text{kinetic energy of } \mathrm{C}^{12}. \qquad [(689)]$$

Since the kinetic energy of the α-particle of polonium is 5.25×10^6 electron-volts, it follows that the energy of the emission of a neutron cannot be greater than about 8×10^6 electron-volts. The velocity of the neutron is about 3.3×10^9 cm per second, so that the proposed disintegration process is compatible with observation. (Chadwick, *loc. cit.*, p. 699)

Chadwick explained the Rasetti coincidences by the γ-radiation emitted from an excited C^{12} nucleus (see Chadwick, *loc. cit.*, p. 707).[883]

From his experimental investigations, Chadwick also determined the nature and properties of the neutron (in §4 and §5 of Chadwick, *loc. cit.*). Thus, he derived the mass from the reaction $\mathrm{B}^{11} + \mathrm{He}^4 \rightarrow \mathrm{N}^{14} + \mathrm{n}^1$, namely:

$$\text{mass of } \mathrm{B}^{11} + \text{mass of } \mathrm{He}^4 + \text{kinetic energy of } \mathrm{He}^4$$
$$= \text{mass of } \mathrm{N}^{14} + \text{mass of } \mathrm{n}^1 + \text{kinetic energy of } \mathrm{N}^{14}$$
$$+ \text{kinetic energy of } \mathrm{n}^1. \qquad [(690)]$$

Unlike the mass of Be^9, the masses of the nuclei B^{11} and N^{14} had been determined already quite well. Thus, he obtained a value of 1.005 to 1.008 in the mass units of Aston, a value below the sum of the masses of proton and electron, as he expected

[883] In a further letter to *Naturwissenschaften*, dated 3 September 1932, Becker and Bothe again insisted on the fact that their counter-experiment just registered γ-radiation (Becker and Bothe, 1932b).

from the view of the neutron as being a proton–electron bound state with a radius of the order of 10^{-13} cm.[884] Because of its zero charge—the electrical field should be negligible at least down to distances of the order of 10^{-12} cm—the neutron would be able to penetrate into nuclei. Chadwick further concluded that neutron–nucleus scatterings would occur very rarely as compared to Coulomb scattering. Preliminary tests seemed to confirm these conclusions: Collisions with a proton turned out to be more frequent than those with nuclei of light atoms, and those with electrons occurred only very rarely (see Dee, 1932).[885] Finally, he drew another, important conclusion:

> Although there is certain evidence for the emission of neutrons only in two cases of nuclear transitions [namely the α-particle scattering on Be^9 and B^{11}], we must nevertheless suppose that the neutron is a common constituent of atomic nuclei. We may then proceed to build up nuclei out of α-particles, neutrons and protons, and we are able to avoid the presence of uncombined electrons in a nucleus. This has certain advantages for, as is well known, the electrons in a nucleus have lost some properties which they have outside, e.g., their spin and magnetic moment. (Chadwick, 1932b, p. 706)

This important conclusion—one may rather call it a hypothesis—solved the great puzzles of the previous theories of nuclear structure, which we have discussed in Section III.7. Chadwick went on to argue further in favour of his hypothesis:

> If the α-particle, the neutron, and the proton are the only units of nuclear structure, we can proceed to calculate the mass defect or building energy of a nucleus as the difference between the mass of the nucleus and the sum of the masses of the constituent particles. It is, however, by no means certain that the α-particle and the neutrons are the only complex particles in the nuclear structure, and therefore the mass defects calculated this way may not be the true binding energies of the nuclei. In this connection it may be noted that the examples of disintegration discussed by Dr. Feather in the next paper [Feather, 1932] are not at all of one type, and he suggests that in some cases a particle of mass 2 and charge 1, the hydrogen isotope recently reported by Urey, Brickwedde and Murphy, may be emitted. It is indeed possible that this particle also occurs as a unit of nuclear structure. (Chadwick, *loc. cit.*)

With these last remarks, Chadwick referred to the originally quite surprising observation, which had been reported in a short note by Harold Urey, Ferdinand Brickwedde, and G. M. Murphy of Columbia University and the National Bureau of Standards, signed on 5 December 1931, and published in the 1 January 1932, issue of the *Physical Review*: From an analysis of atomic spectra of fractionated liquid hydrogen in a discharge tube, they derived the existence of a hydrogen iso-

[884] This had been Rutherford's conception of the neutron in 1920.

[885] Chadwick noted (1932b, p. 704) that these experiments were carried out by several of his collaborators, notably, Dr. Gray and Mr. Lea.

tope having a mass of about 2 and a relative abundance in natural water of 1 : 4000 (Urey, Brickwedde, and Murphy, 1932).[886] While this stable isotope would play, similar to the likewise strongly bound He^4 isotope, a great role in the discussion of nuclear structure and forces, the theoreticians—especially Dmitrij Iwanenko (1932a, b) and Werner Heisenberg (1932b, c)—would first pick up the idea of building up nuclei simply of protons and neutrons. Notwithstanding the details of the further development, Chadwick's discovery in Cambridge opened a new era in nuclear physics, not only by explaining naturally the surprising observation of the heavy hydrogen isotope, but by giving rise to a consistent quantum-mechanical theory of nuclear constitution involving a new concept of nuclear forces, which in turn led even to a further insight into the structure of matter and the existence of new elementary particles.

The first half of the year 1932, especially the month of February, proved to be an even more successful period for the Cavendish Laboratory concerning experiments on nuclear physics. In these, another student and collaborator of Ernest Rutherford's, namely, John Cockcroft, became involved. At the turn of the year from 1928 to 1929, immediately after obtaining his doctorate, Cockcroft had proposed—based on the stimulation received from George Gamow's nuclear theory—to construct an apparatus to accelerate protons and α-particles beyond the energies obtained from nuclear transformations and obtained the help of Ernest Thomas Sinton Walton as collaborator.[887] By January 1932, their accelerator machine was ready, and they reported in a letter published in the 13 February issue of *Nature*:

> For maximum energy of protons produced up to the present has been 710 kilovolts.... We do not anticipate any difficulty in working up to 800 kilovolts with our present apparatus. (Cockcroft and Walton, 1932a, p. 242)

They described more details in a paper, which Lord Rutherford communicated on 23 February to the *Proceedings of the Royal Society* (Cockcroft and Walton, 1932b). They pointed out that the high voltage was created in a cascade circuit built of a series of four condensers, such that a voltage multiplication resulted.[888]

[886] Harold Urey would receive the Nobel Prize for Chemistry in 1934. Born on 29 April 1893, in Walterton, Indiana, Urey studied zoology and chemistry at Montana State University after serving (from 1911 to 1914) as a high school teacher; then, he joined the University of Montana (except from 1917 to 1919 when he worked as a research chemist at Barret Company, Baltimore). He continued his studies at the University of California at Berkeley, where he received his Ph.D. in 1923. He spent the year 1923–24 with Niels Bohr in Copenhagen on an American–Scandinavian Foundation fellowship; then, he worked at Johns Hopkins University as a research associate. In 1929, Urey received a professorship at Columbia University (associate professor, 1929–1934; professor, 1934–1945), during which period he participated as the leading expert in isotope chemistry in the Manhattan Project in World War II. Then, he joined the University of Chicago and in 1958 the Scripps Institute for Oceanography in San Diego. He died on 6 January 1981, in San Diego.

[887] For the biographical data on Cockcroft and Walton and the beginning of their accelerator enterprize, see Volume 4, pp. 35–36.

[888] The principle of this voltage multiplication goes back to the Swiss physicist Heinrich Greinacher, who applied it first in 1914 (Greinacher, 1914)—see Volume 5, Part 1, p. 284. Hence, this part of the Cockcroft–Walton machine would occasionally be referred to as the 'Greinacher circuit.'

The high voltage was then applied to an experimental tube allowing positive ions to be accelerated; the accelerated ions were finally directed into a chamber screened from electric fields, where they hit a target consisting of different substances. Originally, Cockcroft and Walton used beryllium as the target (because of the then recent interest in that substance), but they had troubles in detecting any result originally, namely, a luminescence in the reaction chamber. Ultimately, Rutherford, who urgently desired results, insisted that Cockcroft and Walton should observe the reaction with his favourite method, using a fluorescent zinc sulphide screen, which had worked so well in the detection and counting of α-particles earlier. Until June 1932, Rutherford's coworkers confirmed a number of artificial nuclear transitions created by the bombardment with their accelerated protons, starting with targets of lithium (which subsequently broke into two α-particles), beryllium, boron, fluorine up to uranium (Cockcroft and Walton, 1932c). They had thus provided the nuclear physicists with a new, powerful instrument to obtain controlled disintegration of nuclei, which Rutherford proudly demonstrated to many visitors who came in 1932 and the following years to his flourishing laboratory.[889]

It should be mentioned that the Cockcroft–Walton method did not constitute the first serious approach described in the literature to accelerate charged particles, like protons and electrons, to high energies. These approaches were initiated from two sides: namely, from the Norwegian Rolf Wideroe and others who developed the 'betatron' idea in the 1920's for specifically accelerating electrons, and from Robert J. Van de Graaff, National Research Fellow at Princeton University, whose electrostatic generator should work in principle for all charged particles. At the Schenectady meeting of the American Physical Society in September 1931, Van de Graaff introduced an apparatus which provided 1,500,000 volts, 'a powerful means for the investigation of the atomic nucleus and other fundamental problems' (Van de Graaff, 1931, p. 1919). Then, on 20 February, a few weeks after Cockcroft and Walton announced their first results in Cambridge, the *Physical Review* received a detailed paper of Ernest O. Lawrence and M. Stanley Livingston from Berkeley in which they announced the invention of their 'cyclotron' (1932). However, the application of this method, which would provide even a wider application in nuclear and high-energy physics than the Cockcroft–Walton apparatus, in the case of producing nuclear reactions—namely, the scattering of protons by lithium nuclei—came later than that of the Cambridge team, notably, in a note submitted on 15 September 1932, to *Physical Review* (Lawrence, Livingston, and White, 1932).[890]

While Great Britain surpassed America in obtaining the first high-voltage in-

[889] Nearly 20 years after their work, Cockcroft and Walton were honoured with the 1951 Nobel Prize for Physics; they were cited for having 'produced a totally new epoch in nuclear research' (from the Presentation Speech of Ivar Waller, reprinted in *Nobel Foundation*, ed., 1964, p. 165).

[890] We shall talk more about the development of particle accelerators in Section IV.5.

duced nuclear reactions, the New World soon answered with another pioneering deed: the discovery of antimatter, in particular, the experimental proof of Dirac's 'anti-electron.' Nearly seven months later, Carl Anderson recalled the moment of discovery:

> On August 2, 1932, during the course of photographing cosmic ray tracks produced in a vertical Wilson chamber (magnetic field of 15,000 gauss) designed in summer 1930 by Professor Millikan and the writer, the tracks shown in Fig. 1 were obtained, which seemed to be interpretable only on the basis of the existence in this case of a particle carrying a positive charge but having a mass of the same order of magnitude as that normally possessed by a free negative electron. Later studies of the photograph by a whole group of men of the Norman Bridge Laboratory only tended to strengthen this view. (Anderson, 1933c, p. 491)

The public came to know about this finding in a short 'special article,' signed by Anderson on 1 September 1932, and published under the title 'The Apparent Existence of Easily Deflectable Positives' (Anderson, 1932b). There he reported:

> In measuring the energies of charged particles produced by cosmic rays, some tracks have recently been found which seem to be produced by positive particles, but if so the masses of these particles must be small compared to the mass of the proton. The evidence for this statement is found in several photographs, three of which are discussed below.
>
> . . .
>
> The interpretation of these tracks as due to protons, or other heavier nuclei, is ruled out on the basis of range and curvature. Protons or heavier nuclei of the observed curvatures could not have ranges as great as those observed. The specification is close to that of an electron of the same curvature, but indicating a positively-charged particle comparable in mass and magnitude of charge with an electron. (Anderson, *loc. cit.*, pp. 238–239)

In retrospect, Anderson's observations of August 1932 have been celebrated generally as the discovery of Dirac's hypothetical 'anti-electron' proposed more than a year previously (Dirac, 1931c).[891] A closer look at the historical events tells a much more complex story, consisting rather of a sequence of discoveries and beginning several years before Anderson's particular result and continuing far into the year 1933.[892] Since the mid-1920s, Robert A. Millikan had chosen the investigation of cosmic radiation as the central task of his experimental programme; especially, he attempted to support the hypothesis that is consisted essentially of high-energy gamma radiation.[893] In some contrast to Millikan's assumption,

[891] See, e.g., Cahn and Goldhaber, 1989, pp. 5–6.

[892] Several historical accounts of the discovery of the anti-electron, or 'positron' (as Anderson would call it) have been given, especially those by Hanson (1963) and De Maria and Russo (1985).

[893] For a historical account, see, e.g., Xu and Brown, 1987.

Dmitri Skobeltzyn of Leningrad observed as early as spring 1927 tracks of charged particles in his cloud chamber, combined with a magnetic field, and published in early 1929 a detailed report (Skobeltzyn, 1927, especially, p. 377; 1929). Both he and, independently, Werner Kolhörster and Walther Bothe of Berlin, who developed the coincidence method with counters (Kolhörster, 1928; Bothe and Kolhörster, 1929), confirmed that cosmic radiation consisted partly of high-energy electrically charged particles.[894]

Then came Carl Anderson upon the scene, who later recalled the circumstances of a new cloud-chamber programme in Pasadena:

> At about the end of 1929, when it became clear to me that I was likely to receive my Ph.D. degree at Caltech in June 1930, I made an appointment to see Dr. Millikan. The purpose of my visit was to see if it were at all possible to spend one year more at Caltech as a postdoctoral research fellow. My reason for doing so was twofold: to carry out an experiment I had in mind and to learn something about quantum mechanics. (C. Anderson and H. Anderson, 1983, p. 135)

But Millikan decided that Anderson should rather continue his research work at another place, and (endowed with a National Research Council fellowship) Anderson decided to apply to A. H. Compton at the University of Chicago. However, several months later, Millikan changed his mind and strongly wished Anderson 'to spend one more year at Caltech to build an instrument to measure energies of the electrons present in cosmic radiation' (C. Anderson and H. Anderson, *loc. cit.*, pp. 136–137).[895] Having previously obtained some expertise in photographing secondary electrons in a cloud chamber, he began 'to work on the design of the instrument he [i.e., Millikan] had proposed for cosmic-ray studies': 'It was to consist of a cloud chamber operated in a magnetic field ... a very powerful magnetic field, for the cosmic-ray electrons were expected to

[894] See the report of Skobeltzyn (1981). Dmitri V. Skobeltzyn was born on 24 November 1892, in St. Petersburg and graduated from the University of Leningrad. In 1925, he became a research fellow of the Leningrad Polytechnical Institute. There, he began to investigate the Compton-effect electrons and later the cosmic-ray electrons, and spent some time (from 1929 to 1931) at Marie Curie's Paris laboratory. In the 1930s, he specialized on cascade studies in cosmic rays. Shortly before World War II, he moved to the Lebedev Physical Institute of the Soviet Academy of Sciences (Director from 1951 to 1973) in Moscow; there, he also founded the Institute of Nuclear Physics at the Moscow State University. He died on 16 November 1990, in Moscow.

[895] Carl Anderson was born of Swedish parents in New York on 3 September 1905. Graduating with a B.Sc. degree in physics and engineering from the California Institute of Technology in 1924, he stayed on there to work with Robert A. Millikan for the Ph.D.: His thesis contained a Wilson cloud chamber study of the space distribution of photoelectrons produced by X-rays in various gases (Anderson, 1929, 1930). In the period from 1930 to 1933, he was a research fellow, then an assistant professor, and finally, a full professor from 1939 until his retirement in 1976. During World War II (from 1941 to 1946), he served on projects of the Defense Research Committee and the Office of Scientific Research and Development. He died on 11 January 1991. (For more biographical information on Anderson, see *Nobel Foundation*, 1965, p. 377, and the brief obituary note 'Carl D. Anderson, 1905–1991' in *CERN Courier*, March 1991, p. 30.)

have energies in the range of at least several hundred million electron volts.' (C. Anderson and H. Anderson, *loc. cit.*, p. 137)

In November 1931, Millikan presented and discussed the first 11 cosmic-ray photographs taken by Anderson's new instrument at the *Institut Henri Poincaré* in Paris and at the Cavendish Laboratory in Cambridge (see Millikan and Anderson, 1932a, especially, p. 325). His conclusion—that is, 'all the tracks seem to be interpreted from the standpoint of the photon theory of the nature of the rays' (Millikan and Anderson, 1932b, p. 1056)—did not meet with the agreement of European experts, although he had available an interpretation for the charged particles observed in cosmic radiation by Skobeltzyn and Bothe and Kolhörster: They should be created by a primary γ-ray photon hitting an atomic nucleus. In particular, Skobeltzyn in Leningrad, who was informed by letters from Cambridge and Paris, also criticized Millikan's identification of positive-charge tracks with protons.[896] Anderson also recalled that he had quarrels with Millikan in those days on the nature of the positive tracks (C. Anderson and H. Anderson, 1983, pp. 139–140), and even more so on their energy (C. Anderson and H. Anderson, *loc. cit.*, p. 143). He finally succeeded, supported by his student Seth H. Neddermeyer, to persuade the stubborn Millikan of the existence of 'the energy of the cosmic rays ... in a few cases ... of the order of 10^9 electron-volts' (Anderson, 1932a, p. 420), while Millikan had earlier insisted that the energies could not exceed 400 to 500 MeV. Anderson, however, also noticed: 'The specific ionization along the tracks showing positives is in most instances not much greater than that of the electrons,' but added—in agreement with Millikan's assertion—also that 'the positives can only be protons, and cannot themselves represent nuclei of much higher number than unity' (Anderson, *loc. cit.*, p. 418). Two months later, he publicly expressed a different opinion in a contribution to *Science* by claiming the existence of 'a positively charged particle comparable in mass and magnitude of charge with an electron' (Anderson, 1932b, p. 239). In spite of this rather obvious conclusion, the physicists at Caltech remained cautious about it.[897]

In fall 1932, the centre of development on the 'positive electron' shifted to Cambridge, where Patrick Maynard Stuart Blackett had been working at the Cavendish Laboratory since the 1920's as an expert on cloud chamber observations. In July 1931, a new collaborator, Giuseppe Occhialini, had arrived at the Cavendish from Italy, where he had worked previously at the Arcetri Physics

[896] For further details, see De Maria and Russo, 1985, pp. 244–245.

[897] Thus, J. Robert Oppenheimer wrote in fall 1932 to his brother Frank:

> We have been running a nuclear seminar, in addition to the usual ones, trying to make some order of the great chaos, [but] not getting very far with that. We are supplementing the paper I wrote last summer [on electron impacts] with a study of radiation in electron impacts, and worrying about the neutron and Anderson's positively charged electron, and are cleaning up a few residual problems in atomic physics. (Robert Oppenheimer to his brother Frank, circa fall 1932, published in Oppenheimer, 1980, p. 159)

Laboratory of the University of Florence.[898] In spring 1932, he visited Paris and became acquainted with the photographs, taken by Irène Curie and Frédéric Joliot, for the scattering of the radiation from polonium-beryllium in a cloud chamber (I. Curie and F. Joliot, 1932b). Curie and Joliot had observed strange 'electrons emitted backwards with respect to the incident beam' and claimed that they originated from the scattering of neutrons with matter (I. Curie and F. Joliot, *loc. cit.*, p. 1230). During the summer of 1932, Blackett and Occhialini had built an apparatus consisting of two Geiger counters arranged in a coincidence circuit, one above and one below a cloud chamber (in order to have cosmic rays when passing through the chamber, stimulating its expansion and, thus, the creation of tracks); in addition, a magnet had been added to analyze the observed tracks (Blackett and Occhialini, 1932).[899] Blackett and Occhialini then started in fall 1932 to take photographs, and observed the same 'anomalies' as Curie and Joliot in Paris had found in spring with the terrestrial source (the polonium-beryllium source), also in cosmic radiation without drawing any conclusions. As Occhialini wrote later (in an Italian report of spring 1933): 'In the magnetic field some tracks are curved in the direction corresponding to negative particles, others to positive particles.... It had been evident since last summer, considering both penetration and ionization, that the tracks curving to the positive side could not be produced by protons.' (Occhialini in *La Ricerca Scientifica*, 1933, p. 373, English translation by De Maria and Russo, 1985, p. 267) But Blackett and Occhialini first tried to find explanations through some 'unclear mechanism,' even though Francis Aston brought the news about Anderson's conclusion to Cambridge, after a visit to Pasadena in September 1932. It took a while until Blackett and Occhialini had gone through a series of investigations and tests that they came out with a decisive publication. On 7 February 1933, the *Proceedings of the Royal Society* finally received the report on 'Some Photographs of the Tracks of Penetrating Radiation,' communicated by Rutherford (Blackett and Occhialini, 1933). After explaining certain technical details of their apparatus and method and adding some general remarks on 'the astonishing variety and complexity of those multiple tracks' observed in the photographs, Blackett and Occhialini proceeded to the physical

[898] Occhialini was already familiar with the Geiger-counter methods (through Bruno Rossi's stay at Berlin) and was supposed to learn about the British cloud chamber techniques.

G. P. S. Occhialini was born on 5 December 1907, in Rossombrone, the son of Augusto Occhialini, who had been Director of the Physics Institute at the University of Genoa. Giuseppe studied physics at the University of Florence and obtained his doctorate at the University of Florence in 1929. Then, he joined the group around Rossi under Antonio Garbasso (later, Mayor of Florence, Senator of Italy, and Chairman of the Italian National Research Council), and served as a research assistant. He spent the years 1931 to 1934 on an Italian fellowship at the Cavendish Laboratory. In 1937, he left Italy and worked on cosmic rays at the University of São Paulo, Brazil; in 1945, he accepted an appointment at the University of Bristol, where he discovered (with Cecil F. Powell, Cesar Lattes, and Hugh Muirhead) the π-meson. His later appointments were in Brussels (Free University, 1948–1950), Geneva (1950–1952), and Milan (after 1952). He died on 30 December 1993, in Paris.

[899] Similar apparatus had been used for cosmic-radiation studies earlier in the United States, e.g., by L. M. Mott-Smith and Gordon L. Locher (1931) at Rice University, and by J. C. Street and Thomas H. Johnson (1932) of the Franklin Institute.

interpretation in Section 3 (entitled 'The Nature of the Particles and Showers'). 'It is not always easy to [identify the particles producing the tracks] as evidence furnished by the photographs is often inclusive,' they began cautiously, and continued perceptively:

> But it will be shown that it is necessary to come to the same conclusion that has already been drawn by Anderson [1932b] from similar photographs. This is that some of the tracks must be due to particles with a positive charge but whose mass is much less than that of the proton. (Blackett and Occhialini, *loc. cit.*, p. 703)

They confirmed this conclusion by a very detailed examination of the ionization density of the fast particles and the curvatures of the tracks, expounding eventually the result: 'Altogether we have found 14 tracks occurring in showers which must almost certainly be attributed to such positive electrons, and several others which are less certain.' (Blackett and Occhialini, *loc. cit.*, p. 706) Thus far, Blackett and Occhialini had not gone beyond the results found by Anderson in previous August, but their analysis actually revealed more about the properties and nature of the positive electron of their American colleague.

A closer study of the frequency of showers, Blackett and Occhialini especially argued in Section 4 of their paper, made it 'seem plausible to assume that [they] arise from some nuclear disintegration process stimulated by particles or protons of high energy associated with the penetrating radiation' (Blackett and Occhialini, *loc. cit.*, p. 709). Indeed, the showers were found basically to emerge from the walls of the chamber; hence, they advanced (in Section 5) three possible 'mechanisms of the showers'; i.e.: 'They have existed previously in the struck nucleus, or they may have existed in the incident particle, or they may have been created during the process of collision.' (Blackett and Occhialini, *loc. cit.*, p. 712) They decided: 'Failing any independent evidence that they existed as separate particles previously, it is reasonable to adopt the last hypothesis.' (Blackett and Occhialini, *loc. cit.*, pp. 712–713) Their hypothesis was now strongly supported by the obvious absence of electrons in nuclei (as noted by Heisenberg and others); hence, Blackett and Occhialini arrived at the following conclusion concerning the shower mechanism:

> In this way one can imagine that negative and positive electrons may be born in pairs during the disintegration of light nuclei. If the mass of the positive electron is the same as that of the negative electron, such a twin birth requires an energy of $2\ mc^2 \sim 1$ million [electron] volts, that is much less than the translatory energy with which they appear in general in the showers. (Blackett and Occhialini, *loc. cit.*, p. 713)

Thus, they expounded first the 'pair-creation' mechanism derived from experiments.

The question now arose why the positive electrons exist in showers but otherwise 'have hitherto eluded observation.' The obvious reason, Blackett and Occhialini answered, was 'that they can have only a limited life as free particles since they do not appear to be associated with matter under normal conditions,' but: 'It is conceivable that they enter into combination with other elementary particles to form stable nuclei.' (Blackett and Occhialini, *loc. cit.*, p. 714) However, they quickly added: 'It seems more likely that they disappear by reacting with a negative electron to form two or more quanta.' At this point, Blackett and Occhialini finally referred to the theory of their Cambridge colleague Paul Dirac: 'The latter mechanism is given immediately by Dirac's theory of electrons.' (Blackett and Occhialini, *loc. cit.*) Apparently, the reference was given after some hesitation, but once it was out, Blackett and Occhialini made full use of the hole-theory formalism available and presented the annihilation calculation to demonstrate the quick appearance of the positive electron in matter. 'We are indebted to Professor Dirac not only for most valuable discussions of these points, but also for allowing us to quote the result of a calculation made by him of the actual probability of the annihilation process,' they admitted (Blackett and Occhialini, *loc. cit.*, p. 715).

The publication of Blackett and Occhialini indeed decided all previous theoretical and experimental discussions in favour of Dirac's ingenious anti-particle hypothesis. The meeting of the Royal Society of London on 16 February 1933, when the paper was presented, caused some public stir beyond the scientific community; the news even went beyond the Atlantic ocean. Watson Davis, Director of the American *Science Service*, informed Carl Anderson about it and suggested the name 'positron' to him, who accepted the proposal on 18 February 1933 (see De Maria and Russo, 1985, p. 271). Anderson now quickly finished his detailed paper on 'The Positive Electron,' which was received by the *Physical Review* on 28 February 1933, and published in the issue 15 March (1933c). Having been occupied in the previous months with details of the energy measurement of cosmic-ray particles (Anderson, 1933a) and the analysis of cosmic-ray bursts (Anderson, 1933b)—all items only indirectly connected with the positive electron—he returned for the first time to his discovery of August 1932. Still, he hesitated to accept the Cambridge interpretation on the basis of Dirac's hole theory and suggested alternative interpretations of the annihilation process, such as the annihilation of a 'proton-negatron pair'; further he suggested that 'the greater symmetry between the positive and negative charges revealed by the discovery of the positron should prove a stimulus to search for evidence of the existence of negative protons' (Anderson, 1933c, p. 494). Only slowly did he change over to accepting the British view of the positron as anti-electron (Anderson, 1933d, e; Anderson and Neddermeyer, 1933). Yet even this careful and substantiated change of opinion encountered the opposition of Millikan, who caused Anderson again to be doubtful. In an address, delivered on 27 December 1933, at the Boston meeting of the American Physical Society, Anderson stated the consistency of the laboratory γ-ray observations (such as the Meitner–Hupfeld effect to be discussed further below). With the Cambridge hypothesis of pair creation (Anderson, 1934), he joined his own voice with Millikan's later in December 1933 when he wrote:

> The simplest interpretation of the nature of the interaction of cosmic rays with the nuclei of atoms, lies in the assumption that when a cosmic-ray photon impinges upon a heavy nucleus, electrons of both sign are ejected from that nucleus and appear in the form of positrons and negatrons shown in our photographs. The large, and the, in general, uneven number of positrons and negatrons appearing in such photographs … seem difficult to reconcile with the Dirac theory, as interpreted by Blackett and Occhialini, of the creation of electron pairs out of the incident photons, and point strongly to the existence of nuclear reactions of a type in which the nucleus plays a more active role than merely that of the catalyst. (Anderson *et al.*, 1934, p. 363)

In Europe, the laboratory production of positive electrons was studied more closely (Chadwick, Blackett, and Occhialini, 1933; Meitner and Philipp, 1933; I. Curie and F. Joliot, 1933a, b). Blackett summarized the eventual outcome of all investigations in his review in *Nature* in December 1933 on 'The Positive Electron' as follows:

> These conclusions as to the existence and the properties of positive electrons have been derived from the data by the use of simple physical principles. That Dirac's theory of electrons predicts the existence of particles with just these properties, gives strong reason to believe in the essential correctness of his theory. (Blackett, 1933, p. 918)

Dirac, on the other hand, was convinced about the correctness of his theory; in the second half of 1933, he delivered several talks on 'The Theory of Positrons,' beginning in September at Leningrad (Dirac, 1934a), continuing at the seventh Solvay Conference on Physics in Brussels in October (Dirac, 1934b), and finally in his Nobel lecture in December 1933 in Stockholm (Dirac, 1934c).[900] Within a few years, his view was generally accepted, and the Chairman of the Nobel Committee for Physics, H. Pleijel, stated in the Presentation Speech for the Nobel Physics Prize to Carl Anderson in December 1936 that with the observation of the positron also 'the positron Dirac had been searching for was thus found' (in *Nobel Foundation* (ed.), 1965, p. 358). Twelve years later, when Patrick Blackett was honoured with the Nobel Prize for Physics for 1948, again the pair creation and 'the earlier mathematical electron theory elaborated by Dirac on the quantum basis' was emphasized (Presentation Speech by G. Ising, in *Nobel Foundation* (ed.), 1965, p. 65).

While the 'hole theory' thus celebrated an early experimental triumph, the other brilliant hypothesis which Paul Dirac proposed in 1931, that of the 'monopole,'

[900] In America, definite support came from J. Robert Oppenheimer and M. S. Plesset, who calculated explicitly the creation of pairs from gamma rays in the electrostatic field of nuclei in a simplified model obtaining good agreement with the experimental findings of Anderson and Neddermeyer (Oppenheimer and M. S. Plesset, 1933; Anderson and Neddermeyer, 1933).

was described as 'just a disappointment' (Kragh, 1990, Chapter 10).[901] Among Dirac's closest colleagues and friends, many considered monopoles as mere speculation of Dirac's mathematically oriented mind: In particular, Niels Bohr and Wolfgang Pauli disliked this concept. The witty group, which performed in April 1932 at Bohr's Institute in Copenhagen the play 'QUANTUM-THEORETICAL WALPURGIS NIGHT' (an adaptation of the scene in Goethe's famous play *Faust*), introduced an entry on the monopole with the words:

> Two Monopoles worshiped each other,
> And all of their sentiments clicked.
> Still neither could get to his brother,
> Dirac was so fearfully strict.
> (See the English translation in Gamow, 1966, p. 202)

In the 1930's, perhaps the strong support for the monopole came from Pascual Jordan in Rostock. In a paper of 1935, he rederived the monopoles from a quantum-electrodynamical formalism (Jordan, 1935b), while his Finnish student Bernd Olof Grönblom demonstrated the spherical symmetry of the object (Grönblom, 1935); three years later, Jordan returned to the topic and argued that, in spite of the prevalent sceptical attitude, one 'would now rather be inclined to regard the Dirac poles as a possibility worthy of serious investigation,' since in the meanwhile 'the number of known elementary particles has increased considerably' (Jordan, 1938a, p. 66). The senior Indian theoretical physicist Megh Nad Saha devoted a large part of his address on 'The Origin of Mass in Neutrons and Protons,' delivered on 8 February 1936, at the Indian Science Congress, to various aspects of the monopole; for instance, he derived the value of the monopole strength, Eq. [(686)], from a consideration of the quantized angular momentum (Saha, 1936, especially p. 145).[902]

Experimentally, the search for monopoles was started a few months after Dirac's paper in which he introduced the idea of the monopole in September 1931 by Owen Willians Richardson's letter to *Nature*. Richardson speculated about the possible existence of 'magnetic' atoms, similar to the usual electrical atoms, and calculated the spectra of such atoms (they could be extremely small, about 10^{-14} to 10^{-15} cm, and have very high spectral frequencies, about 3×10^{25}, compared to 10^{-8} cm and 10^{15} of the usual atoms); further, in cosmic radiation, even free monopoles might occur, and their presence 'obviously changes the basis for discussion of a good many cosmological questions,' he argued (Richardson, 1931, p. 582). The American physicists also joined the empirical search for monopoles,

[901] In a detailed historical account, Kragh has considered especially the effect of the concept of the monopole in theoretical and experimental physics of the 1930s (Kragh, 1981b). He discussed five theoretical publications, starting with Igor Tamm's immediate response (which we have already mentioned: Tamm, 1931), and some others from the 1940s. Actually, there were a couple of more such papers (see Kragh, 1990, Chapter 10), but this does not change the situation materially.

[902] See also the later notes of H. A. Wilson (1949) and Saha (1949).

e.g., Rudolph M. Langer of Millikan's laboratory at Caltech (who suggested a consideration about the energy levels of two bound objects similar to Richardson's: Langer, 1932). The following year, Merle A. Tuve of the Carnegie Institution in Washington, D.C. found that the 'recent discovery of a positively charged particle ... presumably related to the positive electron predicted by Dirac ... justifies calling the attention of other experimentalists briefly to the probability of detecting the existence of single isolated poles, as predicted by Dirac, by proper deflection with magnetic or electric fields, most conveniently the former' (Tuve, 1933, p. 770). Stimulated by Richardson's note of 1931, he calculated the path of a monopole with a mass considerably greater than that of the electron and high speeds of the order of 10^8 electron volts. 'One experiment adapted to the detection of such high-energy isolated magnetic poles has been a part of our projected programme for some time, waiting on the acquisition of a magnet of suitable dimensions,' he closed his letter of 17 April 1933, to the *Physical Review*, adding: 'Other tests requiring smaller magnetic fields and dealing with a lower energy region are being undertaken.' (Tuve, *loc. cit.*, p. 771) But Tuve never reported the discovery of magnetic monopoles, nor have others done so.[903]

(d) Quantum Mechanics of the Atomic Nucleus and Beta-Decay (1931–1934)

The couple of years following the Royal Society's 'Discussion on the Structure of Atomic Nuclei' of 7 February 1929, described in Section III.7, in which George Gamow's theory of α-decay and the alpha-particle structure of nuclei emerged as the strong points of progress while the problem of statistics of nitrogen just emerged, did not change the outlook in nuclear theory drastically. Only two major experimental difficulties, the continuous spectrum of β-decay electrons and the wrong statistics of certain nuclei—if regarded as composed of protons and electrons—became more pressing, even desperate, and physicists were prepared to accept more appropriate hypotheses to find a way out of the crisis, such as the breakdown of energy conservation and of certain properties of the elementary electron (such as spin and statistics?) or the hypothesis that a new neutral light particle (Pauli's 'neutron') existed. While the theoretical progress somehow stagnated in the same period, the experimental tools to investigate nuclear problems improved considerably, and the interest of the physicists in the whole topic grew as the previous frontiers of quantum physics moved forward to deal with them. This

[903] Work on magnetic monopoles, both experimental and theoretical (e.g., Fierz, 1944; Banderet, 1946; and Dirac, 1948) continued in the 1940s, and the search for them has never ceased since; for a detailed review covering this topic up to the early 1970s, see Amaldi and Cabibbo, 1972. It remained a field of wide speculations, even in the days of superstring theory and grand unified theories. (For the continuation of the search for monopoles into the 1980s, see Kragh, 1990, pp. 219–222; for the prehistory of the monopole concept before Dirac, see Hendry, 1983.) In June 1980, Dirac remarked: 'I don't believe anymore that monopoles exist; with the long and arduous search for them they have never been found.' (Conversations with Mehra in Chicago)

can be illustrated, for instance, by the fact that after 1931 several international conferences devoted to nuclear physics were held at various places—besides the more 'private' meetings at Niels Bohr's Institute in Copenhagen (where, in any case, most of the international elite of nuclear physics met)—especially the *Physikalische Vortragswoche* at the *ETH* in Zurich, held from 20 to 24 May 1931, the *Convegno di Fisica Nucleare*, organized from 14 to 18 October 1931, in Rome. These conferences served as a prelude to the even more historic (though also elite) meeting held in October 1933 at Brussels, the seventh Solvay Conference on Physics dealing with 'The Structure and Properties of Atomic Nuclei,' where the new nuclear theory was presented in a more or less well-established form.

A closer look at the Zurich and Rome conferences reveals the status of considerations before the great revolution, which took place in the following year—1932—and emerged from the discovery of neutron. Eugene Guth (from Vienna) and E. Bretscher wrote, on the basis of the notes of the lecturers, a summary of the reports given in Zurich dealing with the following items: First, the phenomena described by the α-particle model, which seemed to fit the observations on nuclear reactions grossly though not in all details (e.g., the extra γ-rays demanded by the theory to emerge in the α-decay of ThC were missing according to Lise Meitner's observations); second, the recently observed 'γ-radiation' from beryllium if bombarded by α-particles (Walther Bothe and Herbert Becker, 1930), which created considerable theoretical problems; third, the details of the hyperfine structure data in the cases of lithium and nitrogen, which (as emphasized by Pauli) also could not be explained by the standard theory (see Bretscher and Guth, 1931). The theoretical conclusions, summarized by Guth, emphasized the following points: (i) In principle, the questions of nuclear physics can be treated by the usual quantum-mechanical methods, with most nuclei being considered as built from α-particles and protons alone; just occasionally, *ad hoc*, i.e., phenomenological, attractive forces have to be introduced to fit observations (which perhaps will be explained in future by a correct relativistic quantum electrodynamics); (ii) more serious problems were caused by the assumption of electrons present within nuclei, which not only had to obey Dirac's relativistic equation with the mysterious negative-energy states, but also somehow violated conservation laws (in the continuous energy of the β-electrons or the statistics of nuclei); (iii) finally, a new effect observed in the scattering of hard γ-rays (by Lise Meitner and others) could not be accounted for by the interaction with nuclear α-particles and protons but might have to do with the problematic nuclear electrons. (Bretscher and Guth, *loc. cit.*, pp. 672–674)

Unlike the Zurich meeting convened by Wolfgang Pauli because of his great interest in the problems of nuclear structure—he had invited an illustrious group of mostly junior researchers from all over Europe, especially George Gamow from Copenhagen, Otto Stern, Immanuel Estermann and Robert Frisch from Hamburg, Lise Meitner, Hans Kopfermann, and Hermann Schüler from Berlin, Walther Bothe from Gießen, Hendrik Kramers from Utrecht, Maurice de Broglie, Louis Leprince-Ringuet, and Frédéric Joliot from Paris, Patrick Blackett from Cambridge, Eugene Guth and Theodor Sexl from Vienna—the Rome meeting served a different purpose, namely, the preparation of establishing an institute devoted to nuclear physics in the Italian capital. In Rome, the Sicilian Orso Maria Corbino, an Italian Senator and Minister of Education, represented physics; he had brought Enrico Fermi (in 1927) and Franco Rasetti (in 1929) to the University of Rome. Emilio Segrè, who received his doctorate with Fermi in 1928, recalled:

> We knew [shortly before 1930] that atomic spectroscopy was in a state of being completed. Quantum mechanics had been fully developed, and therefore something new had to come, and this something new was rather evident. It was the atomic nucleus.... In 1929 Corbino delivered an extraordinarily prophetic speech in his characteristic Italian. He discussed this address with Fermi.... In spite of being quite young, we had invested already considerably, in particular into the experimental equipment for atomic physics; hence now it was not easy for us to pass over to nuclear physics. Nevertheless Fermi convinced everybody that the transition had to be made, and we started to turn over—we, these are, Fermi, Rasetti and myself. Naturally, the first step occurred in the direction of spectroscopy, since we had gathered some experience in spectroscopy.... This led to the publications of Fermi on hyperfine structure, and of Rasetti on the Raman effect. (Segrè, 1981, p. 4)

As Segrè remarked, the second step consisted in organizing 'a small conference called together in Rome,' of which '[Guglielmo] Marconi, then president of the Italian Academy acted as host' and 'Corbino wrote an inauguration speech' (Segrè, *loc. cit.*, p. 5). Although Ernest Rutherford, the most distinguished senior expert on nuclear physics, could not attend the Rome meeting, a most respectable number of participants assembled, especially Niels Bohr and George Gamow from Denmark, Francis Aston, Patrick Blackett, Charles D. Ellis, Ralph Fowler, Nevill Mott, and Owen Richardson from England, Guido Beck, Walther Bothe, Peter Debye, Hans Geiger, Werner Heisenberg, Lise Meitner, Arnold Sommerfeld, and Otto Stern from Germany, Léon Brillouin, Marie Curie, and Jean Perrin from France, Paul Ehrenfest and Samuel Goudsmit from the Netherlands, and Wolfgang Pauli from Switzerland; they joined the Italian participants from Rome, Antonio Garbasso and Bruno Rossi from Florence, and Enrico Persico, G. C. Trabacchi, and G. Wataghin from Turin. The talks presented at the conference included Bohr's on 'Atomic Stability and Conservation Laws,' Gamow's on 'Nuclear Structure,' Ellis' on 'β-Rays and γ-Rays,' as well as Bothe's on 'Artificial Nuclear Transition and Excitation, Isotopes.' The participants from the Rome institute wanted mainly to learn, but already in July of the following year, Enrico Fermi was invited to present a report on '*Lo stato attuale della fisica del nucleo atomico* (The Present Status of the Physics of the Atomic Nucleus)' at the *Cinquième Congrès International d'Electricité* (Fifth International Congress on Electricity) in Paris (Fermi, 1932c). In this report, he first summarized the results obtained before the year 1932, and finally, he discussed the recent developments since James Chadwick's discovery of the neutron (Fermi, *loc. cit.*, pp. 112–113). In between, he also mentioned Pauli's proposal of a 'neutron (*neutrone*)' which takes away a part of the energy in β-decay (Fermi, *loc. cit.*, p. 109), and pointed out in the discussion that it had a much smaller mass than did Chadwick's neutron. What he did not yet cover was Heisenberg's new theory of atomic constitution based on the proton–neutron structure, because the first paper (Heisenberg, 1932b) had not yet appeared in print.

On the other hand, Fermi addressed in Paris in quite some detail what he called an 'important peculiarity' observed in the absorption of γ-rays 'in recent years by Chao, Meitner and Hupfeld ... who have found that the absorption coefficient for various substances, if referred to a fixed number of electrons, is not constant but increases with the atomic number in the absorbing substance,' notably:

> For the light atoms, the absorption coincides with that calculated on the basis of the Klein-Nishina formula, while for the heavier atoms it will be higher. Perhaps this phenomenon can be attributed to a diffusion of atomic electrons, which grows in intensity with increasing atomic number of the absorbing nucleus. (Fermi, 1932c, p. 111)

Earlier, in Section II.7, we have reported on the relativistic treatment of the scattering of γ-rays by electrons (of atomic absorbers) suggested by Oskar Klein and Yoshio Nishina, as well as the experimental test carried out by Louis Harold Gray of Cambridge; the latter had in particular arrived at a perfect agreement of his data with the theory of Klein and Nishina (Gray, 1929). Soon afterward, however, the situation changed, as new investigations were performed in Europe and the USA. Thus, for example, Carl Anderson of Millikan's Caltech laboratory recalled:

> At that time [i.e., in 1929] ..., Dr. Chung-yao Chao, working in a room close to mine, was using an electroscope to measure the absorption and scattering of γ-rays from ThC$''$. His findings interested me greatly.... Dr. Chao's results showed clearly that both the absorption and scattering were substantially greater than calculated by the Klein-Nishina formula. (C. Anderson and H. Anderson, 1983, pp. 135–136)

Anderson then proposed (without success, because his professor, Millikan, had other plans) to study the situation in a cloud chamber experiment and voiced 'firm conviction that had this experiment been carried out, the positive electron would have been discovered, for about 10 percent of the electrons emerging from the lead plate would have had a positive charge'—the reason being that the excess absorption discovered by Chao was caused by electron–positron pair production, and the excess scattering by γ-rays produced from electron-positron annihilation (C. Anderson and H. Anderson, *loc. cit.*, p. 136).

The detailed story of this 'excess' effect involved indeed 'four papers submitted from three different laboratories in May 1930':

> Each group made use of the ThC$''$ γ-ray source (consisting of a nearly pure line at the high energy of 2.61 MeV) and each reported results confirming the Klein-Nishina

> (*KN*) formula for absorbers of low atomic number. Three of the papers reported that additional new scattering and/or absorption phenomena, apparently associated with the nucleus, resulted in increased absorption in heavy elements beyond that predicted by *KN*. The Berlin group of Lise Meitner and H. H. Hupfeld was actually the first to publish, and the effect was associated with those names. (Brown and Moyer, 1984, p. 132)

Indeed, Meitner and her student Hupfeld at the *Kaiser Wilhelm-Institut für Chemie* submitted their note '*Über die Prüfung der Streuungsformel von Klein und Nishina an kurzwelliger γ-Strahlung* (On the Verification of the Klein and Nishina Formula for Short-Wavelength γ-radiation)' already on 9 May 1930, to *Naturwissenschaften*, where it appeared in the issue of 30 May (Meitner and Hupfeld, 1930), while Chao's paper was communicated to the *Proceedings of the National Academy of Sciences (USA)* on 15 May 1930 (Chao, 1930a); on the other hand, the papers of G. T. P. Tarrant and Louis Gray of the Cavendish Laboratory were both received on 5 May 1930, by the *Proceedings of the Royal Society of London* and appeared in the issues of 1 July and 15 August, respectively, but they did not—unlike the other two—show a clear increase for materials of higher atomic numbers (Tarrant, 1930; Gray, 1930). Meitner and Hupfeld, after referring to the previous confirmation of the Klein–Nishina formula (by Skobeltzyn and Stoner), which they criticized as having been carried out with the complex γ-line spectra of $\mathrm{RaB} + \mathrm{C}$, compared their new data with the available formulae for the Compton effect, by Compton, Dirac–Gordon, and Klein–Nishina, respectively, and concluded:

> The values obtained agree best by far with the formula of Klein and Nishina. However, there exist clear deviations, which for increasing atomic weight grow increasingly large and certainly lie beyond the experimental error. (Meitner and Hupfeld, 1930, p. 535)

They agreed then that the Klein–Nishina formula had to be correct theoretically; however, there existed an extra scattering effect beyond the photoeffect and additional classical scattering, which might be attributed perhaps to a scattering of very shortwave radiation by the atomic nuclei. Chao, who observed the same effect, examined in a second paper the angular dependence of the scattered radiation in the case of lead, and concluded: 'The wavelength and space distribution of these are inconsistent with an extra nuclear scatterer and hence must have their origin in the nuclei.' (Chao, 1930b, p. 1519)[904] In the course of further experimental investigations, the different teams in Germany, England and the USA added more, at times puzzling details.[905] In particular, Chao (already in 1930b) and Gray and Tarrant found, besides the forward peaked scattering, an isotro-

[904] It might be pointed out that the different discoverers of the anomalous scattering used different methods to register the radiation: Meitner and Hupfeld used counters, Tarrant and Gray used ionization chambers, and Chao used the electroscope.

[905] For a more detailed report on the Meitner–Hupfeld effect story, see Brown and Moyer, 1984.

pic component at roughly 0.5- and 1.0-MeV energy (Gray and Tarrant, 1932). Meitner and Hupfeld, who had presented in early December 1930 a detailed report on the effect, using both the ThC″-line and filtered RaC γ-radiation (1931), returned in March 1932 again to the topic: On the one hand, they disagreed with the existence of the shifted radiation observed by the English and American competitors; on the other hand, they proposed the anomalous effect to originate from the scattering of hard γ-rays by nuclear electrons (Meitner and Hupfeld, 1932). The latter claim, of course, transferred the problem to a deeper-lying one, which would be decided only later.[906]

In February 1932, with the announcement of the discovery of the neutron, a new epoch began in nuclear physics. Soon afterward, on 28 April 1932, Lord Rutherford opened another 'Discussion on the Structure of Atomic Nuclei' at the Royal Society, in which he focused on the progress achieved since the last discussion in 1929 (Rutherford *et al.*, 1932). Referring to the previous standard model of the nucleus, he remarked:

> It is generally supposed that the nucleus of a heavy element consists mainly of α-particles with an admixture of a few free protons and electrons, but the exact division between these constituents is unknown. On the theory, there is a great difficulty in including within the minute nucleus particles of such widely different masses as α-particles and electrons.... It appears as if the electron within the nucleus behaves quite differently from the electron in the outer atom. This difficulty may be of our own creation for it seems to me more likely that an electron cannot exist in the free state in a stable nucleus, but must always be associated with a proton or other massive units. The indication of the existence of the neutron in certain nuclei is significant in this connection. (Rutherford *et al.*, *loc. cit.*, pp. 736–737)

While Chadwick reported on some details of his recent discovery of the neutron, he did not add anything about its particular role in the nuclear constitution. However, he enlarged on this point in an extended paper received by the *Proceedings of the Royal Society* on 10 May 1932, by stating:

[906] We have come across Lise Meitner's work on the problems of nuclear physics already several times. She was born on 17 November 1878 in Vienna and studied physics and mathematics (with Ludwig Boltzmann and Franz Exner) at the University of Vienna, and obtained her doctorate with an experimental thesis on heat conduction. In 1907, she went to Berlin to continue her studies in theoretical physics with Max Planck; simultaneously, she worked with Otto Hahn in the chemical institute of the University of Berlin on problems of radioactivity. In 1912, she joined Hahn in the just founded *Kaiser Wilhelm-Institut (KWI) für Chemie*; she also served then as assistant to Planck. After World War I, during which she worked as an X-ray nurse in Austria, she returned to Berlin and established her own physical division in radioactivity at the *KWI* (with Otto Hahn leading the corresponding chemical division). From 1922, she taught at the University of Berlin (promoted to professorship in 1926), but she lost this position in 1933 as a consequence of the Nazi racial laws. After the *Anschluß* (the annexation of Austria into the *Third Reich*), Meitner's life was endangered and she escaped via Holland to Sweden, where she got a modest position at the Nobel Institute in Stockholm—which improved only after World War II (1946: guest professor at the Catholic University in Washington, D.C.; 1947: laboratory leader of the Swedish Atomic Energy Commission; 1953–1960, head of the laboratory at the Engineering Academy). She retired to Cambridge to live with her nephew, the physicist Otto Frisch, and died there on 27 October 1968.

> We must suppose that the neutron is a common constituent of atomic nuclei. We may then proceed to build up nuclei out of α-particles, neutrons and protons, and we are able to avoid the presence of uncombined electrons in a nucleus.... If the α-particle, the neutron and the proton are the only units of nuclear structure, we can proceed to calculate the mass defect on the binding energy of a nucleus. (Chadwick, 1932b, pp. 705–706)

Still, he added that one cannot be sure that besides the particles mentioned no other complex particles, such as the heavy hydrogen isotope of Harold Urey and his collaborators, may play a role.

On 28 April 1932, before Chadwick's paper was submitted, Dmitrij Iwanenko submitted a short note to *Nature* on 'The Neutron Hypothesis,' which appeared in the issue of 28 May. He picked up on Chadwick's earlier note (*Nature*, 1932a), talked about the discovery of the neutron, and continued:

> Is it not possible to admit that neutrons also play an important role in the building of nuclei, the nuclei electrons *all* packed in α-particles or neutrons? The lack of a theory of nuclei makes, of course, this assumption rather uncertain, but perhaps it sounds not so improbable if we remember that the nuclei electrons profoundly change their properties when entering into the nuclei, and lose, so to say, their individuality, for example their spin and magnetic moment.
>
> The chief point of interest is how far the neutrons can be considered as elementary particles (something like protons and electrons). It is easy to calculate the number of α-particles, protons and neutrons for a given nucleus, and form in this way an idea about the [angular] momentum of the nucleus (assuming for the neutron a momentum $\frac{1}{2}[h/2\pi]$). It is curious that beryllium nuclei do not possess free protons but only α-particles and neutrons. (Iwanenko, 1932a, p. 798)

In early August of the same year, Maurice de Broglie communicated to the *Académie des Sciences (Paris)* another note of Iwanenko, '*Sur la constitution des noyaux atomique* (On the Constitution of Atomic Nuclei,' in which he proceeded to work out the proton–neutron structure of nuclei following his method of banning all electrons from the nuclei (Iwanenko, 1932b, pp. 439–440). Thus, he constructed the chlorine isotopes Cl_{35} and Cl_{37} out of eight α-particles, one proton, and two or four neutrons, respectively; or Bi_{209} isotope of 41 α-particles, one proton, and 44 neutrons. If he especially endowed the neutron with a spin of $\frac{1}{2}\frac{h}{2\pi}$ he found that the N_{14} nucleus obtained integral spin and obeyed Bose–Einstein statistics, which was just the right property that was observed empirically.[907] We

[907] Together with E. Gapon, Iwanenko sent a further note on the topic to *Naturwissenschaften*, entitled '*Zur Bestimmung der Isotopenzahl* (On the Determination of the Isotopic Number),' in which the authors assumed the nuclear particles proton and neutron to be bound by a central field; they calculated qualitatively the quantum states with this potential for the sequence of nuclei N_{15}, O_{16}, O_{17}, O_{18}, F_{19}, Ne_{20}, Ne_{21} using five quantum numbers (Gapon and Iwanenko, 1932). Iwanenko was born on 29 July 1904 in Poltava. Graduating in 1927 from the University of Leningrad in 1927, he became after 1930 professor at the institutes of Kharkov, Tomsk, Sverdlovsk and Kiev, in 1942 finally at the University of Moscow.

should not assume that Iwanenko's radical proposal to abolish all nuclear electrons remained the only model of nuclear constitution after the discovery of the neutron. Other models sprang up, e.g., the one of Georges Fournier: In a note communicated by Jean Perrin in the session of 25 April 1932, of the *Académie des Sciences (Paris)*, Fournier proposed to compose nuclei of an assembly of α-particles, electrons, and 'demi-helions'—a compound of two protons and one electron (Fournier, 1932). However, the success of the proton–neutron model, which Iwanenko first published, won out, especially after Heisenberg made the same proposal and even provided on its basis a quantum-mechanical theory of nuclear structure in a series of three pioneering papers that were received by *Zeitschrift für Physik* on 7 June, 30 July, and 22 September 1932 (Heisenberg, 1932b, c; 1933).

Also, a few years previously, Heisenberg, in order to avoid the 'misfortunes with spins,' had suggested that 'there no longer really are electrons in the nucleus' (Heisenberg to Bohr, 20 December 1929). In a further, later letter, he had then sketched a 'lattice model' of the microscopic world consisting of cells of volume $(h/Mc)^3$—with M as the proton mass—in which the nucleus would consist just of quanta of mass M (not necessarily charged) and photons (see Heisenberg to Bohr, 10 March 1930); but he soon abandoned this idea because it did not allow for relativistic invariance.[908] During the following one and a half years, Heisenberg had then been occupied with different problems (mainly connected with relativistic quantum field theory); however, in October 1931, he had attended the Rome Congress on Nuclear Physics, after which he had entered again upon some exchange with Niels Bohr, informing him about new considerations on cosmic-radiation phenomena (especially connected with the behaviour of relativistic electrons: Heisenberg, 1932a). In January and again in early March 1932, he met with Bohr (on a skiing vacation in the Bavarian Alps, together with Felix Bloch and Carl Friedrich von Weizsäcker), who then found upon his return to Copenhagen a letter from James Chadwick, dated 24 February 1932, containing a copy of his letter to *Nature* about the neutron (Chadwick, 1932a). Thus, Heisenberg was informed of the discovery of the neutron even before its publication (through letters from Bohr, dated 21 and 22 March 1932), and he had the opportunity of discussing the implications of the discovery of the neutron for nuclear physics and other fields at the following meeting in Copenhagen, 3–13 April 1932. Bohr, in particular, thought along the following lines:

> A neutron may be regarded from a formal descriptive point of view as a nucleus of an element with atomic number zero. Just as little as it is possible at the present stage of atomic mechanics to account in detail for the stability of ordinary nuclei, it is impossible at present to offer a detailed explanation of the constitution of the neutron. Of course its mass and charge suggest that a neutron is formed by a combination of a

[908] For details of Heisenberg's early concern with nuclear problems, and the relation of his ideas to Bohr's programme of renouncing conservation laws, see Bromberg, 1971, pp. 323–329. Heisenberg evidently fluctuated between the opposite positions taken by Bohr and Pauli, respectively.

> proton and an electron, but we cannot explain why those particles combine in such a way as little as we can explain why 4 protons and 2 electrons should combine to form a helium or α-particle. (See Bohr's manuscript 'On the Properties of the Neutron,' dated 25 April 1932, published in Bohr, 1986, pp. 117–118, especially, p. 117.)

This was the basic input when Heisenberg began to approach nuclear theory with the help of the neutron.

While, initially, Heisenberg had just thought about the use of the neutron to explain certain cosmic-ray problems (Heisenberg to Bohr, 24 March 1932), after his visit to Copenhagen in April, he changed the topic of interest, and a couple of months later he sent to Bohr 'the proofs of a paper on the nuclei which I completed in the past weeks,' and wrote: 'The basic idea is to shift all difficulties of principle to the neutron and to deal with the nucleus by [ordinary] quantum mechanics.' (Heisenberg to Bohr, 20 June 1932) Bohr replied a week later that 'I hasten to write how very much we all appreciated your wonderfully beautiful paper.' (Bohr to Heisenberg, 27 June 1932; see Bohr, 1987, p. 703) Since the available correspondence reveals little about the genesis of the paper in question, we shall quote von Weizsäcker's recollections:[909]

> I had the chance to spend with him [Heisenberg] in May 1932 his pentecost vacations—during which time of the year he was attacked by hay fever—in Botterode in the *Thüringer Wald*. In a phase of most intense labour that characterized his style of work so often, and at the same time always walking and hiking in free nature, he discussed and wrote his paper "*Über den Bau der Atomkerne. I*," which was received by the *Zeitschrift für Physik* on 7 June 1932. (Carl Friedrich von Weizsäcker, 1989, p. 186)

Heisenberg's paper referred to here was the first of a sequence of three papers, all of which were organized in the same manner (Heisenberg, 1932b, c; 1933). There were one or more sections on the quantum-theoretical Hamiltonian of the nucleus and its evaluation to discuss the observed structure and stability of nuclei. Then, sections on the scattering of γ-rays from nuclei followed (addressing especially the Meitner–Hupfeld effect) and on the structure of the neutron (the fundamental problem). For the actual progress of nuclear physics, the first part provided the most important results. Here, Heisenberg considered the 'neutron as an independent fundamental [or elementary] constituent'—with some hesitation, for he added that 'it may be assumed that it can be split under suitable circumstances into a proton and an electron, probably by renouncing the conservation laws of energy and momentum' (Heisenberg, 1932b, pp. 1–2). Then came the central practical message of the work, when Heisenberg introduced the interaction be-

[909] For an introduction to the contents of this and two further papers of the series, see—besides von Weizsäcker, 1989—also Brown and Rechenberg, 1989. The whole topic of nuclear structure and β-decay, which is treated below, has been discussed in some detail by Brown and Rechenberg, 1988, and Brown and Rechenberg, 1996, Chapter 2.

tween the 'elementary' constituents, especially the famous exchange force between the neutrons and the protons as:

> If one puts the neutron and the proton at a distance comparable to nuclear dimensions, then—in analogy to the H_2^+-ion—an exchange of places (*Platzwechsel*) of the negative charge will occur, whose frequency is given by a function $\frac{1}{h}J(r)$ of the distance of the particles. The quantity $J(r)$ corresponds to the exchange integral, or better the *Platzwechsel* integral, of nuclear theory. Thus one can again visualize *Platzwechsel* in the pictures of electrons [as] having no spin and obeying the rules of Bose statistics. However, it may be more correct to consider the *Platzwechsel* integral $J(r)$ as a fundamental property of the neutron-proton pair without reducing it to the motion of electrons. (Heisenberg, *loc. cit.*, p. 2)

Two facts must be registered here. First, Heisenberg was still motivated a bit by the former electron–proton model of the nucleus, which he had not yet abandoned completely. Second, here again a general exchange integral was offered for the nuclear force, without referring to any 'migration of electrons,' which has been considered by some historians to be the more progressive idea (see, e.g., Miller, 1984, p. 255).

The decisive formal step consisted in providing the Hamiltonian function for the nucleus, namely,

$$
\begin{aligned}
H = {} & \frac{1}{2M}\sum_k \mathbf{p}_k^2 + \frac{1}{2}\sum_{k>l} J(r_{kl})(\rho_k^\xi \rho_l^\xi + \rho_k^\eta \rho_l^\eta) \\
& + \frac{1}{4}\sum_{k>l} K(r_{kl})(1+\rho_k^\zeta)(1+\rho_l^\zeta) \\
& + \frac{1}{4}\sum_{k>l} \frac{e^2}{r_{kl}}(1-\rho_k^\zeta)(1-\rho_l^\zeta) - \frac{1}{2}D\sum_k (1+\rho_k^\zeta),
\end{aligned}
\tag{691}
$$

where M is the mass of the protons, r_{kl} are the distances, and $\mathbf{p}_k$ are the momenta of protons and neutrons. Evidently, the first force-term was associated with the proton–neutron exchange force, the second force-term with the neutron-neutron force—here, Heisenberg had in mind the analogy to the homopolar binding force between two neutral atoms in the hydrogen molecule—and the third with the Coulomb repulsion force between protons (which was not an exchange force). The last term took into account the mass defect between the protons and neutrons.[910] In writing the expression (691), Heisenberg introduced—besides the space ($\mathbf{r}$) and spin (σ) variables—a new set of 'numbers ρ^ζ to describe a particle in the nucleus, which can assume the two values $+1$ and -1,' namely:

[910] The large neutron–proton interaction was emphasized theoretically by Niels Bohr and substantiated experimentally by Meitner and Philipp (1932).

> $\rho^\zeta = +1$ should indicate that the particle is a neutron, while $\rho^\zeta = -1$ denotes a proton. Since in the Hamiltonian function, because of the *Platzwechsel* processes, also transitions occur between $\rho^\zeta = +1$ and $\rho^\zeta = -1$, it will be practical to consider also the use of the matrices
>
> $$\rho^\xi = \begin{vmatrix} 0 & 1 \\ 1 & 0 \end{vmatrix}, \quad \rho^\eta = \begin{vmatrix} 0 & -i \\ i & 0 \end{vmatrix}, \quad \rho^\zeta = \begin{vmatrix} 1 & 0 \\ 0 & -1 \end{vmatrix}. \qquad [(692)]$$
>
> The space of the ξ, η, ζ, of course, does not have to do [anything] with the real space. (Heisenberg, 1932b, pp. 2–3)

The new space was later called 'isotopic space' or 'isospace' and connected with the charge transition of nuclear particles and eventually the charge-independence of nuclear forces (see Section IV.5 below).

With these matrices, Heisenberg could easily describe the number of neutrons ($n_1 = \sum(1 + \rho_k^\zeta)$) and protons ($n_2 = \frac{1}{2}(1 - \rho_k^\zeta)$). The Hamiltonian (691), which was independent of the change of sign of $\sum \rho_k^\zeta$, assumed a minimum for $\sum \rho_k^\zeta = 0$; hence, Heisenberg concluded:

> The minimum energy created by *Platzwechsel* integrals is obtained if the nucleus consists of as many neutrons as protons. This result fits well with the experimental data in general.... By the last three terms of [(691)] the ratio of neutron to proton numbers corresponding to the energy minimum is shifted in favour of the former, i.e., for growing total number n because of the Coulomb forces of the protons. (Heisenberg, *loc. cit.*, p. 4)

Based upon this qualitative success, Heisenberg considered the simplest complex nucleus, the lowest energy state of the heavy-hydrogen isotope of Urey *et al.*, composed of a proton and a neutron, and then more qualitatively that of the helium nucleus. He further found that the nuclei repel each other at large distances (because of their charges), while at short distances they are bound by a kind of Van der Waals force and the neutron–neutron forces. In light of these considerations, he discussed in §3 and §4 of the paper the most stable cases of nuclei with respect to the dependence of the neutron to proton number ratio, while in §5 he analyzed the stability data for beta-decay.

Heisenberg extended these stability considerations also to α-decay in parts II and III, received on 30 July and 22 December, respectively (Heisenberg, 1932c; 1933).[911] In these papers, he treated in particular the topics mentioned above, notably, 'other physical phenomena for which the neutron can no longer be con-

[911] In part III, the molecular analogy was extended; thus, the neutron–proton exchange forces became supplemented by an 'electrostatic' (nonexchange) force, similar to what had been noticed in the theory of the H_2^+-ion. In addition, Heisenberg employed there the well-known Thomas–Fermi method for actual calculations, applying in the minimalization-of-energy procedure an approximate Hamiltonian, with the restriction that the magnitude of the total ρ-spin was fixed. This procedure forecast the later charge-independence of nuclear forces (see Section IV.5).

sidered a static structure ..., e.g., the Meitner-Hupfeld effect ... [and] all experiments in which the neutrons can be split into protons and electrons' (Heisenberg, 1932b, p. 1)—in short, all effects where the peculiar property of the proton—or its assumed compositeness played a role. Heisenberg was fully convinced that in that case the laws of quantum mechanics would break down since 'the very existence of the neutron contradicts the laws of quantum mechanics in its present form' (Heisenberg, 1932c, p. 163), or:

> The discovery of the stability of the neutron, not describable by the present theory, allows a clean separation of the cases in which quantum mechanics is applicable from those [cases] in which it is not; for this stability allows purely quantum-mechanical systems to be built up out of protons and neutrons, in which the new kind of features that enter into β-decay do not create any difficulty. This possibility of a sharp separation of the quantum-mechanical aspects and those new features characteristic of the nucleus seems to get lost if electrons are considered as independent nuclear constituents. (Heisenberg, 1933, p. 595)

For instance, Heisenberg explained (incorrectly) the Meitner–Hupfeld effect, which belonged to the second category, by two kinds of processes: the normal (quantum-mechanical) Rayleigh and Raman effects, due to the scattering of γ-rays by the nuclear constituents proton and neutron, and a specific scattering by the electrons in the nucleus that does not obey quantum-mechanical rules.

While Heisenberg was on a summer trip to the USA in 1932, he received a letter from Bohr, who gave him some news: 'In Brussels it was decided that the next Solvay meeting would be about nuclear problems. Cockcroft, Joliot and Chadwick will be asked to prepare reports on the latest experimental advances. Furthermore, Gamow will be asked to give an account of the relationship between α- and γ-spectra and you and I were suggested as the organizers of a discussion about the more fundamental theoretical questions.' (Bohr to Heisenberg, 7 July 1932) On his way back home from America to Leipzig, Heisenberg stopped over for a few days in Copenhagen and discussed with Bohr the task envisaged and the progress of his own work on nuclear theory, which he published in part III (Heisenberg, 1933).[912] In the Winter Semester, a new visitor arrived in Leipzig, whose work Heisenberg soon announced to Copenhagen as: 'Majorana (Jr.) has written quite a nice paper about which I shall report to you soon.' (Heisenberg to Bohr, 23 February 1935) From January 1933, Ettore Majorana, a member of Fermi's institute in Rome, stayed with Heisenberg until the beginning of summer. He was extremely talented; he had worked on spectroscopic questions and the relativistic electron before turning to problems of nuclear theory later in 1931, when he entered upon a critical study of the results published by Meitner and the Joliot–Curies before the discovery of the

[912] See Heisenberg to Bohr, 17 October 1932.

neutron.[913] Other than Fermi in Rome, Heisenberg—for whom Majorana showed 'a great admiration and feeling of friendship'—'persuaded him without difficulty by the sheer weight of his authority to publish his paper on nuclear theory' (Amaldi, 1966, p. 36). On 3 March 1933, the *Zeitschrift für Physik* received the investigation entitled '*Über die Kerntheorie* (On Nuclear Theory),' proposing—as Majorana pointed out in the abstract—'a new foundation of Heisenberg's nuclear theory, leading to a somewhat deviating Hamiltonian' (Majorana, 1933a, p. 137).[914]

The main difference between the approach of Heisenberg and the new one of Majorana lay in the fact that the latter dropped the analogy of nuclear to molecular forces and simply assumed the existence of nuclear matter, formed by neutrons and protons and the forces among them. Majorana described these forces by the expression

$$(Q', q'|J|Q'', q'') = -\delta(q' - Q'')\delta(q'' - Q')J(r), \tag{693}$$

with Q and q denoting the coordinates of neutrons and protons, respectively, and $r = |q' - Q'|$ their mutual distance. Hence, in an α-particle, two neutrons acted on each proton, such that the two neutrons and the two protons formed a closed shell, where all particles occupied the same (lowest) state. In contrast to Majorana, Heisenberg's Q and q denoted all coordinates plus spin variables, and his interaction energy [unlike Eq. (693)] exhibited a positive sign; hence, he failed to obtain

[913] Edoardo Amaldi, who had witnessed the development of Majorana in Rome, mentioned two examples of the latter's insight in a biographical sketch: First, Majorana, having seen the papers of Joliot and Curie from Paris, realized that the results had to be interpreted as 'the recoil of protons produced by a heavy neutral particle' (thus, anticipating the conclusions of Chadwick in February 1932); second, independently of Iwanenko and Heisenberg, Majorana also hit upon the idea of the proton–neutron composition of the atomic nucleus. (Amaldi, 1966, especially, pp. 30–31; see also Segrè, 1979, pp. 47–49)

Ettore Majorana was born on 5 August 1906, in Catania, a nephew of the physicist Quirino Majorana, and received his school education as a boarder at the *Istituto Massimo* in Rome, graduating in 1923 with his *maturita classica*. Then, he studied engineering at the University of Rome (as a fellow student of Emilio Segrè), switching to study physics under Fermi in early 1928. He received his doctoral degree with the thesis '*Sulla meccanica dei nuclei radioattivi* (On the Mechanics of Radioactive Nuclei).' Though generally rather reserved, he maintained a close friendship with Giovanni Gentile, a fellow Sicilian and lecturer at the physics institute in Rome, with whom he collaborated on his first publication dealing with the X-ray spectra of cesium. In November 1932, Majorana became a lecturer at the University of Rome, in spite of the fact that by that time he had only five publications (though highly appreciated). With a fellowship of the Italian National Research Council, he went to Leipzig in the beginning of 1933, then to Copenhagen, and again to Leipzig. After his return to Rome, he fell sick and withdrew increasingly from Fermi's institute. In early 1937, there was a competition for the chair of theoretical physics at the University of Palermo (where Segrè held the experimental professorship), and Majorana did not get it, but did obtain another such chair at the University of Naples in November 1937. On 25 March 1938, he sent a telegram from Palermo to a colleague in Naples; he boarded a steamer there in the evening of the same day but never arrived in Naples. (For Majorana's biography, see Amaldi, 1966)

[914] An Italian version of the paper appeared in *La Ricerca Scientifica* (Majorana, 1933b).

any saturation effect (as Majorana did) and also had to add additional repulsive forces acting at small distances (see Heisenberg, 1933, pp. 590–591). Majorana's energy expression looked much less complicated, as he simply wrote:

$$W = T + E + A, \tag{694}$$

with T denoting the kinetic energy of the nuclear particles (of momentum p), E denoting the electrostatic energy of the protons, and A denoting the neutron–proton exchange energy, or in detail

$$T = \frac{1}{2M}\mathrm{Trace}[(\rho_{\mathrm{N}} + \rho_{\mathrm{P}})p^2], \tag{694a}$$

$$E = \frac{e^2}{2}\int (q'|\rho_{\mathrm{P}}|q')\frac{1}{|q' - q''|}(q''|\rho_{\mathrm{P}}|q'')\, dq'\, dq'', \tag{694b}$$

and

$$A = -\int (q'|\rho_{\mathrm{N}}|q'')J(|q' - q''|)(q''|\rho_{\mathrm{P}}|q')\, dq'\, dq'', \tag{694c}$$

for the new exchange-force *Ansatz*. The exchange integral $J(r)$ might assume one of the two alternate forms

$$J(r) = \lambda\frac{e^2}{r} \tag{695a}$$

or

$$J(r) = A\exp(-\beta r), \tag{695b}$$

of which Majorana preferred the second version, because it was regular at $r = 0$ and provided two parameters to fit the mass defects of both the nuclei of heavy hydrogen and of helium.

In those days, Eugene Wigner was also concerned with the mass defect of the heavy-hydrogen isotope. In a paper on this topic, received by *Physical Review* on 10 December 1932, and published in the issue of 15 February 1933, Wigner started from the 'point of view proposed by Dirac and adopted by [James H.] Bartlett in his discussion of light elements [Bartlett, 1932]' that 'the neutrons are elementary particles and the nuclei are built up by protons, electrons and neutrons' (Wigner, 1933a). Therefore, he replaced Heisenberg's exchange interaction between protons and neutrons by a simple potential $V(r)$. By assuming a suitable form for such a potential—Wigner tried the *Ansatz* $V(r) = 4v_0[1 + \exp(r/\rho)]^{-1}[1 + \exp(-r/\rho)]^{-1}$, with constants v_0 and ρ—he succeeded in fitting the mass defect of the H^2-nucleus and helium nucleus (α-particle); furthermore, he indicated how to arrive at the mass defects of more complex nuclei.[915] The papers of both Majorana and Wigner were discussed in the comprehensive report prepared by Heisenberg for the seventh Solvay Conference, held from 22 to 29 October 1933, in Brussels on the theme '*Considérations théoriques générales sur la structure des noyaux* (General Theoretical Considerations on the Structure of Nuclei)' (Heisenberg, 1934a).[916] Besides Heisenberg's report, others were presented by John Cockcroft ('Disintegration of Elements by Accelerated Protons'), James Chadwick ('Anomalous Scattering of α-Particles and the Transmutation of Elements by α-Particles' and 'The Neutron'), Frédéric Joliot and Irène Curie ('Penetrating Radiation from Atoms Under the Action of α-Rays'), Paul Dirac ('Theory of Positrons'), and George Gamow ('The Origin of γ-Rays and the Nuclear Energy Levels').[917] Gamow, in particular, called attention to the anomalous scattering of high-energy γ-rays by elements of high atomic number—the Meitner–Hupfeld effect—which, according to the British experiments, gave rise to secondary radiation with components 0.5- and 1.0-MeV quantum energy, and he suggested the following explanation: The incident γ-rays produces an artificial β-disintegration which leaves the nuclear proton in an excited state, and eventually the excited nucleus emits a secondary γ-radiation and returns to a ground state (Gamow, 1934, p. 259). Gamow also mentioned another explanation, due to Blackett, namely, that the γ-rays produce electron–positron pairs in the field of the nucleus with the positrons being annihilated then by combining with other electrons.[918]

Heisenberg discussed the status of the theory of nuclear constitution brilliantly in a long report, talking in §1 about principles, in §2 on hypotheses entering into the description of atomic structure, and in §3 on the application of the new quantum-mechanical theory of the nucleus. Evidently, the last part containing a recapitulation of nuclear systematics, i.e., the stability curves listing the binding energy of atomic nuclei *versus* the atomic mass number—the binding energy being defined by the mass defect of a nucleus compared to the sum of masses of its constituents—on the basis of his own work and that of Majorana using statistical models, exhibited a considerable aspect of the new theory. In §2 on hypotheses, Heisenberg first spoke about Gamow's old 'liquid drop model,' which emphasized the α-particle structure of the nucleus, before displaying in greater detail the model

[915] Wigner added some comments on the possible existence of H^3, a hydrogen isotope of mass 3. He concluded: 'It might be therefore that the second neutron is only somewhat (perhaps twice) as strongly bound as the first. The relative occurrence of H^3 would be therefore much rarer than that of H^2.' (Wigner, 1933a, p. 255)

[916] Heisenberg had prepared his report in a close exchange of ideas with Bohr, whom he met in March 1933 on a skiing vacation in the Bavarian Alps and again in early fall on a visit to Copenhagen. Moreover, he communicated with Wolfgang Pauli by correspondence.

[917] The reports and discussions at the seventh Solvay Conference were treated historically by Mehra, 1975a, Chapter 8, pp. 211–226.

[918] We shall discuss the outcome of the Meitner–Hupfeld effect discussions later.

of the proton–neutron structure and the corresponding exchange forces (his own and that of Majorana, advocating in particular the latter).[919]

In his §2 on the hypotheses, Heisenberg also addressed the difficulties 'of treating in a satisfactory manner the question of the stability of a nucleus against β-disintegrations,' which arose from the observed continuous spectrum of the emitted electrons. In particular, he said:

> Pauli has discussed the hypothesis that, simultaneously with the β-rays, another very penetrating radiation always leaves the nucleus—perhaps consisting of "neutrinos" having the electron mass—which takes care of energy and angular momentum conservation in the nucleus. On the other hand, Bohr considers it more probable that there is a failure of the energy concept, and hence also of the conservation laws in nuclear reactions. (Heisenberg, 1934a, p. 315)

Actually, this suggestion came about by an earlier exchange with Wolfgang Pauli on the contents of Heisenberg's Solvay report, carried out in their correspondence between June and October 1933. Originally, in his German manuscript, Heisenberg had written the sentence: 'At the moment it is not clear whether the statement that "energy conservation is violated in β-decay" represents a valid application of the energy concept.' But then he crossed it out and replaced it by the sentence quoted above.[920] Evidently, a letter which Pauli wrote to him on 2 June 1933, persuaded him to do so, because he remarked:

> Concerning nuclear physics I again believe very much in the validity of the energy theorem in β-decay, since other very penetrating light particles will be emitted. I also believe that the symmetry character of the total system as well as the momentum will always be preserved in all nuclear processes. (Pauli, 1985, p. 167)

The development of Pauli's 'neutron hypothesis' has been described in various accounts, first by Pauli himself (Pauli, 1961), and later by several historians of science (e.g., Brown, 1978; Enz, 1981; von Meyenn, 1982; and Peierls, 1982). After his letter of 4 December 1930, to the Tübingen meeting on radioactivity, Pauli mentioned the hypothesis again in a talk at the American Physical Society meeting in Pasadena, 15–22 June 1931, of which no abstract exists except a note in the *Time* Magazine issue of 29 June 1931, with the headline 'Neutron?' stating that

[919] The 'liquid-drop model' of atomic nuclei had emerged at the 1929 Royal Society meeting on the structure of atomic nuclei, where it was explained especially by George Gamow: He assumed 'that all the α-particles which constitute a nucleus are in the same quantum state with quantum number unity'; in 'first rough approximation,' the nucleus was described by two equations as follows: '(1) an equation connecting the energy of α-particles with the surface tension of the imaginary "water drop," and (2) the quantum condition of ordinary quantum mechanics' (Rutherford *et al.*, 1929, p. 386). Gamow then expanded on the model in his book published later (Gamow, 1931). It would be revived later on the basis of the proton–neutron model of the nucleus (see Section IV.5).

[920] See Heisenberg's manuscript, entitled '*Allgemeine theoretische Überlegungen über den Bau der Atomkerne*,' in *Werner-Heisenberg-Archiv*, Munich, p. 27.

Pauli wanted to add a fourth to the 'three unresolvable basic units of the universe' (see Brown, 1978, p. 24). Later that year, Samuel Goudsmit talked at the Rome Conference (in October 1931) about what Pauli had said in Pasadena; in particular, he reported 'that the neutrons [i.e., what Pauli then called 'neutrons'] should have an angular momentum $1/2(h/2\pi)$ and also a magnetic moment and no charge'; further 'they are kept in the nucleus by magnetic forces and are emitted together with β-rays in radioactive disintegration'; thus, 'this might remove the present difficulties in nuclear structure and at the same time in the explanation of the β-ray spectrum, in which it seems that the law of conservation of energy is not fulfilled'; also, 'the mass of the "neutron" has to be very much smaller than that of the proton, otherwise one would have detected the change in the atomic weight after β-emission' (Goudsmit, 1932, p. 41). On his American trip in early summer 1931, Pauli gave another talk on his 'neutron' in Ann Arbor, as J. Robert Oppenheimer and J. Franklin Carlson reported; they also mentioned that the hypothetical particle would explain some cosmic-ray phenomena (Carlson and Oppenheimer, 1931, p. 1787). As Pauli himself recalled, at the Rome Conference in October 1931 (in which he participated, though he apparently arrived late; see Brown, 1978, p. 25), Fermi showed 'immediately a lively interest in my new neutral particle,' whereas Bohr rather preferred his nonconservation arguments. The question was, 'whether from an empirical point of view the beta-spectrum of electrons exhibited a sharp upper limit or a Poisson distribution extending to infinity' (Pauli, 1961, p. 161).[921] In 1932, at the Fifth International Conference on Electricity in Paris, Enrico Fermi mentioned 'Pauli's neutrons,' which 'are emitted simultaneously with β-particles' (Fermi, 1932c, in Fermi, 1962a, p. 498); and in the discussion of his talk, he emphasized 'that these neutrons are not the ones found [by Chadwick] but had a lower mass' (see Segrè, in Fermi, 1962a, p. 488). That is, Fermi had remained favourable to the concept; he even baptized the new particle, as Franco Rasetti recalled:

> The name "neutrinos" was jokingly suggested by Fermi in a conversation with other Rome physicists.... The Italian word for the neutron, *neutrone*, suggests a compound of *neutro*, neutral, and *one*, meaning "a large object"; correspondingly *neutrino* would mean "a small neutral object." (Rasetti, in Fermi, *loc. cit.*, p. 538)

The name 'neutrino' became known to physicists beyond Rome, and at least since the seventh Solvay Conference of October 1933, it was accepted internationally.

Fermi, the godfather of the 'neutrino,' did even more to promote its fame. After returning to Rome from Brussels—where he also attended the Solvay Conference—he thought further about the problem of β-decay and decided that he had to learn second quantization, as Emilio Segrè recalled:

[921] Charles Ellis then promised to investigate the situation more closely, and after a couple of years, he found that Pauli's view was supportable (because a clear upper limit existed for the β-spectrum (Ellis and Mott, 1933).

> He had bypassed creation and annihilation operators in his famous electrodynamics article [Fermi, 1932b], because he could not make them out very well. Now in 1933, he decided he had to understand them. Then he said: "I think I have understood them. Now I am going to make an exercise to check whether I can do something with them." And so he went on to set forth his theory of β-decay, which in his own estimation was probably the most important work he did in theory. (Segrè, 1979, pp. 49–50)

In Brussels, Fermi had been reminded of two important ingredients, Pauli's neutrino idea and Heisenberg's ρ-spin formalism. Then, he sat down and composed the paper entitled '*Tentativo di una teoria dell'emissione dei ragii "beta"* (Attempt at a Theory of β-ray Emission),' which was quickly published in the December issue of the Italian journal *Ricerca Scientifica* (Fermi, 1933); on 16 January 1934, the *Zeitschrift für Physik* received an extended version of his article, as did the Italian journal *Il Nuovo Cimento* (Fermi, 1934a, b).[922]

Fermi stated the essence of his theory in two points:

> [i] Theory of the emission of β-rays from radioactive substances, founded on the hypothesis that the electron emitted from the nuclei do not exist before its disintegration but are being formed, together with a neutrino, in a way analogous to the formation of a quantum of light which accompanies the quantum jump in an atom. [ii] Confrontation of the theory with empirical data. (Fermi, 1933, p. 491)

He basically searched for a quantitative description of β-decay on the basis of the known principles of relativistic quantum field theory, starting from the assumption that 'the total number of electrons and neutrinos in the nucleus is not necessarily constant' and employing Heisenberg's idea to consider 'the heavy particles, neutron and proton, as two quantum states connected with two possible values of an internal coordinate ρ' (Fermi, *loc. cit.*, p. 492)—that is, Fermi treated the heavy particles involved in β-decay in a nonrelativistic approximation. Then, he selected for the interaction energy an *Ansatz* such that in the transition of a nuclear neutron into a nuclear proton (both described by the ρ-formalism) always an electron (ψ)-neutrino (ϕ) pair was created. This led to the specific Hamiltonian,

$$H = QL(\psi\phi) + Q^*L^*(\psi^*\phi^*), \tag{696}$$

where L stood for a bilinear form of the wave functions ψ and ϕ (with the starred operators denoting the Hermitean conjugates). Fermi then restricted L by the

[922] Fermi originally intended to announce the results of his beta-decay theory in a letter to *Nature*, but the manuscript was rejected by the editor of that journal as containing abstract speculations too remote from physical reality to be of interest to readers. He then sent a somewhat longer paper to *Ricerca Scientifica*, where it was promptly published. The more complete articles, including all essential details of the calculation, were then sent to *Zeitschrift für Physik* and *Nuovo Cimento*. But already the first publication contained all results, such as the fit with numerical $F\tau$-values. In our analysis below, we closely follow Brown and Rechenberg, 1988, pp. 986–987.

condition that it behaved under coordinate transformations like the time component of a polar four-vector; hence,

$$L(\psi\phi) = g(\psi_2\phi_1 - \psi_1\phi_2 + \psi_3\phi_4 - \psi_4\phi_3). \tag{697}$$

The constant g in Eq. (697) represented the strength of the β-decay interaction, which Fermi derived by evaluating the frequency $\frac{1}{\tau}$ of β-decays from his theory, i.e.,

$$\frac{1}{\tau} = \text{const. } g^2 qF(\eta_0) \tag{698}$$

—where q is the space integral over the eigenfunctions of the heavy particles (proton and neutron) and $F(\eta_0)$ is a complicated function of the maximum momentum η_0 of the electron—and comparing $\frac{1}{\tau}$ with the observed data. Empirically, the product $\tau F(\eta_0)$ took on values between 1 and 10^2; hence, the coupling constant g became

$$g = 5 \times 10^5 \text{ in units of cm}^5 \text{ g s}^{-2}. \tag{699}$$

It might be added that Fermi also indicated the possibility of a forbidden β-decay, namely, when the neutron–proton space integral q was zero.

As seen from their correspondence, both Pauli and Heisenberg immediately welcomed Fermi's theory. 'Bloch told me interesting things from Fermi,' Pauli wrote to Heisenberg and gave some details about the new theory (Pauli to Heisenberg, 7 January 1934), while Heisenberg enthusiastically replied: '*Das wäre also Wasser auf unsere Mühle*. (This would be grist for our mill.)' (Heisenberg to Pauli, 12 January 1934, in Pauli, 1985, p. 249) Heisenberg would soon generalize the β-decay theory into a theory describing *all* nuclear forces, as we shall discuss below. The story of β-decay continued immediately with an experimental discovery reported from Paris: At the meeting of the *Académie des Sciences* on 15 January 1934, Jean Perrin communicated a note of Irène Curie and Frédéric Joliot entitled '*Un nouveau type de radioactivité* (A New Type of Radioactivity)' (Curie and Joliot, 1934). In pursuing an earlier observation (of June 1933) of the emission of positive electrons from several light elements (beryllium, boron and aluminum) when bombarded by the α-particles from polonium (Curie and Joliot, 1933c), Curie and Joliot discussed the following phenomena:

> The emission of positive electrons by certain light elements, if hit by α-rays from polonium, continues for a longer or shorter period, which could assume more than half an hour in the case of boron after the α-particle source has been removed. (Curie and Joliot, 1934, p. 254)

After giving certain details about the experiments, they proceeded to claim:

> These experiments demonstrate the existence of a new type of radioactivity connected with the emission of positive electrons. We believe that the emission process goes on as follows in the case of aluminum:
>
> $$^{27}_{13}\mathrm{Al} + {}^{4}_{2}\mathrm{He} = {}^{30}_{15}\mathrm{P} + {}^{1}_{0}\mathrm{n}. \qquad [(700)]$$
>
> The isotope $^{30}_{15}\mathrm{P}$ of phosphorus would be radioactive with a period of 3 min 15s, and emit positive electrons according to the reaction
>
> $$^{30}_{15}\mathrm{P} = {}^{30}_{14}\mathrm{S} + \overset{+}{\varepsilon}. \qquad [(701)]$$
>
> (Curie and Joliot, *loc. cit.*, p. 255)

Analogous reactions could occur with boron and magnesium, producing the unstable isotopes $^{13}_{7}\mathrm{N}$ and $^{27}_{14}\mathrm{Si}$, respectively, which—like the isotope $^{30}_{15}\mathrm{P}$—were not observed in nature because of their short decay times. They concluded: 'It has definitely been possible for the first time to create with the help of an external agent the radioactivity of certain nuclei which can continue for a measurable period of time in the absence of the exciting cause.' (Curie and Joliot, *loc. cit.*, p. 256)

F. Joliot and I. Curie quickly informed their colleagues abroad about their discovery of 'a new kind of radio-element' in a short note to *Nature* (Joliot and I. Curie, 1934). The result was immediately accepted, as even before their first announcement had appeared in print, Pauli had written to Heisenberg: 'Do you know that Fermi's theory of β-decay yields for the frequency of processes neutron = proton + electron + neutrino and proton = neutron + positron + neutrino (possibly with the cooperation of energy provided by heavy particles passing by)? These should certainly be observable.' (Pauli to Heisenberg, 21 January 1934, in Pauli, 1985, p. 256). That is, he more or less predicted the observations of Joliot and Curie, and as soon as he saw the note published in *Comptes Rendus* 'with the greatest interest,' he congratulated the French experimentalists 'for this new result' and asked for further details of the positive electron decay, which he considered as proceeding like the usual β-decay with continuous e^+-energy and the joint emission of a neutrino (Pauli to Joliot, 26 January 1934, in Pauli, *loc. cit.*, p. 265). Later that year, Rutherford described their findings as 'the first proof of artificial production of a radioactive element' (Rutherford *et al.*, 1935, p. 14). Already in 1935 the Nobel Prize for Chemistry went to 'Drs. Irène Joliot-Curie and Frédéric Joliot of Paris for their synthesis of new radioactive elements carried out together' (Wilhelm Palmaer in *Les Prix Nobel en 1935*, P. A. Norstedt and Söner, Stockholm, 1937,

p. 38).[923] Their method of creating new radioactive substances exhibiting positive–electron decay should be regarded, Joliot pointed out in his Nobel lecture, as only the beginning of a new epoch extending the wealth of known elements; in this, he referred in particular to the recent experiments of the Rome group under Enrico Fermi, where neutrons were used to stimulate artificial transitions to new elements (Joliot, 1937, p. 3).

The year 1934 thus saw the final clarification of the complex of problems which had bothered physicists since about 1928: It involved the paradoxes of the relativistic electron and its presence in the atomic nucleus.[924] The riddle of the Meitner–Hupfeld effect also got solved. While Meitner and Kösters in spring 1933 concentrated on the investigation of the scattering unshifted in wavelength and confirmed its explanation as being due to nuclear scattering—Max Delbrück in an addendum spoke about ‘a photoeffect caused by one of the infinitely many electrons in the state of negative energy’ (see Meitner and Kösters, 1933, especially, p. 144)—Gray and Tarrant confirmed in a new series of experiments the existence of the shifted 0.5- and 1.0-MeV radiation (Gray and Tarrant, 1934). Patrick Blackett, in an earlier report published on ‘The Positive Electron’ in the *Nature* issue of 16 December 1933, provided the following explanation:

> One would expect that the absorbed energy would be re-radiated in two ways. An ejected positive electron may disappear by the reverse process to that which produced it, that is, by reacting with a negative electron and a nucleus, to give a single quantum of a million volts energy. Or it can disappear, according to Dirac’s theory, by another type of process, in which a positive electron reacts with a free or lightly-bound negative electron so that both disappear with the emission of two quanta of half a million volts energy. (Blackett, 1933, p. 918)

Though the details of this explanation still remained to be confirmed, the deviation from the Klein–Nishina formula must be regarded as finally understood in the essential aspects.[925] Perhaps only one fundamental question remained to be

[923] Irène Curie was born on 12 September 1897, in Paris. She studied physics and mathematics at the University of Paris from 1914 to 1920; during World War I, she served as an X-ray assistant. In 1918, she became an assistant at the Radium Institute of her mother Marie Curie; in 1932, she was promoted there to a leadership position, and from 1946, she directed the Institute. Irène Curie was appointed professor at the Sorbonne in 1937, and from 1946 to 1950, she belonged to the directorate of the French Atomic Energy Commission; then, she built the new nuclear physics laboratory at Orsay. She died on 17 March 1957, in Paris.

Frédéric Joliot, who married Irène Curie in 1926, was born on 19 March 1900, in Paris. In 1920, he began to study physics at the *École Supérieure de Physique et Chimie* with Paul Langevin, and later joined Marie Curie’s Radium Institute as personal assistant to the director. He obtained his doctorate in 1930; in 1937, he became director of the Curie Laboratory with the Radium Institute and professor at *Collège de France*. In 1946, he was appointed High Commissioner of the French Atomic Energy Commission (until 1950). After his wife Irène’s death, he took up the directorship of the Radium Institute, but died already on 14 August 1958, in Paris.

[924] For a review of the situation in late 1933, see Bothe, 1933.

[925] To the study and discussion of the Meitner–Hupfeld effect, during the period 1933–1934, the following papers also contributed: Oppenheimer and Plesset, 1933; Fermi and Uhlenbeck, 1933; and Joliot, 1934.

answered, namely, whether the neutrino could be detected experimentally and what mass it possessed. While the observations in Cambridge spoke in favour of a zero mass (Henderson, 1934), the direct search for Pauli's neutrino failed to be successful until much later (see, e.g., Chadwick and Lea, 1934).

(e) Universal Nuclear Forces and Yukawa's New Intermediate Mass Particle (1933–1937)

Hideki Yukawa of the Kyoto Imperial University recalled his active entrance into the problem which had bothered him already for some time, especially since he had eagerly studied the discoveries of the year 1932, including the neutron and the ensuing theory of atomic structure:

> In April 1933, the *Physico-Mathematical Society of Japan* [*PMSJ*] held a meeting at Tohuko University in Sendai. On this occasion I gave my first research report on the subject "The Electrons Within the Nuclei." I did not have very much confidence in this research and did not, in the long run, publish the paper in the journal. There were many obvious difficulties in treating the electron as the "ball" exchanged between neutron and proton. In the first place, the electron's characteristics, such as its spin and the kind of statistics it obeys, make the electron unsuitable for this role. Nevertheless, I tried to use the electron field that satisfies Dirac's wave equation as the field of nuclear force. (Yukawa, 1982, p. 196)

Yukawa pondered about the consequences concerning the nature of nuclear forces, and began one of the manuscripts related to his talk at Sendai by stating: 'The nucleus, especially the problems of the nuclear electrons, are so intimately related with the problems of the relativistic formulation of quantum mechanics that when they are solved, if they ever will be solved at all, they will be solved together.'[926] That is, Yukawa still connected, in agreement with some familiar ideas of Heisenberg and others, the problems of nuclear physics with those of relativistic quantum field theory. In a summary of Heisenberg's work on nuclear structure, which Yukawa discussed a little later in 1933, he also addressed a major defect:

> In this paper Heisenberg ignored the difficult problems of electrons within the nucleus, and under the assumption that all nuclei consist of protons and neutrons only, considered what conclusions can be drawn from the present quantum mechanics. This essentially means that he transferred the problem of the electron in the nucleus to the problem of the makeup of the neutron itself, but it is also true that the limit to which the present quantum mechanics can be applied to the atomic nucleus is widened by this approach. Though Heisenberg does not present a definite view on whether the neutrons should be seen as separate entities or a combination of a proton and an electron, this problem like the β-decay problem stated above, cannot be

[926] See the unpublished manuscript of Hideki Yukawa, entitled 'On the Problem of Nuclear Forces. I,' and dated early 1933 ([*YHAL*] E05030U1), quoted in Brown, 1989, p. 20, footnote 23.

> resolved with today's theory. And unless these problems are resolved, one cannot say whether the view that electrons have no independent existence in the nucleus is correct. (Yukawa, 1933a, p. 195)[927]

In spring 1933, Yukawa wished to proceed a step further toward a fundamental theory of nuclear forces and β-decay.[928] He first rejected Heisenberg's idea of a complex neutron.[929] Second, he attempted to formulate the charge-exchange force in analogy to quantum electrodynamics by assuming explicitly that the exchange of an electron between a neutron and a proton would produce the nuclear force, just as a photon provided the electromagnetic force in quantum electrodynamics. Third, he made use of Dirac's relativistic electron equation, in which he included a source term J depending on the neutron and the proton wave functions; i.e.,

$$D\psi = J, \tag{702}$$

where D denoted a 4×4 matrix differential operator *à la* Dirac, including the electromagnetic potentials. J possessed a form somehow similar to Dirac's electromagnetic current j_μ, but it was a more complicated quantity: It transformed like a spinor (as it contained Dirac matrices acting upon a spinor) and involved the ρ-spin matrices (changing a neutron into a proton and *vice versa*). Yukawa then tried to write the correct equation and to solve it properly; however, the solution exhibited 'a form like the Coulomb field,' modified by an exponential factor $\exp i\rho_3\left(mc|\boldsymbol{r}' - \boldsymbol{r}|/\dfrac{h}{2\pi}\right)$; hence, it did 'not decrease sufficiently with distance.'[930] In the published abstract of the talk at Sendai on 3 April 1933, Yukawa claimed to have obtained an exponential decrease of the nuclear charge-exchange force with a range given by the quantity $\left(\dfrac{h}{2\pi}\right)/mc$, where m denoted the mass of the electron [Yukawa, 1933b, p. 131(A)].[931] However, in the manuscript which he actually read he withdrew the result by stating:

> In any case, the practical calculation does not yield the looked-for result that the interaction term decreases rapidly as the distance becomes larger than $(h/2\pi mc)$, unlike I wrote in the abstract of this talk. (Yukawa, manuscript entitled 'A Comment on the Problem of Electrons in the Nucleus,' [*YHAL*] E05080U01, translated in Kawabe, 1991a, pp. 248–249, especially, p. 249)

[927] For a translation of the introduction and more details of Yukawa's first publication, see Brown, 1981, pp. 96–97 and pp. 121–122.

[928] The details of Yukawa's concern with nuclear forces have been treated in the following publications: Brown, 1981, 1985, 1986, 1989 and 1990; Kawabe, 1991a; and Brown and Rechenberg, 1996, Chapter 5.

[929] See Yukawa's unpublished manuscript [*YHAL*] E05060U01 in the Yukawa Hall Archival Library, Kyoto.

[930] See the manuscript cited in Footnote 929.

[931] For a translation, see Kawabe, 1991a, p. 247.

In spite of the negative conclusion following from his endeavours, Yukawa found that certain colleagues thought the approach worthwhile to be pursued. Thus, the senior physicist Yoshio Nishina from Tokyo proposed to him—in order to take care of the problems observed with nuclear statistics (i.e., the wrong statistics for some nuclei in the electron–proton model) and β-decay nuclei in the electron–proton model (i.e., the apparent violation of conservation laws)—to introduce instead of a real electron a 'Bose electron'; however, Yukawa was not ready to renounce in 1933 'the conservative desire to understand nature in terms of known particles' (Yukawa, 1982, p. 196). Therefore, he rather turned to adopt Bohr's idea of having some violation of conservation laws in nuclear physics, as did others at that time.

For example, Guido Beck and Kurt Sitte from Prague published between 1933 and 1934 a series of papers, in which they outlined a new theory of β-decay (Beck and Sitte, 1933, 1934; Beck, 1933).[932] In particular, they assumed the following picture of the physical process: A virtual electron–positron pair was created in the strong nuclear potential, of which then the nucleus absorbed the positron and the electron escaped in such a way that the properties of the positron except its charge (which increased the charge of the nucleus by one unit) got lost (e.g., its spin, magnetic moment and energy) or absorbed by the nucleus. At the Solvay Conference in October 1933, Niels Bohr advocated the Beck–Sitte approach in the discussion of Gamow's talk,[933] and Beck still stuck to it in a contribution to the International Conference on Physics at London in fall 1934 (Beck and Sitte, 1935), in spite of the evolution of Enrico Fermi's successful theory which satisfied all conservation laws (Fermi, 1933; 1934a, b). By that time, other former advocates of the violation of conservation laws in the nucleus—notably, Werner Heisenberg—had turned over to Fermi's theory, including Pauli's neutrino's hypothesis. Heisenberg found Fermi's approach not only attractive for describing β-decay but immediately noticed another very appealing possible application, as he wrote to Pauli without delay:

> If Fermi's matrix elements are correct for the creation of the pair electron plus neutrino, then they must—just as in the case of atomic electrons the possibility of creation of light-quanta leads to the Coulomb force—yield in the second approximation a force between neutron and proton. I have computed these forces, and there it turns out that an *exchange* interaction results between neutron and proton which—depending on the *Ansatz* for the Fermi matrix element—has either the form of Majorana's or mine. As the exchange integral $I(r)$ there results essentially
>
> $$J(r) = \frac{\text{const.}}{r^5}, \qquad [(703)]$$

[932] The development of β-decay theory and its extension into a unified theory of all nuclear forces has been discussed especially by Brown and Rechenberg, 1994, and Brown and Rechenberg, 1996, Chapter 3.

[933] See Gamow, 1934, p. 287.

> which, however, becomes wrong for distances $r \leq \left(\frac{h}{2\pi}\right)/Mc$ [i.e., the Compton wavelength of the proton]. (Heisenberg to Pauli, 18 January 1934, in Pauli, 1985, p. 250)

In his letter to Pauli, Heisenberg then sketched the actual calculation and estimated the magnitude of the exchange energy as

$$J(r) \sim mc^2(10^{-14}/r)^5, \tag{704}$$

noting that it came out to be 'quite (*reichlich*) small' and added: 'However, that may not be a misfortune when considering the sloppiness of the calculation.' (Heisenberg to Pauli, *loc. cit.*, p. 252)[934]

But in spite of the encouraging observation that the exchange of nuclear forces now seemed to be established as a second-order approximation in the combined electron–neutrino field of Fermi, for reasonable values of the nuclear radii (about 2×10^{-13} cm), the integral (704) continued to come out too small by a factor of a million. Still, Heisenberg remained optimistic, since also the recent experimental work of Irène Curie and Frédéric Joliot (1934) confirmed 'wonderfully the [theoretical] work of Fermi on β-decay as well as the exchange forces between the neutrons and proton' (Heisenberg to Pauli, 8 February 1934, in Pauli, 1985, p. 281). Heisenberg rather argued that the effective value of the exchange force derived from the electron–neutrino field theory depended strongly on the behaviour of $I(r)$ at very small r, and that the usual perturbation-theoretical calculation should not make sense for distances r less than the proton's Compton wavelength.[935] Fermi, whom Heisenberg told about his idea of deriving the exchange forces from the β-decay theory, replied that he had thought along similar lines and found that 'the interaction which arises has the right form, but is quantitatively much too small' (Fermi to Heisenberg, 30 January 1934). On the other hand, he suggested the possibility of obtaining a larger exchange force which should arise by taking into account the scalar and longitudinal components of the electron–neutrino field (similar to the case of quantum electrodynamics where these components provided the comparatively strong electric Coulomb field). While this idea seemed to point into the right direction, Fermi did not see any possibility to play much with the magnitude of his coupling constant, as Heisenberg had also suggested. Several months later, Gian Carlo Wick, a member of Fermi's institute in Rome, sent a

[934] It should be mentioned that Heisenberg's enthusiasm for the neutrino also influenced Niels Bohr to reconsider the return of conservation laws to nuclear physics. Thus, Bohr admitted that he was 'completely prepared to accept that we here really have a new situation which may be equivalent to the real existence of neutrinos' (Bohr to Heisenberg, 15 March 1934).

[935] We should recall that Heisenberg had contemplated that β-decay forces gave rise to nuclear exchange forces already in summer 1933, when he wrote to Pauli: 'From the standpoint of your theory one would always have to say: [neutron] decay is into electron, proton and neutrino. Also then, the exchange force should be present.' (Heisenberg to Pauli, 17 July 1933, in Pauli, 1985, p. 195)

letter to Heisenberg at Leipzig, and reported about a 'crazy idea': Perhaps the smallness of Fermi's constant did not imply such a hopeless situation for explaining the nuclear forces because, first, Fermi's original form of the β-decay Hamiltonian, Eq. (696), might not be exact; second, Heisenberg's exchange force actually involved much shorter wavelengths for the electron than did those involved in β-decay; hence, the discrepancy in magnitude between theory and experimental forces could be blamed—as Heisenberg had suggested in his letters to Pauli—on the extrapolation (and possibly the nonapplicability of quantum mechanics at very small distances); third, since an evaluation of the probability for the virtual dissociation process ($n \to p + e^- + \nu$ or $p \to n + e^+ + \nu$) yielded unity, the theory might explain the anomalous value of the proton's magnetic moment, as recently observed by Otto Stern and his collaborators (Frisch and Stern, 1933b; Estermann and Stern, 1933). 'However,' Wick closed his letter to Heisenberg by saying, 'please don't think that I believe all this.'[936]

The fruitful exchange with his Italian friends and colleagues stimulated Heisenberg greatly to employ the extension of Fermi's theory in a series of four Scott Lectures at Cambridge, which Rutherford had invited him to present between 23 and 30 April 1934.[937] In the introduction of these lectures, Heisenberg stressed a fundamental point that entered into his new treatment of nuclear theory: 'In all cases, where one can really follow all the details of [nuclear] processes, one finds that light-quanta or electrons are emitted *after* the collision.' Consequently, as suggested by Fermi's theory of β-decay, the emitted particles were created in nuclear processes and had not existed before in the nuclei. Heisenberg thus drew the following analogy between atoms and nuclei: (i) just as atoms consist of electrons and the atomic nucleus, so do nuclei consist just of protons, neutrons and α-particles; (ii) just as atoms emit light-quanta *after* the collision with electrons (Franck–Hertz experiment), so do nuclei emit electrons, positrons, or light-quanta *after* a collision.[938] In the third Scott Lecture, Heisenberg then established the detailed connection between Fermi's description of the β-decay and the force-law between neutrons and protons. He summarized at the end:

> It seems possible to describe the nuclei to a large extent with the formulation of Fermi; that means: instead of a Maxwell field and the charge e another field plays the important role, the characteristic constant being [Fermi's coupling constant] g [see Eq. (697)]. It seems that especially the neutrons and neutrinos have nothing whatever to do with a Maxwell field but they have to do with this g-field [consisting of the electron-neutrino pair].

[936] And yet, he published later on the idea of explaining the anomalous magnetic moment of the proton via the action of the electron–neutrino field (Wick, 1935).

[937] Heisenberg's Scott Lectures dealt with the topic 'Quantum Theory of the Constitution of Atomic Nuclei.' A handwritten manuscript of Heisenberg's, outlining the sketch of the lectures in 18 pages, exists in the *Werner-Heisenberg-Archiv* in Munich. We shall quote from it in the following. See also Brown and Rechenberg, 1994; 1996, Chapter 3.

[938] Only the helium nuclei, resulting in α-decays, seemed to be already present in the nuclei.

In his lecture, Heisenberg admitted the smallness of the neutron-proton exchange force following from the present theory, namely by a factor of 10^{-10}, but he also stressed the difference between this force and the forces determining the ‘Fermi process,’ i.e., β-decay: the latter corresponded in the atomic analogy to the emission of radiation, while the former to the much larger Coulomb force.

Other theoreticians picked up the topic as well, probably independently of Heisenberg, notably Igor Tamm in Moscow (1934a), Dmitrij Iwanenko in Leningrad (1934) and Arnold Nordsieck in Ann Arbor (1934).[939] Like Heisenberg, they found an exchange force decreasing with the inverse of the fifth power of the distance; but in order to obtain the observed magnitude of the nuclear binding energy (about 1 MeV), one had to assume a mean distance of 10^{-15} cm, about 100 times smaller than the observed range of nuclear forces. Evidently, this theoretical result did not describe nature; hence, Tamm proposed—in a letter to *Nature*—a generalization of Fermi’s theory (Tamm, 1934b). Again, he obtained a r^{-5}-potential, now multiplied by the constants η_1 and η_2 (denoting ‘neutral’ charges of the neutrons and protons, respectively), but it now even seemed to fit the empirical data.[940] On the other hand, in the discussion at the International Conference on Physics, held in London and Cambridge in October 1934, Hans Bethe made another proposal: He and Rudolf Peierls had been discussing alternative forms of Fermi’s interaction, which seemed to describe the low-energy spectra of β-decays better than the original version, Eqs. (696) and (697). That is, they introduced derivatives of the fields entering into the Hamiltonian; since the procedure suppressed the β-decay interaction, a larger coupling constant than Fermi’s g, Eq. (698), had to be used in order to fit the data (Bethe *et al.*, 1935, p. 66). Heisenberg, who had heard about the Bethe–Peierls *Ansatz* earlier (in September 1934) in Copenhagen, was quite pleased with it, and he wrote to Pauli about it:

> Bethe now proposes, e.g.,
>
> $$g\int \Psi_{\text{neutron}}\Phi_{\text{proton}}\frac{\partial\psi_{\text{neutrino}}}{\partial x}\frac{\partial\phi_{\text{electron}}}{\partial x}\,dV \qquad [(705)]$$
>
> [for the interaction Hamiltonian] ... If one makes use of Bethe’s *Ansatz*, then $g \sim 10^{-69}$ erg cm^3, and the exchange force becomes
>
> $$\frac{g^2}{hc}\frac{1}{r^9} \approx \left(\frac{10^{-13}}{3}\right)^9 \text{ erg}, \qquad [(706)]$$

[939] On 13 May 1934, Tamm wrote to Dirac, enclosing ‘a note on some consequences of Fermi’s theory,’ which he asked Dirac to submit to *Nature*. Dirac wrote back: ‘I sent your note to *Nature* and the editor has accepted it, together with a note from Iwanenko. I shall read the proofs.’ (Dirac to Tamm, 7 June 1934). See Kojevnikov, 1996, p. 14 and p. 17.

[940] As Tamm wrote to Dirac later, ‘neutrons and protons are *polar* with these forces, i.e., if we disregard Coulomb forces, two protons and two neutrons repel one another with the same force, with a neutron and a proton attracting one another’ (Tamm to Dirac, 27 April 1935; see Kojevnikov, 1996, p. 25).

> hence one obtains the right order of magnitude. Of course, Bethe's *Ansatz* need not be the correct one; but in any case, one recognizes: there exist simple modifications of Fermi's theory, suggested by experiment, which also yield the correct exchange forces. (Heisenberg to Pauli, 28 October 1934, in Pauli, 1985, p. 355)

In contrast to Heisenberg, Pauli showed no enthusiasm for the new development, because the interaction term [(705)] appeared to him to be quite arbitrary, which was not derived from a general principle. Hence, he concluded that 'there exists no agreement in favour nor any against the assumption of a connection between β-radioactivity and proton-neutron exchange forces' (Pauli to Heisenberg, 1 November 1934, in Pauli, 1985, p. 357). Still, Heisenberg remained positive and optimistic; he wrote to his former student Peierls to publish a note in *Nature* about it, and added that also Fermi and Wick liked the idea (Heisenberg to Peierls, 28 January 1935).[941]

The first detailed paper on the 'unified' theory of nuclear forces was submitted by Heisenberg in February 1935 to the *Zeeman Festschrift*; his contribution, entitled '*Bemerkungen zur Theorie des Atomkerns* (Remarks on the Theory of the Atomic Nucleus),' summarized both his own ideas and those of others on the subject (Heisenberg, 1935a).[942] Heisenberg first described the analogy between the new theory in a table relating the electromagnetic forces governing the atoms and the forces governing the atomic nuclei, where the Maxwell field was partly replaced by what he called 'the Fermi field': the latter determined the exchange forces between the elementary constituents protons and neutrons and simultaneously led to the emission of electrons, positrons, and neutrinos, while the former gave rise to Coulomb forces (binding protons and electrons in an atom) and the emission of light-quanta (Heisenberg, 1935a, p. 110). Like the Maxwell field, the Fermi field was considered to be a local field, and it permitted 'in principle the mathematical execution of the idea that the existence of exchange forces follows from the possibility of β-decay' (Heisenberg, *loc. cit.*, p. 112). Further, Heisenberg introduced the idea of Bethe and Peierls to replace the original Fermi interaction by expressions containing the derivatives of the wave functions (of the particles involved in β-decay), which resulted in exchange forces between protons and neutrons varying like r^{-7} or r^{-9} (depending on whether one took one or two derivatives). Of course, Heisenberg was aware of the fact that these forces led in principle to infinite self-energy of proton and neutron, which then had to be avoided by assuming appropriate radii for these particles. In spite of using such tricks—Pauli spoke of a 'nuclear physics of indefinite functions' (Pauli to Heisenberg, 1 November 1934, in Pauli, 1985, p. 357)—the 'Fermi field theory' became for the next couple of years the standard theory of nuclear forces, advocated in the

[941] Bethe and Peierls did not comply, but eventually E. J. Konopinski and George E. Uhlenbeck would publish the same theory of the β-decay interaction, apparently without knowing about their predecessors (Konopinski and Uhlenbeck, 1935).

[942] Heisenberg had presented the main contents of this paper already in September 1934 at a meeting in Copenhagen.

new book by Carl Fredrich von Weizsäcker, *Die Atomkerne* (von Weizsäcker, 1937a), but also in the second edition of George Gamow's monograph of 1931, now entitled *Structure of Atomic Nuclei and Nuclear Transformations* (Gamow, 1937). Moreover, the extremely influential review articles of Hans Bethe and collaborators on nuclear physics in the *Review of Modern Physics*—the so-called 'Bethe bible'—were based on the same fundamental theory of nuclear forces (Bethe and Bacher, 1936; Bethe, 1937; Livingston and Bethe, 1937).[943] Indeed, until about mid-1937, no mention occurred of a competing theory which had been introduced as early as 17 November 1934, in a meeting of the *Physico-Mathematical Society of Japan* at Osaka and published in the English language issue (January–February) of the journal of this society (Yukawa, 1935).

Yukawa had moved to Osaka after his marriage in April 1932, where a colleague at the Osaka Imperial University directed his attention to Fermi's German publication of the β-decay theory, including Pauli's neutrino (Fermi, 1934a).[944] 'I was not aware of Pauli's arguments,' he said later, and added:

> Fermi, however, had based his theory of beta-decay on Pauli's idea. After reading Fermi, I wondered whether the problem of the strong nuclear forces could be solved in the same manner. That is to say, could neutrons and protons be playing "catch" with a *pair* of particles, namely the electron and the neutrino? The "ball" would be replaced by a pair of particles. (Yukawa, 1981, p. 201)

Now, while Yukawa thought along such lines, the physicists in Europe had been pondering about the same ideas; when he saw the notes of Tamm (1934a) and Iwanenko (1934) in the *Nature* issue of 30 June 1934, he noticed that 'the results were negative,' because the resulting force turned out to be 'incomparably smaller than the nuclear force.' He 'was heartened by the negative result,' which 'opened his eyes ... not [to] look for the particle that belongs to the field of the nuclear force among the known particles, including the new neutrino' (Yukawa, 1981,

[943] For details and the further development of the Fermi-field theory, see Brown and Rechenberg, 1994; 1996, Chapter 3.

[944] Hideki Yukawa was born on 23 January 1907, in Tokyo, the fifth of seven children of Takuji and Koyuki Ogawa. His father, a geologist in state service, became a professor of geology at the Kyoto Imperial University in 1908. The family moved to Kyoto, and Hideki received his education there. In high school, Sin-itiro Tomonaga was his classmate, and then fellow student at Kyoto Imperial University, which both entered in 1926 and from which they graduated three years later; they stayed on there until 1932 as unpaid assistants. Then, Tomonaga joined Yoshio Nishina's institute at RIKEN in Tokyo, while Yukawa was appointed a lecturer at Kyoto Imperial University. In 1932, he got married and was adopted—according to custom—into the family of his wife, surnamed 'Yukawa,' which he also assumed. In 1933, he obtained the position of a lecturer at Osaka Imperial University (but also retained his position in Kyoto); in 1936, he was promoted to an associate professorship in Osaka, and finally to a full professorship at Kyoto University in fall 1939. From 1948 to 1953, he lived and worked in the United States, first as a visiting professor at the Institute for Advanced Study in Princeton and then (from 1949) as a professor of physics at Columbia University. In 1953, the 'Research Institute for Fundamental Physics' was established in Kyoto, and Yukawa—the first Japanese to be honoured with the Nobel Prize for Physics in 1949—returned to Japan. He retired from Kyoto University in 1970 and died on 8 September 1981, in Kyoto. For details of Yukawa's life, see Brown, 1990.

p. 201). Yukawa rather turned the problem around and then started from the properties of the nuclear force field in order to derive the characteristics of the object he was looking for. 'The crucial point came to me one night in October [1934],' he recalled:

> The nuclear force is effective at extremely small distances, of the order of 0.02 trillionth of a centimetre. That much I knew already. My new insight was the realization that this distance and the mass of the new particle that I was seeking are inversely related to each other. Why had I not noticed that before? The next morning I tackled the problem of the mass of the new particle and found it to be about two hundred times that of the electron. It also had to have the charge of plus or minus that of the electron. (Yukawa, *loc. cit.*, p. 202)

Actually, Yukawa had come across the relation between the range of the nuclear force and the mass of the exchanged particle already in spring 1933, when he considered the electron to be responsible for Heisenberg's nuclear exchange forces. Now, one-and-a-half years later, he investigated in greater detail all properties of the observed nuclear force, including its range; thus, he realized that the electron, due to its mass, would be connected with a much larger range than found empirically. Still, a further question had to be considered, namely, why an elementary particle of 200 electron masses had not yet been detected in nature. He found that 'the answer was simple,' as 'an energy of 100 million electron volts would be needed to create such a particle, and there was no accelerator, at that time, with that much energy available.' (Yukawa, *loc. cit.*, pp. 202–203)

The following weeks in fall 1934 were filled eagerly with work, of which the documents preserved bear ample witness. In several sets of notes, Yukawa dealt in particular with the wave equation that should be obeyed by the postulated new particle of mass m_U, which was related to the range of the nuclear potential, λ^{-1}, as[945]

$$m_U c = \lambda \frac{h}{2\pi}. \tag{707}$$

On 17 November 1934, Yukawa presented an outline of the material at the meeting of the *Physico-Mathematical Society of Japan* in Osaka; he was allotted only ten minutes for his talk, entitled 'On the Interaction of Elementary Particles,' which he evidently used to advantage, because he found: 'Professor Nishina was very interested in the theory.' (Yukawa, *loc. cit.*, p. 203)[946] Immediately after the

[945] The Yukawa Hall Archival Library [*YHAL*] has four sets of calculations containing the proton–neutron force problem, dated October 1934, plus a few drafts entitled 'On the Interaction of Elementary Particles'; the first of the latter (and the only one in Japanese) was dated 27 October 1934, and probably contained the contents of the talk at Osaka (to be discussed below).

[946] The programme of the meeting, which started at 1.30 p.m., listed seven topics, from 'Meromorphic Functions' to 'The Polarity of Thunder Clouds'—the latter talk was allotted 40 minutes (see [*YHAL*] E01090P1). For details of the story, see Brown and Rechenberg, 1996, Chapter 5, and the papers of Brown, referred to in Footnote 928.

meeting, he sat down and composed a paper in English which he submitted to the *Physico-Mathematical Society of Japan*; it was received on 30 November 1934, and appeared in the January–February 1935 issue of that journal (Yukawa, 1935).

In the introduction of his first paper dealing entirely with his own research, Yukawa referred to Heisenberg's '*Platzwechsel*' forces of 1932 and Fermi's treatment of the β-disintegration of 1933/34; he drew attention to the smallness of the forces involved in Fermi's theory of β-decay, which could not 'account for the binding energies of neutrons and protons in the nucleus' (Yukawa, *loc. cit.*, p. 48). He continued:

> To remove this defect, it seems natural to modify the theory of Heisenberg and Fermi in the following way. The transition of a heavy particle from neutron state to proton state is not always accompanied by the emission of light particles, i.e., a neutrino and an electron, but the energy liberated by the transition is taken up sometimes by another heavy particle, which in turn will be transformed from proton state to neutron state. (Yukawa, *loc. cit.*)

If the probability of occurrence of the proton–neutron transition was much greater than that of the β-decay transition, Yukawa now argued, then the proton–neutron interaction must be much larger than that given by the Fermi-field theory, and 'such an interaction between the elementary particles [i.e., proton and neutron] can be described by means of a field of force, just as the interaction between charged particles is described by the electromagnetic field' (Yukawa, *loc. cit.*). Then, he came to the main points: 'The above considerations show that the interaction of heavy particles with this field is much larger than that of the light particles with it' (Yukawa, *loc. cit.*), and finally: 'In the quantum theory this field should be accompanied by a new sort of quantum [which Yukawa and others would call the "heavy quantum" later], just as the electromagnetic field is accompanied by the photon.' (Yukawa, *loc. cit.*, p. 49)

Yukawa thus developed his formalism—as Fermi and especially Heisenberg had done—in analogy with the electromagnetic field.[947] On replacing the $\frac{1}{r}$-potential of the latter by the corresponding nuclear potential,

$$U(r) = \pm \frac{g}{r} \exp(-\lambda r), \tag{708}$$

where g denoted a constant having the dimension of an electric charge, and λ the inverse range, Yukawa noted that $U(r)$ constituted the spherically symmetric static solution of the generalized wave equation

$$\left(\Delta - \frac{1}{c^2}\frac{\partial^2}{\partial t^2}\right) U = 0, \tag{709}$$

[947] That is, the theories of Fermi, Heisenberg, and Yukawa represented—apart from occasional nonrelativistic approximations to describe proton and neutron, the heavy nuclear particles—relativistic quantum field theories implying creation and annihilation operators.

with Δ the d'Alembertian operator $\left(=\frac{\partial^2}{\partial x^2}+\frac{\partial^2}{\partial y^2}+\frac{\partial^2}{\partial z^2}\right)$. In the presence of heavy particles (i.e., protons and neutrons), then, the zero on the right-hand side of Eq. (709) turned into a source term J describing the transition of the neutron to the proton state; that is

$$J = -4\pi g \tilde{\Psi} \frac{\tau_1 - i\tau_2}{2} \Psi, \tag{710}$$

where Ψ (and its complex conjugate $\tilde{\Psi}$) were the wave functions for the heavy particles (being functions of space, time, and τ_3—the third component of Heisenberg's ρ-spin—took on the values $+1$ or -1).[948]

At this point, it should be emphasized that Yukawa maintained the full analogy of his U-field with the electromagnetic four-vector potential, though he disregarded the (three-dimensional) vector for the moment, as 'there's no correct relativistic theory for the heavy particles' (Yukawa, *loc. cit.*, p. 50). Thus, he obtained from a single (quasi-scalar) nonrelativistic Schrödinger equation the effective potential between two heavy nuclear particles, and then observed that 'this Hamiltonian is equivalent to Heisenberg's Hamiltonian ... if we take the "*Platzwechsel* integral" $J(r) = -g\exp(-\lambda r)$' (Yukawa, *loc. cit.*, p. 51). The two constants g and λ finally followed from experiment, and ultimately, via Eq. (707), with $\lambda = 5 \times 10^{12}\ \mathrm{cm}^{-1}$ the mass m_U of the U-field resulted as '2×10^2 times as large as the electron mass' (Yukawa, *loc. cit.*, p. 53). He explained why this particle had not been observed so far in nuclear transformations by showing that the energies required to produce it were not available in known nuclear reactions.

Yukawa did not stop with these considerations of the nuclear forces between heavy particles. He assumed that the U-quantum (of the U-field) could couple to another charge-changing current, different from J in Eq. (710), namely, to that where the electron and neutrino fields (associated with the light nuclear particles) replaced those of the proton and the neutron. Thus, he proposed an alternative theory of β-decay to Fermi's by just taking into account the additional source term J',

$$J' = -4\pi g' \sum \tilde{\psi}_k \phi_k \tag{711}$$

(which had to be added to the right-hand side of Eq. (709)), with ψ_k and ϕ_k denoting the electron and neutrino fields and g' denoting a second coupling constant. By comparing the matrix element calculated from Eq. (711) for β-decay with Fermi's, he found the relation

$$\frac{4\pi g g'}{\lambda^2} = g_{\mathrm{Fermi}} = 4 \times 10^{-50}\ \mathrm{cm}^3 \cdot \mathrm{erg}; \tag{712}$$

[948] The U-field carried a positive charge for $(p \to n)$-transition, while the conjugate U-field described the $(n \to p)$-transition.

hence, upon inserting $g = 2 \times 10^{-9}$ and $\lambda = 5 \times 10^{12}$, he obtained $g' = 4 \times 10^{-17}$. He concluded the paper by saying: 'This means that the interaction between the neutrino and the electron is much smaller than between the neutron and the proton, so that the neutrino will be far more penetrating than the neutron and consequently more difficult to observe.' (Yukawa, *loc. cit.*, p. 56) That is, he expounded here clearly for the first time not only the existence of two kinds of nuclear forces, a strong one and a weak one, but he also considered his 'quantum with large mass' (Yukawa, *loc. cit.*, p. 53) as a kind of what one would later call a 'unified intermediate boson,' which coupled both to particles having strong and weak nuclear reactions.

Having completed this pioneering paper 'On the Interaction of Elementary Particles. I,' Yukawa pushed ahead and continued working on his new theory in the following months. Indeed, a number of manuscripts related to a further article, 'On the Interaction of Elementary Particles. II,' exist in the Yukawa Hall Archives, as well as a manuscript on a related talk presented at the annual meeting of the *Physico-Mathematical Society of Japan* (*PMSJ*) on 6 April 1935, dated 30 March. A still earlier memorandum, dated 19 March, emphasized the 'defects' of the published Part I as follows:

(1) Only the exchange force was considered.
(2) The forces between like particles were not considered.
(3) The spin-dependence was not considered.
(4) The range of the parameter λ and the coupling constant determined from the collision theory and from cosmic ray bursts become rather large, so the mass of the U-quantum is large.
(5) The interaction of the charged U-quantum with the electromagnetic field was not investigated. (Yukawa, Memorandum, filed as [*YHAL*] F03090 P12.)

Although Yukawa became very productive scientifically in the following one-and-a-half years, and published seven papers either alone or mostly in collaboration with his student Soichi Sakata, none of them continued the pioneering study of 1935.[949] Finally, in fall 1936, after the lapse of one-and-a-half years, Yukawa began to work and talk again about a second investigation involving U-quanta. Thus, the programme of the *PMSJ* meeting on 28 November listed as the last of 11 five-minute talks: 'Yukawa, On the Interaction of Elementary Particles. II.'[950] Yukawa's renewed interest, which eventually resulted in two weighty publications, submitted in November 1937 and March 1938, respectively (Yukawa and Sakata, 1937; Yukawa, *et al.*, 1938), must be attributed to one main reason: the discovery of a new particle in cosmic radiation which seemed to have the properties of the

[949] Just in one of the papers on β-decay and allied phenomena, a reference occurred (Yukawa and Sakata, 1935, p. 469). For details of Yukawa's work between 1935 an 1937, see Hayakawa, 1983, Rechenberg and Brown, 1990, and Brown and Rechenberg, 1996, Chapter 6.

[950] A manuscript of this talk exists: [*YHAL*] E02060 P13, the content of which has been discussed by Hayakawa, 1983, pp. 88–89.

hypothetical *U*-quantum. Indeed, in a letter dated 18 January 1937, addressed to the British journal *Nature*, Yukawa advocated 'A Consistent Theory of the Nuclear Force and β-Disintegration.'[951] After referring to the 'β-hypothesis of nuclear forces' (i.e., the fashionable Fermi-field theory) and its 'well-known inconsistency between the small probability for the β-decay and the large interaction of the neutron and proton,' he drew attention of the Western scientific community to 'one possible way of solving this difficulty which was proposed by the present writer about two years ago.' He then outlined briefly his *U*-quantum theory and concluded the letter by discussing certain results of a recent article of the California experimentalists Carl Anderson and Seth Neddermeyer:

> Now it is not altogether impossible that the anomalous tracks discovered by Anderson and Neddermeyer [1936], which are likely to belong to unknown rays with $\frac{e}{m}$ larger than that of the proton, are really due to such [*U*-] quanta, as the range-curvature relation of these tracks are not in contradiction to this hypothesis. At present, much reserve is, of course, indispensable owing to the scantiness of the experimental information.

No documented reaction from the editor of *Nature* has survived, but Yukawa recalled later that 'soon the manuscript was sent back with the reply that it could not be printed in that journal because there was no experimental evidence to support my idea' (see Kawabe, 1991b, p. 263).

Actually, unknown tracks with $\frac{e}{m}$ larger than that of the proton and smaller than that of the electron or positron had already been observed several years earlier. In a paper submitted in May 1933, Paul Kunze of the University of Rostock analyzed some individual tracks obtained with his cloud chamber operating in a uniform strong magnetic field of 18,000 gauss.[952] In particular, he described the following noteworthy feature:

> The other double track shows in the same neighbourhood a thin track of an electron having 37 million [electron] volts and another of a positive particle of smaller curvature, which ionizes much more strongly. The nature of the latter particle is not known; it ionizes too little for a proton and too much for a positive electron. This double track is probably a part of a "shower" of particles, such as have been observed by Blackett and Occhialini [1933], hence the result of a nuclear explosion. (Kunze, 1933, p. 10)

This first observation was not particularly noticed by the contemporaries, but in the following three years, the experimental study of cosmic-ray phenomena pro-

[951] See Kawabe, 1991b, for the contents of this letter and its fate.

[952] Julius Paul Kunze was born on 2 November 1897, in Chemnitz. In 1928, he became *Privatdozent*; in 1933, extraordinary; and in 1936, ordinary professor of physics at the University of Rostock. In the 1960s, he moved to the *Technische Hochschule* in Dresden as Director of the Institute for Experimental Nuclear Physics. He died on 6 October 1986, in Dresden.

gressed enormously, especially in England (where Patrick Blackett worked at Birkbeck College, London), France (where Pierre Auger and Louis Leprince-Ringuet established a group in Paris), and the United States.[953] Carl Anderson and Seth Neddermeyer, who had collaborated since 1932 in Pasadena, used a counter-controlled cloud chamber with a magnetic field of 7,900 gauss in 1935 to observe cosmic rays, both at the summit of Pike's Peak, Colorado, and down in Pasadena, California, giving a detailed report in a comprehensive paper published in August 1936.

Anderson and Neddermeyer devoted special attention to analyzing a large number of their photographed pictures, clarifying the energy determination and discussing the peculiar features and anomalies of the tracks. In Fig. 12, they noted in the caption:

> If the observed curvature were produced entirely by magnetic deflection, it would be necessary to conclude that this track represents a massive particle with an $\frac{e}{m}$ much greater than that of a proton or any other known nucleus. (Anderson and Neddermeyer, 1936, p. 270)

Still, because of experimental reasons, they were hesitant to state clearly the consequence that the tracks belonged to an 'unknown particle' (as had been expressed by Kunze), and rather 'tentatively interpreted [the particle] as a proton' (Anderson and Neddermeyer, *loc. cit.*). However, Watson Davis reported on a colloquium in Pasadena:

> Discovery of an unknown particle that may prove to be as important as the positron was made known by Dr. Carl Anderson and his colleagues at the California Institute of Technology just a short time after he was notified of his sharing the Nobel physics prize for his discovery of the positron. (Watson Davis, in *Science Service*, 13 November 1936)

Although Anderson himself, in his Nobel lecture of 12 December 1936, only weakly hinted at 'highly penetrating particles,' which were not 'free positive or negative electrons' and, hence, 'will provide interesting material for future study,' he became quite explicit finally in a paper submitted to the *Physical Review* in March 1937 (Neddermeyer and Anderson, 1937).[954] There, Neddermeyer and Anderson concluded strongly from their new observations:

> The present data appear to constitute the first experimental evidence for the existence of both penetrating and nonpenetrating character in the energy range extending

[953] For a report on the early activities in London, see Blackett (1937) and J. G. Wilson (1985); in Paris, see Auger, 1985, and Leprince-Ringuet, 1983; in the USA, see C. D. Anderson and H. L. Anderson, 1983.

[954] Anderson, when recalling his Nobel lecture, stated that he 'received no reaction' in Stockholm and afterward on his announcement (C. D. Anderson and H. L. Anderson, 1983, p. 147), but this need not surprise anybody because his presentation had been rather cautious.

> below 500 MeV. Moreover, the penetrating particles in this range do not ionize perceptibly more than nonpenetrating ones, and cannot therefore be assumed to be of protonic mass. (Neddermeyer and Anderson, *loc. cit.*, p. 886)

Their findings were soon confirmed by another American group, consisting of Jabez Street and E. C. Stevenson of Harvard University (Street and Stevenson, 1937a, b); later on in summer the Tokyo team of Yoshio Nishina, Masa Takeuchi, and Tarao Ichimiya (1937) joined together, and first reported the values for the mass of the new particle.[955]

As we have mentioned above, already the first vague indication in the *Physical Review* of new particles with $\frac{e}{m}$ much greater than that of a proton in the report of Anderson and Neddermeyer (1936) had excited Hideki Yukawa. In his talk of 28 November 1936, 'he made numerical estimates to determine whether cloud chamber tracks obtained by Anderson and Neddermeyer might be those of heavy [U-] quanta,' and he 'showed that the upward particle track was consistent with a U-particle of mass 200 m_e, and he suspected that the four downward particles could also be U-particles' (Hayakawa, 1983, p. 89). Encouraged by this result, Yukawa returned to investigate further the 'interaction between elementary particles,' and also wrote the aforementioned letter to *Nature*. Upon its rejection, he waited for half a year, in which the necessary empirical evidence was provided and then submitted another letter, entitled 'On a Possible Interpretation of the Penetrating Component of the Cosmic Ray,' this time to the Japanese journal (Yukawa, 1937).[956] In spite of the unfavourable reaction of Western journals, Yukawa's theory meanwhile received the first recognition by colleagues from the Western physics community. Notably, in the *Physical Review* issues of 15 June and 1 July, the Americans J. Robert Oppenheimer and Robert Serber (1937) and the Swiss Ernst C. G. Stueckelberg (1937b) drew attention to Yukawa's theory of nuclear forces, although they arrived at opposite conclusions concerning its value: Thus, Oppenheimer and Serber denied any connection with the new cosmicray particle, while Stueckelberg claimed that Yukawa had predicted it.[957] In any case, the international community of quantum physicists began to take notice of the work done in Japan after a delay of more than two years. It soon would become *the* standard theory of nuclear forces, as we shall see in Section IV.5 below.

[955] For details of the discovery of the new penetrating cosmic-ray particle, see Galison, 1983, Takeuchi, 1985, and Rechenberg and Brown, 1990.

[956] It should be mentioned that a later attempt by Yukawa to publish a letter, dated 4 October 1937, in the *Physical Review* failed again. The letter was entitled 'On the Theory of the New Particle in Cosmic Ray' and signed by Yukawa, Sakata, and Taketani; the report from *Physical Review*, dated 2 December and signed by the assistant editor J. W. Buchta, declared that the theory was not acceptable (see Kawabe, 1991b, pp. 186–190).

[957] For details of the first recognition of Yukawa's theory, see Rechenberg and Brown, 1990, especially, pp. 233–242.

IV.4 Solid-State, Low-Temperature, and Relativistic High-Density Physics (1930–1941)

(a) Introduction

When Friedrich Hund joined Werner Heisenberg at the University of Leipzig as the second professor of theoretical physics, the topic of his research deviated somewhat from that of his more famous colleague. As he remarked later:

> After [treating] atoms and molecules, for me now of course [the topic of] electrons in solids was due. I would have liked to form a circle of collaborators at that time to deal with that kind of solid state physics. I was still quite young then, but we really had too few students. At that time theoretical physics was in disrepute (*verleumdet*), and almost nobody studied theoretical physics. (Hund, in Rechenberg, 1994, p. 103)

What Hund outlined with these words was the situation in German science, which had developed soon after the Nazis came to power and chased away a considerable fraction of the professors and students from the universities and research institutions, primarily because of their Jewish descent. The partisans of the new government even went as far as defaming a little later—in the mid-1930s—the modern relativity and quantum theories as 'Jewish physics,' and personally attacking the remaining outstanding physicists, especially Werner Heisenberg, and denying them positions appropriate to their stature.[958] No wonder that the young, talented, and ambitious students—unlike during prior decades—avoided devoting themselves to research in theoretical physics; certainly, their numbers dropped drastically as compared to the years immediately preceding 1933. Still, the atomic physicists, who still remained in their positions in Germany, tried to pursue research in their field as well as they could.

With respect to his programme, Hund had indeed directed his attention already as early as fall 1931 to consider again—for the first time after leaving the subject in 1925 in favour of atomic and later molecular physics—the problems of solid-state theory. He noted in his diary (*Wissenschaftliches Tagebuch* or *Tagebuch*):

> In a one-dimensional chain of atoms with $q = 1$ to p valence electrons, one can show the occurrence of metallic binding $\sim q(p-q)/p$. An improvement by screening and estimate of terms due to Bloch seems to be possible. (Hund, *Wissenschaftliches Tagebuch*, 22 September 1931)

The question, which bothered him at that time, concerned the proper understanding of the existence of metals and insulators on the basis of the detailed constitution of atoms and their binding in solids. While his predecessors and the

[958] See, e.g., Beyerchen, 1977, and Rechenberg, 1992.

pioneers of the quantum-mechanical description of solids, such as Felix Bloch, Rudolf Peierls, and others, had been interested primarily in the principal phenomena exhibited by the atomic lattices, Friedrich Hund wanted to investigate the influence of atomic shapes—their particular valences—and the structure of the lattices on the macroscopic properties, like electrical conduction, etc. In December 1931, he submitted a study to *Zeitschrift für Physik*, entitled '*Zur Theorie der schwerflüchtigen Atomgitter* (On the Theory of Non-Volatile Nonconducting Atomic Lattices)' and dealing with the so-called valence-bond lattices, such as diamond (Hund, 1932b). Hund showed in his paper—by visualizing the valence-bonds—how in the latter case, the carbon atoms formed a firmly bound lattice, in which the number of valence-electrons equalled the number of closest neighbours; hence, the substance not only turned out to be nonvolatile, but also nonconducting. In the case of graphite, on the other hand, the different structure of the lattice planes would cause metallic conductivity.

In summer 1932, Hund continued to ponder at Leipzig about solid-state theory; he gave a talk on the term-structure of solids in Marburg (see his *Tagebuch* entry of 18 June); he examined, for instance, the relation between the magnetization of bodies and their term-structure (*Tagebuch* entries of 24 June and later). During the following winter semester, Hund even devoted his lecture course to the subject of solid-state theory and discussed in his diary (*Tagebuch*) such items as superconductivity and ferromagnetism. Hund proposed certain ideas about the origin of superconductivity (in November 1932), based on the following assumption: 'A state, which can be described according to Bloch and Heitler-London by *s-electrons*, lies a bit higher than the "*d*-state" and possesses higher term density. For the *s*-state, Bloch's theory is valid; for the *d*-state, the latter serves as an approximation. There are two phases.' (*Tagebuch* entry dated 9 November). But the difficulties remained as to how to explain the very high values of conductivity and the existence of a transition temperature (entry of 29 November). During the first half of the year 1933, the political turbulence in Germany and other distracting obligations prevented him from undertaking further serious investigations, but in the fall of that year, Hund resumed his work. Again, the theory of conductivity, especially superconductivity, attracted him most—besides other properties of solids such as diamagnetism and ferromagnetism. For the International Conference on Physics in London in October 1934, he prepared a lecture dealing with the 'Description of the Binding Forces in Molecules and Crystal Lattices on Quantum Theory' (Hund, 1935a). In two notes in his *Tagebuch*, dated 1 and 3 September 1934, Hund referred to new work on the subject by Eugene Wigner and Frederick Seitz and John Slater in the United States. From early 1935 on, he then started a series of publications on the theory of crystals, notably, on the electrostatic energy in ionic lattices (Hund, 1935b), on the electron terms in a crystal lattice (Hund and Mrowka, 1935a, b), and on the motion of electrons in nonmetallic lattices (Hund, 1935c), and finished in January 1936 with a study revealing the relation between crystal symmetry and the electronic states in crystals (Hund, 1936a). We shall refer to these papers below.

From early 1936 to October of that year, suddenly there appeared another topic in the research programme of Friedrich Hund, which he denoted in his *Tagebuch* entry of 22 January as '*Materie in extremem Zuständen* (Matter under Extreme Conditions),' adding the comment that matter then consisted essentially of an ideal gas of electrons, to which he could apply—as he wrote a few weeks later—certain ideas that John Slater and Harry M. Krutter had devised in solid-state theory (see below). Hund especially proposed to apply his ideas to the theory of the stars, which he discussed in detail in spring and summer 1936, including the 'Chandrasekhar catastrophe' and the transition of normal stars into neutron stars if superhigh pressures act. From 26 July to 7 August 1936, he composed a review article on the whole topic for *Ergebnisse der exakten Naturwissenschaften* (Hund, 1936b). He returned to the theory of stars later in 1937, after having been concerned in the meantime (from fall 1936 to fall 1937) with the problems of nuclear constitution—see Section IV.5 below—and again with some solid-state problems (since fall 1937).

Hund's diverse research programme in the 1930's covered much of the domain to which the present section is devoted, i.e., solid-state theory, theory of low-temperature phenomena (implying, in particular, the proof of quantum degeneracy, as proposed by Albert Einstein in his theory of ideal gases in 1924), and finally, the high-density situation occurring in relativistic astrophysics. Although Hund contributed to all of these topics—except that he did not publish his ideas on superconductivity—he was no longer, as in former times, the leading theorist in any of these. One could rightly guess that he might have had a more fruitful and productive time, had he had the occasion to work and communicate in a productive scientific community such as the one that existed in Germany before 1933—consisting of, for example, Hans Bethe, Max Born, Paul Ewald, Lothar Nordheim, Rudolf Peierls, and Eugene Wigner. But all of these people had been driven out by the Nazis, and had in some cases left the *Third Reich* voluntarily and now assisted old and new centres abroad, notably, in Great Britain and the United States, in establishing preeminence in physical theory; it was not in Germany, but in England and the United States that modern solid-state theory was mainly promoted after 1934. And yet, he could contribute quite a bit.

The situation in low-temperature physics seemed to be a little different. The pioneering experimental investigations had been started by Heike Kamerlingh Onnes in Leyden, Holland, and followed in the 1920's in laboratories in Canada and at the *Physikalisch-Technische Reichsanstalt* in Berlin. In the early 1930s, then, the Royal Society's Mond Laboratory was established in Cambridge, England, and also in neighbouring Oxford's Clarendon Laboratory refugees from Germany, such as Franz (later Sir Francis) Simon and Kurt Mendelssohn, helped to create a new tradition in low-temperature physics. In the East, the Soviet Union entered into serious research in low-temperature physics by installing laboratories in Kharkov and, after 1934, in Moscow (detaining Peter Kapitza, who was visiting home for holidays from Cambridge, England, and purchasing his important apparatus from the Mond Laboratory). While the *Reichsanstalt* suffered from the takeover by Johannes Stark, who had become hostile to all modern theory, and with the departure of Walther Meissner, the authorities of the *Kaiser Wilhelm-Gesellschaft* hurried to install suitable low-temperature apparatus for Peter Debye's new *Kaiser Wilhelm-Institut für Physik* in Berlin-Dahlem (which, however, started operations only in 1938). The fruitful discoveries made at the above-mentioned laboratories and institutes in the 1930s, were thoroughly analyzed with the help of quantum mechanics. Still, the problem of superconductivity resisted all ingenious attempts to come to grips with it. However, the phenomenon of superfluidity, which had been discovered later toward the end of the 1930s and in the early 1940s, found a satisfactory explanation (actually, rather, two explanations).

In the problems that Friedrich Hund had addressed as 'matter under extreme conditions,' especially the distinguished Cambridge astrophysicist Arthur Stanley Eddington and the young Indian scholar Subrahmanyan Chandrasekhar ('Chandra') acted as pioneers. During the years following Chandrasekhar's work, they became engaged in a sharp and bitter controversy about this ingenious application of relativistic quantum theory, especially the electron theory, to describe the degenerate matter in stars. In this dispute, Chandrasekhar received the support of quantum theoreticians, including Paul Dirac, Rudolf Peierls, and Léon Rosenfeld. Further contributions to high-density astrophysics came from Lev Landau in Kharkov and Robert Oppenheimer and his students in California, which will also be reviewed below. Thus, we will show how some of the main concepts of modern astrophysics, such as that of neutron stars, evolved from a proper extension of quantum mechanics already in the 1930s.

(b) New American and European Schools of Solid-State Physics (1933–1937)

Before John Clarke Slater moved from Harvard University to accept the professorship of physics and chairmanship of the physics department at the neighbouring MIT in fall 1930, he had been asked by John Tate, at that time, editor of *The Physical Review* and *Reviews of Modern Physics*, if he 'could write a review article on the electron structure of metals for the *Reviews of Modern Physics*' (Slater, 1975, p. 192). Owing to heavy obligations connected with the change of position and other professional interests, Slater did not find time to write the required article until summer 1934. However, when he did write it, it became a rather comprehensive essay of 71 pages covering the known material in 31 sections plus seven appendices. Slater's review, entitled 'The Electronic Structure of Metals,' constituted the first account published in the United States of the extensive literature which had appeared especially since the new electron (Fermi–Dirac) statistics in 1926 (Slater, 1934c). The literature cited at the end of the article included 118 references up to summer 1934, organized according to the year of publication (since Hendrik Lorentz's book on *The Theory of Electron* in 1906), exhibited quite an interesting feature, namely, an abrupt transition in the institutions of the authors between 1932 and 1934: Their positions had shifted from

Central Europe to the West, in the first place from Germany to England and—to a lesser extent—to America. In his scientific autobiography, John Slater went so far as to claim that the initiative in quantum physics had passed after 1933 from Europe to America (see Slater, 1975, p. 163). An example may be seen by the fact that, while William G. Penney and Robert Schlapp came in 1932 from Great Britain to assist John H. Van Vleck in Madison, Wisconsin—to investigate in some detail the splitting of levels in crystals containing three-dimensional or four-dimensional transition elements—in the year 1933, 'a turning point in the development of the theory of solids' set in, especially:

> The first step came from Princeton: the first of several papers by [Eugene] Wigner and Frederick Seitz, his student, on the constitution of metallic sodium.... These papers were the first ones which broke definitely out of the pattern of LCAO [i.e., the linear combination of atomic orbitals] *versus* plane waves which had been the direction in which solid state theory had been traveling. They represented the first attempt at applying something very much like the self-consistent field to a crystal. (Slater, *loc. cit.*, p. 173)

Eugene Wigner, who, in 1930, had obtained a half-time position as a visiting professor at Princeton University, which was turned in 1933 into a full-time professorship, began to train new students and collaborate with them. Frederick Seitz was the first student to arrive from Stanford, California, to take his doctorate with Wigner.[959] In March 1933 and June 1934, Wigner and Seitz submitted two papers, entitled 'On the Constitution of Metallic Sodium,' to *Physical Review* (Wigner and Seitz, 1933, 1934). Together with an intermediate article of John Slater on 'Electronic Energy Bands in Metals' (Slater, 1934b) and Wigner's comprehensive article 'On the Interaction of Electrons in Metals' of October 1934 (Wigner, 1934), these investigations indeed opened a new American era in solid-state physics. 'The problem [addressed by Wigner and Seitz] was formidable,' wrote Walter Kohn more than 60 years later, and continued:

> As Wigner knew well from his acquaintance with the quantum theory of small atoms and molecules, any serious estimates of the energy as function of the nuclear positions—which is required for the calculation of the lattice parameters, cohesive energies and elastic constants—had to go beyond mean field theories (Sommerfeld, Hartree, Hartree-Fock) and include the effects of dynamical correlations. The Rayleigh-Ritz variational method had resulted in extremely accurate calculations for

[959] Frederick Seitz was born on 4 July 1911, in San Francisco, California, and began to study at Leland Stanford, Jr. University (A.B. in 1932). He obtained his Ph.D. from Princeton in 1934, and then became an instructor in physics (1935–1936) and assistant professor (1936–1937) at the University of Rochester. After two years as a research physicist with the General Electric Company, he became an assistant professor at the University of Pennsylvania (1939–1941), and then an associate (1941–1942) and full professor of physics and head of the physics department at the Carnegie Institution of Technology (1942–1949); he then moved to the University of Illinois (where he stayed until 1965). In 1965, Seitz became full-time President of the National Academy of Sciences in Washington, D.C., and, afterward, President of the Rockefeller University in New York City.

> the He-atom (Hylleraas) and the hydrogen molecule (James and Coolidge), but the effort increased very rapidly with the number of atoms involved, and the method was totally inapplicable to the case of an "infinite" metal. (Kohn, 1997, pp. 357–358)

The method of Wigner and Seitz consisted in 'an intermediate point of view by applying the free electron picture but aiming at a calculation of chemical properties of metallic sodium such as lattice constant, heat of vaporization, compressibility, etc.' (Wigner and Seitz, 1933, p. 804) It had been used first by Friedrich Hund originally in the theory of molecules (Hund, 1927a), later by Hund again to investigate 'little volatile, non-conducting atomic lattices,' such as diamond (Hund, 1932b). In the latter paper, Hund had approached 'the eigenfunctions of the crystal by the eigenfunctions of the single electrons in a substitute field corresponding to the crystal,' and further noticed: 'The eigenfunctions of the single electrons can be approximated again by eigenfunctions in the central fields of separated atoms. We obtain, for any choice of quantum numbers of the separated atoms a huge number of states of the single electrons of the crystal.' (Hund, *loc. cit.*, p. 5) He had thus succeeded in explaining qualitatively the existence of insulators (all groups consisting of ground states of separated atoms are occupied by two electrons, and they exhibit a finite distance from those of the excited atom) or metals (in which case, the ground states of the single electrons form a continuum). Wilhelm Lenz and Hans Jensen in Hamburg and J. E. Lennard-Jones in Bristol had also applied Hund's procedure to two-dimensional metallic lattices, before Wigner and Seitz took it up; Kohn characterized their specific approach as follows:

> They pointed out that in a metal each electron is surrounded by a neutralizing hole of total charge $\pm e$ in the charge distribution of the other electrons. They calculated this hole in the Hartree-Fock (HF) approximation for the case of a uniform electron gas.... They noted, however, that in the HF-approximation the hole was due to *statistical* correlations of electrons of parallel spin and that the important *dynamical* correlations due to the electron-electron repulsion, which affected electrons of both parallel and antiparallel spin were ignored. In the event they adopted the heuristic viewpoint that any electron when located in a particular atomic cell, while the ions of the other cells were perfectly screened by the charges of the other electrons. (Kohn, 1997, p. 358)

Wigner and Seitz practically introduced an effective potential $V(r)$ for the valence-electron of atomic sodium, depending on the cell number k, and the space point r lying within the cell volume surrounding the ion in k. Now, the solution of the Schrödinger equation appeared to be straightforward, as Wigner and Seitz remarked:

> It will not be necessary to solve it for the entire lattice, because it will have the same symmetry as the crystal and hence will merely repeat itself a great number of times. Because of this symmetry, the derivative of the wave function at every crystallographic symmetry plane will be zero perpendicular to this plane. This will be used as a boundary condition. (Wigner and Seitz, 1933, p. 805)

Since the sodium crystal treated had a body-centred cubic structure, with one atom at the centre and at each corner of the cubic lattice, they constructed from it a nearly spherical 'truncated octahedron' surrounding each atom. This cell, later called 'the Wigner cell,' is according to Slater's description 'in fact equivalent in ordinary space of the Brillouin zone in reciprocal space, and it can be proved that the Wigner-Seitz cell for the body-centered cubical structure is identical in shape with the Brillouin zone for the face-centered cubic structure, and *vice versa*.' (Slater, 1975, p. 173)[960] On carrying out the calculation for the lowest energy wave function of the 3*s*-band in metallic sodium, which differed from the atomic 3*s*-function just by the boundary condition referred to above, and taking into account the exclusion principle—i.e., by adding to the band energy the mean Fermi energy of a uniform gas of density $\left(\frac{4\pi}{3}r_s\right)^{-1}$ and minimizing the sum with respect to r_s—Wigner and Seitz obtained the total energy per ion at absolute-zero temperature, and from there, they derived the corresponding properties, such as the lattice parameter d, the cohesion-energy parameter λ (i.e., the energy difference between gaseous and solid state in Rydberg units), and the compressibility κ, respectively,

$$d = 4.2\,\text{Å}, \quad \lambda = 25.6\ k\ \text{cal/mol}, \quad \kappa = 1.6 \times 10^{-11}\ \text{c.g.s. units.} \tag{713}$$

These values, they claimed, 'compare favorably' to the experimental data, immediately adding: 'partly, without doubt, as a consequence of compensating errors' (Wigner and Seitz, 1933, p. 810).

In a second, more extended, paper submitted six months later, Wigner and Seitz examined certain aspects of their theory in great detail. In Part I, they began by taking a closer look at the effective potential, which consisted of 'first, the potential arising from the ion at the center of the 2*s*-sphere [i.e., the idealized polyhedron of the previous paper], second, the potential arising from other free electrons' (Wigner and Seitz, 1934, p. 509); then they introduced certain modifications before dealing at length with the question of the Fermi energy (i.e., the 'zero-point energy' of the Fermi gas in the crystal considered) in Part II. Walter Kohn emphasized in his commentary that 'the main contribution of this paper is the first serious attack on the problem of *correlation energy*, the change of the total energy due to electric correlations resulting from their mutual repulsion,' and added:

> The authors realized that this energy was due mostly to the fact that the electrons of anti-parallel spin would be kept apart, since, even without repulsion, those with parallel spin are kept apart by the Pauli exclusion principle. They made the inspired *Ansatz* [i.e., $\psi(x_1, \ldots x_n; y_1, \ldots y_n) =$

$$\frac{1}{n!}\begin{vmatrix} \psi_1(y_1, \ldots y_n; x_1) \ldots \psi_1(y_1, \ldots y_n; x_n) \\ \vdots \\ \psi_n(y_1, \ldots y_n; x_n) \ldots \psi_n(y_1, \ldots y_n; x_n) \end{vmatrix} \cdot \begin{vmatrix} \psi_1(y_1) \ldots \psi_1(y_n) \\ \vdots \\ \psi_n(y_1) \ldots \psi_n(y_n) \end{vmatrix} \qquad [(714)]$$

[960] For details, we refer, for instance, to Slater, 1975, Chapter 22, pp. 173–184.

> for a system with n electrons, and x_k denoting the three Cartesian coordinates of the k-th electron having spin upward and y_k the corresponding coordinates of the k-th electron having spin downward] for the many-electron wave function, notwithstanding its violation of strict anti-symmetry. Further inspiration was needed to deal with the functions $\psi_v(y_1, \ldots y_n; x_1)$ occurring in this *Ansatz*. So the authors assumed that, in dealing with the spin-up electrons of coordinates x_1, an approximate "mean configuration" for the spin-down electrons y_n was a closed-packed lattice occupied by pairs of the latter! With this brilliantly outrageous assumption, the correlation energy for a uniform electron gas was approximately calculated as a function of r_s. When this was added to the appropriate Hartree energy and exchange energy, and minimized with respect to r_s, a lattice parameter of 4.75 Å and a cohesive energy of 26.9 kcal were obtained. (Kohn, 1997, p. 359)

Wigner and Seitz noted, of course, in their new paper, the discrepancy between these theoretical values and the observed ones (4.23 Å and 23.2 kcal), and commented that 'it is hardly necessary to mention that the calculation of the last section must be regarded only as an attempt to find the correct wave function for the electrons in the metal, and we are well aware that we could guess its form roughly.' Especially, they argued that the discrepancy of 3.7 kcal for the heat of cohesion arose from two sources: On the one hand, the so-called 'Prokofjew field' used did not describe the situation completely; on the other hand, 'the actual wave function is not represented to a sufficient degree by a wave function of the form of [(714)]' (Wigner and Seitz, 1934, p. 522).

In a subsequent work on lithium, Frederick Seitz investigated the first point more closely, and especially:

> The previous work [by Wigner and Seitz] was divided into two parts, namely, the solving of the best one-electron approximation [involving the Prokofjew field mentioned above], on the one hand, and the investigation of more general statistical correlations of electron-positions [in the original text this has been misprinted as "positrons," (*sic*)] than those afforded by the first part, on the other. In the case of Na, the first part yielded about one-fifth of the observed binding energy while the second, for which the most satisfactory treatment has been given by E. P. Wigner in a very recent paper, removes about 80 percent of the remaining discrepancy. In the case of Li, it is found that the one-electron picture is appreciably changed, the individual wave functions being less similar to free-electron wave functions than in the case of Na. This has as its consequence that almost half of the observed energy is included in the one-electron solution. At the present stage of calculation, the result of Wigner on the nature of additional correlations is taken over directly and yields a binding energy of 34 kg Cal. as compared with the observed 38.9. (Seitz, 1935a, p. 334)

While Seitz submitted a full account of the results (which we have quoted above from the abstract of a talk he presented at the Pittsburgh meeting of the American Physical Society in December 1934), in a paper submitted to the *Physical Review*, where it was published in the issue of 1 May 1935 (Seitz, 1935b), Wigner had already addressed the scientific public earlier in a general essay, bearing the title 'On the Interaction of Electrons in Metals,' which appeared in the *Physical Review* issue of the previous December (Wigner, 1934). There, Wigner still retained Eq. [(714)] to derive the 'correlation energy'—though he admitted that 'it is certainly not the correct one' (Wigner, *loc. cit.*, p. 1003)—but he suggested a different approximation method, 'which is essentially a development of the energy by means of the Rayleigh-Schrödinger perturbation theory in a power series of e^2' (Wigner, *loc. cit.*, p. 1002). In particular, he assumed for the functions $\psi_v(y_1, \dots y_n; x_k)$, occurring in Eq. [(714)], the *Ansatz*

$$\psi_v(y_1, \dots y_n; x_k) \\ = \psi_v(x_k)\{1 + f_v(y_1 - x_k) + f_v(y_2 - x_k) + \cdots f_v(y_n - x_k)\}, \qquad (715)$$

where ψ_v denoted a plane wave and the functions f_v were expected to be small, short range, and negative, describing the effect of repulsion. Previously, Wigner and Seitz had assumed the y_k to constitute a closed-packed lattice of electron pairs, but now Wigner took the $y_1, \dots y_n$ to be Slater determinants of plane waves; also, he calculated the f_v's in second-order perturbation theory. In order to check the accuracy of the new approximation method, Wigner compared the correlation energy of small atoms with the low-density limit of the metallic cohesion energy obtained according to the above procedure. In the limit $r_s \to \infty$ (with r_s, the radius of a hole surrounding every electron in the metal) when the kinetic energy of the electrons ($\sim r_s^{-2}$) was small against the potential energy ($\sim r_s^{-1}$), the electron would occupy the points of the closed-packed lattice (later named 'Wigner lattice') and yield a correlation energy of $0.292\, e^2/r_s$ per electron. From this limiting value, now the values for a more realistic description of the metal could be derived: Wigner especially estimated in the case of sodium the characteristic quantities to be $d = 4.62$ Å and $\lambda = 26.1$ kcal, as compared to 4.75 Å and 23.2 kcal in the second Wigner–Seitz paper, and 4.2 Å and 25.6 kcal in the first Wigner–Seitz paper.[961]

In between the first and the second Wigner–Seitz publications, there appeared, as we mentioned above, a paper by John Slater on the 'Electronic Energy Bands in Metals' (Slater, 1934b). In it, Slater referred to the work of Wigner and Seitz but proposed a different approach to the same problem, namely, 'instead of using simply one *s* wave function, as Wigner and Seitz do, a combination of eight separate functions is used, one *s*, three *p*, three *d* and one *f*'' (see the abstract of the paper, Slater, 1934a, p. 766). That is, he wanted to adapt the Wigner–Seitz ideas to the more realistic situation existing in metals. Thus, he proceeded in the following way:

> Boundary conditions for an arbitrary electron momentum are satisfied at the mid-points of the lines connecting an atom with its eight nearest neighbors. Energy levels

[961] As Walter Kohn noted, the modern theoretical evaluations produce instead 4.07 Å and 25.8 kcal, while the best experimental values are 4.22 Å and 26.1 kcal; thus, the old theoretical results approximated the data already pretty well. (See Kohn, 1997, p. 360)

> and wave functions are determined as functions of internuclear distance, leading to the following quantitative results: At the observed distance of separation, energy levels are given with remarkable accuracy by the Fermi-Sommerfeld theory, the gaps fall approximately where they should as computed from de Broglie waves, and the wave functions act accurately like plane waves in the region between atoms, but fluctuate violently, like $s, p, \ldots$ functions, near the nuclei. Gaps in energy are precisely filled up, though in each definite direction of propagation there are gaps. As the internuclear distance increases, gaps in energy appear at definite points, the allowed region shrinking to zero breadth about the atomic levels at infinite separation. (Slater, 1934a, pp. 766–767)

Slater concluded his detailed paper (1934b) by stressing the fact that the above results 'both depend on the possibility of actually solving the wave equation for an electron in a periodic field without important approximations'; he hardly expected 'them to follow with anything like some certainty from a perturbation method which would be inaccurate at the actual internuclear separation' (Slater, *loc. cit.*, p. 801). Therefore, he would 'never be able to accept' the Wigner–Seitz treatment of the correlation energy, because 'they based their discussion on a uniformly distributed positive charge and a homogeneous electron gas, whereas it is obvious that at large interatomic distances we must have the formation of individual atoms behaving as they do when isolated from each other' (Slater, 1975, p. 184).

In fact, John Slater deeply concentrated on realistic energy-band calculations, and to help with this problem, he got—for the first time in his career—graduate students involved. 'Up to that time, I had never had graduate students working with me,' he recalled decades later, and added:

> One of the first who went in for it was H[arry] M. Krutter, who worked out the energy bands of the copper crystal in 1935. I had been particularly interested in getting energy bands for the 3*d* transition elements, to see if my hypothesis ... that the 3*d* bands were narrow enough to show ferromagnetism in iron, cobalt, and nickel was actually justified. I felt that copper, the first element beyond these, and yet with a single valence electron like sodium, would be good enough to start with it. (Slater, *loc. cit.*, p. 185)

Krutter was indeed ready in 1935 to publish two papers about his investigations. He composed the first one with Slater, and it treated 'The Thomas-Fermi Method for Metals' (Slater and Krutter, 1935); i.e., they extended a well-known method that had been used earlier in the problems of atomic physics. The authors noted that that method 'rests on the same fact which makes possible the Wigner-Seitz calculation,' namely, 'the potential acting on the electron in the neighborhood of one of the nuclei of the metal is nearly spherically symmetrical, the nucleus being the center, so that the same method of solving differential equations, as for example the Thomas-Fermi method or the Schrödinger equation, which is applicable in an isolated atom, can be used in the metal, simply by using different boundary conditions' (Slater and Krutter, *loc. cit.*, p. 559). In their calculations of the potential field, the charge density and the kinetic, potential, and total energies, Krutter and Slater verified the virial theorem for the energy, which Wilhelm Lenz (1932) and his student Hans Jensen (1932) had proved a few years earlier to hold for the Thomas–Fermi–Dirac method—i.e., the Thomas–Fermi method, including Dirac's treatment (see Section III.4)—and also used in solid-state theory.[962] Though the results obtained did not satisfy Krutter and Slater with respect to the evaluated energy of metal electrons in the neighbourhood of equilibrium, they still concluded that 'the potential field, momentum distribution and various other features promised to be of decided value as first approximations in more accurate treatments of metals' (Slater and Krutter, 1935, p. 568).

In his paper on 'Energy Bands in Copper,' submitted to the *Physical Review* in July 1935, Krutter then extended the methods used previously by Slater to describe the higher energy states and wave functions of metal electrons in body-centred lattices to the face-centred lattices of copper (Krutter, 1935b).[963] Here, he had to solve the Schrödinger equation within a particular cell, which required the continuity of the wave function and its normal derivatives at the midpoints of the faces of the cell and taking into account the Bloch condition—this procedure implied fitting the boundary conditions at 12 points, all at the same distance from the nucleus. Krutter succeeded in performing this laborious task in a satisfactory approximation and to demonstrate the strong overlap of the 3*d* band and the 4*s* band, as Slater had imagined earlier. In particular, he concluded: 'The assignment of electrons to the various energy bands leads to the result that, theoretically, copper is a good conductor, a well-known fact.' (Krutter, 1935b, p. 671) For more quantitative results, he finally argued, the method of obtaining the potential field had to be improved, say, by solving the self-consistent Hartree problem for each metal individually.

At the end of his paper, Krutter thanked—besides John Slater—George E. Kimball 'for many helpful discussions' (Krutter, *loc. cit.*). Kimball, a Ph.D. in chemistry from Princeton University, spent the years 1933–1935 as a postdoctoral fellow at MIT. In an investigation on the electronic structure of diamond—the paper was received on 9 July 1935, by the *Journal of Physical Chemistry* (Kimball, 1935b)—he proposed to explain quantitatively the absence of electrical conductivity and other properties of that crystal, which had been studied more qualitatively earlier by Friedrich Hund (1932b): Kimball now used the Wigner–Seitz method, as extended by Slater, for that purpose (Kimball, 1935a, b). In the case of diamond, the corresponding Wigner–Seitz cells are formed by 'twelve planes bisecting the lines to the next nearest neighbors cut off the corners of [a regular] tetrahedron, leaving a 16-sided-solid,' and 'the cells surrounding the two atoms of the unit cell of the crystal are identical, but are oppositely oriented' (Kimball,

[962] John Slater himself had demonstrated the validity of the virial theorem for the case of molecules by 'assuming that external forces are applied to keep the nuclei fixed' (Slater, 1933, p. 687). For more details, see Slater, 1975, Chapter 24.

[963] Harry Krutter presented an outline of his work at the Washington meeting of the American Physical Society in March 1935 (Krutter, 1935a).

1935b, p. 560). Since Kimball found the task of joining the eigenfunctions of all 16 faces of these cells too complicated, he 'therefore decided to fit the eigenfunction so that the value and normal derivative would be continuous at the four points midway between the atom and its nearest neighbors, that is, at the centers of the hexagonal faces of the cell' (Kimball, *loc. cit.*). He solved the tricky problem of determining eight eigenfunctions in the second approximation, and computed the energy band of diamond as a function of the internuclear distances; thus he obtained four extended bands and four bands of zero width (Kimball, *loc. cit.*, p. 563, Figs. 2 and 3). 'Although not very much of a quantitative nature can be concluded from these results, the essential differences between diamond and the metals are apparent,' he finally stated and added:

> In the diamond the low energy bands are all completely filled, and a large amount of energy would be necessary to promote an electron to an unfilled band. Now in each band, for every electron wave traveling in one direction there is a second wave of the same energy traveling in the opposite direction. The net result is that a filled band can produce no flow of charge. Hence it follows that diamond is a non-conductor. (Kimball, *loc. cit.*, p. 564)

In the United States, work on the theory of metals consisted mainly in pushing further the principles of the subject, which had been discovered earlier in Europe, and solving more realistic problems. On the other hand, a number of solid-state physicists on the old continent did not remain idle. Thus, for example, Léon Brillouin in Paris investigated (between 1932 and 1934) especially the magnetic properties (e.g., Brillouin, 1932) and the ionization potential of metals (Brillouin, 1934). In spring 1934, Friedrich Hund—who had treated the problem of electrostatic energies of ionic crystals before 1925 and now learned, like John Slater and others, how to handle the modern approximation methods (see Hund, 1932d)—turned again to the theory of crystals, initially concerning himself with their magnetic properties. On 13 June 1934, he noted in his *Tagebuch*: 'The model of "semimetals" with [a] one-dimensional chain, each atom having two electrons and s-, p_x-, p_y-states ... may explain the properties of Bi.' In this context, Hund referred to the experimental data of Peter Kapitza in England. Later that summer, Hund had to prepare his lecture for the London conference dealing with the interaction of electrons in the lattice (Hund, 1935a). Finally, in fall of the same year, he plunged into a detailed research programme on solid-state theory, which led to several original publications, the first of which was devoted to a semiclassical calculation of the electrostatic energies in certain ionic crystals, such as β-christobalite or cuprite, based on the empirical structure of these, in general, complex substances (Hund, 1935b). Simultaneously, he turned to the new quantum-mechanical ideas of Wigner and Seitz and Slater, respectively, and on 2 September 1934, he noted in his *Tagebuch*: 'With Slater's method one might perhaps [be able to] calculate the diamond lattice.' It took some time until Hund had completed, together with his assistant Bernhard Mrowka, the extended memoir entitled '*Über die Zustände der Elektronen in einem Kristallgitter, insbesondere* *beim Diamant* (On the Electron States in a Crystal Lattice, Especially of Diamond),' which he presented at the meeting of *Sächsische Akademie der Wissenschaften* on 17 June 1935 (Hund and Mrowka, 1935a). Hund and Mrowka characterized their procedure as follows:

> By taking into account the periodicity and symmetry of a crystal lattice, the qualitative properties of energy bands for the single electrons can be derived. In the case of diamond, this qualitative consideration, the Bloch approximation and a numerical calculation along the lines of Slater supplement each other to provide a fairly quantitative picture of the term structure. (Hund and Mrowka, *loc. cit.*, p. 206)

At the 11th *Physikertagung* in Stuttgart, held in September 1935, Friedrich Hund presented the main results of the above paper in a condensed form (Hund and Mrowka, 1935b). Hund and Mrowka took as the basis of their treatment the Wigner–Seitz assumption that the potential field in the vicinity of the lattice point possesses a spherical shape and considered the *s*-, *p*-, *d*-, etc., solutions of the Schrödinger equation with that potential. Thus, they especially obtained the relations between the wavenumber vector **k**, the energy *E*, and the lattice constants. From the structure of the calculated terms, Hund and Mrowka derived a classification of lattices into four groups: the first consisted of linear equidistant atoms, two-dimensional lattices of the graphite type, the three-dimensional diamond lattice, and a few others; the second group contained linear chains of equal atoms and the cubic lattices of equal atoms; the third group embraced lattices of equal atoms having a more complex structure, and the fourth group had lattices of different atoms or exhibiting different distances. Hund gave another talk at the Stuttgart meeting on solid-state theory, dealing with electron motion in nonmetallic crystal lattices, in which he reviewed some recent results obtained in Germany concerning semiconducting crystals (Hund, 1935c). Finally, he described in a paper—submitted in January 1936—the conclusion that could be obtained theoretically from the relations between the crystal symmetry, on the one hand, and the electron states of solids, on the other (Hund, 1936a). He concluded there that 'the qualitative results derived on the term structure of electrons in a crystal lattice from the symmetry properties (of the spatial group) of the lattice are, *by and large*, *the same as appear in Brillouin's approximation* (apart from its quantitative aspect),' but he also warned that 'they are *not always exactly the same*' (Hund, *loc. cit.*, p. 135). However, these qualitative conclusions would often turn out to be quite vague if they were not supplemented by additional information or conditions. Hund especially emphasized that from symmetry considerations alone, there followed just the necessary conditions for crystals not to conduct electricity; these need not be sufficient, because two energy bands might easily overlap.

Unlike Germany—Hund and his collaborators practically did not publish further papers in the 1930s (and 1940s) on the quantum-mechanical theory of solids (but for one paper on superconductivity)—the work at the American centres around Eugene Wigner and John Slater flourished in the following years. Thus, by

1938, Wigner wrote six papers, either alone or in collaboration with John Bardeen, H. B. Huntington, L. P. Bouckaert, and Roman Smoluchowski.[964] Wigner's investigation with Bardeen, then a Fellow in Mathematics at Princeton University, on 'The Theory of Work Function of Monovalent Metals' (Wigner and Bardeen, 1935), and the following one by Bardeen alone (Bardeen, 1936), laid the foundation of the theory of the electronic structure of metallic surfaces. In this theory, the so-called work function ϕ was expressed as

$$\phi = eD - \mu, \tag{716}$$

with μ denoting the chemical potential and eD denoting the surface-dipole barrier, and then related to the cohesive energies of the earlier Wigner–Seitz calculations. In the paper of Wigner with Huntington, on the other hand, the question was explored whether hydrogen under high pressure might form metallic lattices; they especially drew attention to the existence of layer lattices, different from the usual Bravais lattices of sodium and other atoms having only one electron in the outer shell (Wigner and Huntington, 1935). The three-man paper on 'The Theory of Brillouin Zones and Symmetry Properties of Wave Functions in Crystals' (Bouckaert, Smoluchowski, and Wigner, 1936) went beyond Hund's earlier considerations (Hund, 1936a), because he had dealt only 'with those properties of the Brillouin zones which are common to all zones of the same lattice,' while Wigner and his collaborators considered 'the different zones separately' (Bouckaert, Smoluchowski, and Wigner, 1936, p. 58, footnote 1). Besides Hund and Wigner, Frederick Seitz also analyzed in particular the connections between the space groups of crystals and the wave functions of the Brillouin zones (see, e.g., Seitz, 1935c). Before his move to the University of Rochester, Seitz entered into a collaboration with the Princeton experimentalists R. Bowling Barnes and R. Robert Brattain to interpret the infrared absorption spectra of magnesium oxide by taking into account anharmonic terms in the potential function (Barnes, Brattain, and Seitz, 1935a, b). Afterward, Seitz turned—like Nevill F. Mott and Ronald Gurney in Bristol, England—to quantum-mechanical calculations of the electronic constitution of alkali-halogenides (Ewing and Seitz, 1936).

At this point, we may remark on the mutual relations between the groups of investigators working on solid-state theory in the United States, which were rather close indeed. In Princeton, Wigner collaborated with Seitz, who had come to the University in late 1931 to obtain his Ph.D. degree with Eduard Condon, but then became associated with Wigner. During these years, Seitz also maintained contact with William Shockley, a research student of Slater's at MIT, whom he had first met at a summer school at the California Institute of Technology.[965] The Princeton solid-state group, which included John Bardeen (who had changed from

[964] For an analysis of these papers from the Wigner school, we refer to Kohn, 1997, pp. 360–363.

[965] For more details on connections between the American groups, see Hoddeson *et al.*, 1992, pp. 184–193.

mathematics to physics), was joined in 1934 by Conyers Herring, a graduate student in astronomy from Kansas, who—after spending a year at Caltech—decided to switch to physics and obtain his Ph.D. with Wigner. Herring kept in close contact with Bardeen, who left Princeton in 1935 as a Junior Fellow at Harvard, where he established a strong interaction with Slater's solid-state group at MIT (working on the properties of metallic surfaces). Slater, on the other hand, turned his attention in 1936 to the theory of ferromagnetism, in which he tried to compute the energy bonds according to the method developed by Wigner, Seitz, and himself (Slater, 1936a, b). Then, he collaborated with Shockley on the optical absorption of alkali halides (Slater and Shockley, 1936)—the latter had earlier calculated the energy bands of sodium chloride (Shockley, 1936)—and with the MIT experimentalist Erik Rudberg on the theoretical description of inealstic electron scattering from solids (Rudberg and Slater, 1936). That is, the Americans fully seized the topics in solid-state theory which had been pioneered previously by their European colleagues, notably, in Germany, and soon achieved considerable progress and eminence in nearly all fields. Besides the East Coast, solid-state theory also played some role at the University of Minnesota, where John H. Van Vleck continued to investigate magnetic problems; after he left for Harvard in fall 1938, he got a proper replacement in the person of Bardeen. Furthermore, in 1935, Hans Albrecht Bethe, a former pioneer of solid-state theory in Germany, was appointed to the physics faculty of Cornell University (at the instigation of Lloyd Smith, a former postdoctoral fellow at Sommerfeld's institute at Munich, now leading a group on thermionic emission at Cornell in Ithaca, New York). Continental Europe seemed to have been relegated backward; however, there still existed a new centre in England: Nevill Mott's group at Bristol University, which (like its American counterparts) profited from the arrival of German immigrants.

Already before 1933, solid-state theory had obtained some tradition in Cambridge, with Ralph Fowler and Alan Wilson as its principal representatives. After the Nazis came to power in Germany, the situation in Great Britain improved quite a lot in the appropriate fields. First, the 28-year-old Nevill Francis Mott, formerly a nuclear theorist, became professor of theoretical physics in Bristol and changed his field of research to solid-state physics. Second, among the German refugees, there was Max Born, the great old master of solid-state theory, who came to Cambridge as Stokes Lecturer. Before being forced to leave Göttingen in May 1933, Born had published the article on '*Dynamische Gittertheorie der Metallle* (Dynamical Lattice Theory of Metals),' written jointly with Maria Goeppert-Mayer (1933), which presented those aspects of solid-state theory that could be treated without explicit use of quantum mechanics; now he was quite willing to join the work on the modern theory of the field. After leaving Germany, Born went on vacation to Wolkenstein, a resort in Northern Italy, and stayed there until September of that year; during the summer, two British students (Maurice Blackman from London and J. H. C. Thomson from Oxford, who had originally planned to work at his institute in Göttingen) visited him, and he established for them a kind of summer workshop.[966] Upon arrival in Cambridge, Born first became involved in a collaboration with Leopold Infeld on a nonlinear approach to quantum electrodynamics; only after spending the academic year 1935/36 in Bangalore, India, following an invitation from Chandrasekhara Venkata Raman, did he return in fall 1936 to Great Britain to take over the Tait Professorship of Natural Philosophy at the University of Edinburgh, and there he indeed established a school of solid-state physics.

However, the appointment of Nevill Francis Mott at Bristol had a much greater and more immediate impact. 'Towards the end of my second Cambridge year [as fellow of Gonville and Caius College], an invitation came to be professor of theoretical physics in Bristol,' Mott recalled later and gave the following details:

> Arthur Tyndall, Professor of Physics there, had made friends with a member of the Wills family [the rich tobacco products manufacturers], talked to him about physics and got him to finance a building of an enormous laboratory for the subject, quite out of scale with anything else in the recently founded university. Then he had to staff it, and obtained more money from the [Wills] family, from the Rockefeller Foundation and from elsewhere. He had the right idea, believing that a flourishing research school needed a Professor of Theoretical Physics and secured funds for that too. It was the Melville Wills Chair. The first man to be appointed was J. E. Lennard-Jones, but the Cambridge chemists were becoming interested in theory and in the summer of 1932 he left to take up the new Chair of Theoretical Chemistry there. Tyndall had to find someone else. Alan Wilson and myself appeared to him as the only two people with the right qualification not already holding a chair and Tyndall asked me. (Mott, 1986, pp. 43–44)

Since Mott had received an 'unreserved recommendation ... as an admirable candidate' from Lord Rutherford and was persuaded by him to accept the offer, he indeed made a double move by going to Bristol to work there in a field of research that was new to him.[967]

In establishing a research group, Nevill Mott received considerable support from Frederick A. Lindemann, a member of the British government's *Advisory Council for Scientific and Industrial Research*. Lindemann, the influential Oxford theoretical physicist, 'was concerned at his country's neglect of fundamental work on the behaviour of electrons in metals, and persuaded the *Council* that something should be done about it,' Mott said, and recalled: 'My predecessor Lennard-Jones was told that he would receive the necessary funds if he would undertake to devote some time to the subject. This was hardly to be refused, and as a result Harry

[966] Though Max Born, in his recollections, did not mention the topics he dealt with at the workshop in Wolkenstein, we may guess that they included solid-state theory. In any case, he had already published (earlier in 1933) a paper with one of the students on the fine structure of residual rays (Born and Blackman, 1933).

[967] J. E. Lennard-Jones had advocated the appointment of the German physicist Erich Hückel, a molecular theorist, as his successor. Before Mott accepted, Hans Bethe was also a candidate for the chair (Lennard-Jones to Tyndall, 17 August 1932, see Hoddeson *et al.*, 1992, pp. 196–197).

Jones, a graduate of Leeds and post-graduate student of Fowler's, was appointed a senior research assistant in the laboratory, with the task of studying what had been done and where to go from there.' (Mott, 1986, p. 47)[968]

Harry Jones indeed took his job very seriously and worked on the theoretical programme assigned to him already before Mott came to Bristol. For example, in a paper with Clarence Zener, he established some fundamental equations on the theory of metallic conduction (Jones and Zener, 1934a).[969] The following publication of Jones, entitled 'The Theory of Alloys in the γ-phase,' showed 'that it is possible to relate properties [such as the diamagnetic susceptibility and Hall-effect coefficients] to the crystal structure of the alloys, and to the fact that the composition within the γ-phase follows the Hume-Rothery electronic rule' (Jones, 1934a, p. 225). William Hume-Rothery, the Oxford chemist and metallurgist had—from microscopic studies—derived similarities between the phases of various alloys, which showed up when the ratio of the number of valence electrons to the number of atoms in the lattice was the same (Hume-Rothery, 1927). While he had explained it on the basis of an older model of Frederick Lindemann's (assuming that electrons form a lattice as atoms do), Jones derived from the modern Bloch–Brillouin theory of Fermi surfaces in the case of alloys with γ-structure ratios of valence electrons to atoms very close to the empirical values observed by the Oxford chemist (Jones, *loc. cit.*, pp. 230–231; see Hume-Rothery, 1931). Moreover, he explained the observed diamagnetism and Hall effects in the alloys under investigation. Under Nevill Mott, the new professor, Jones continued to examine the properties of alloys in a paper submitted in July 1934, in which he treated the ε- and η-phases of the binary alloys and the various phases of bismuth (Jones, 1934b); he especially found that 'the theory [of Bloch] shows why bismuth does not form a co-ordination lattice' and concluded that the electrical conductivity and diamagnetism of this metal and its alloys can be derived in good agreement with the experiment (Jones, *loc. cit.*, p. 413). In another investigation, carried out jointly with Zener, he determined the change of resistance of metallic lithium in a magnetic field 'in excellent agreement with the observations of Kapitza' (Jones and Zener, 1934b, especially, p. 269).

Mott had learned about the work done at Bristol on metal theory already before arriving there in fall 1933: At Tyndall's request, he had to referee the work of Jones and Zener.[970] After his move to Bristol, Mott quickly became engaged

[968] For more details of the Bristol chair, we refer to Hoddeson *et al.*, 1992, pp. 193–196.

[969] This paper was submitted in early August 1933 to the *Proceedings of the Royal Society of London*. A revised version was accepted on 19 December 1933, and published in the *Proceedings* issue of 1 March 1934.

Clarence Zener, born in 1906 in Indianapolis, Indiana, had studied at Stanford and Harvard (Ph.D. in 1929) and then spent his postdoc years in Princeton, Leipzig, and Bristol (beginning in 1931). Later on, he served in academic positions at Washington University in St. Louis, City College of New York, Washington State University, the University of Chicago, Texas A. & M. University (as Dean of Science), and finally at the Carnegie Mellon University in Pittsburgh; in between, from 1951 to 1965, he worked at the Westinghouse Research Laboratories. He died in July 1993 in Pittsburgh.

[970] See Hoddeson *et al.*, 1992, p. 198.

upon the subject himself and began a collaboration with Jones and the spectroscopist Herbert W. B. Skinner. During his stay for a year at MIT (with a Rockefeller Fellowship), Skinner had measured with Henry O'Brian the soft X-ray spectra of various light metals (O'Brian and Skinner, 1934). As Mott recalled later:

> Skinner, a brilliant experimenter, always covered with cigarette ash, seemed to have three pairs of hands as he pulled out from his spectrometer the radiation from the L_{III} emission of sodium, magnesium and aluminum. They showed bands, just as electron theory predicted, and what is more these bands showed a sharp upper limit. There was no fuzzing due to electron-electron interaction. (Mott, 1984, p. 910)

What surprised Mott and others at that time was the following situation: Due to the model of free electrons, or the model of electrons in a periodic lattice potential, the energy states of a metal were filled to a limiting value, or the Fermi-surface in the wavenumber space, and this limit or surface appeared not to be smeared out because of the mutual interactions of electrons. Now, in their paper, which was received by the *Physical Review* on 11 December 1933 (and published in the issue of 15 March 1934, right after the experimental paper of O'Brian and Skinner), Jones, Mott, and Skinner (1934) gave the following explanation, as Mott recalled later:

> An electron excited into a state separated from the limiting Fermi energy E_F by a small energy ΔE would have a lifetime determined by the Auger processes, in which it could not lose a value of energy greater than ΔE, because to do so it would involve a transition to a state already occupied by other electrons. Therefore the only electrons to which it could lose energy were those in states in the range of energy ΔE below E_F. It followed that the probability $(1/\tau)$ per unit time of a collision would tend to zero with ΔE; the lifetime of an electron in a state just above E would be very long. This meant, using the uncertainty principle, that the limiting energy E_F would be sharply defined. (Mott, 1984, p. 910)

The theoretical programme of Mott and his quite capable and independent assistants in Bristol grew very rapidly. Thus, they quickly won confidence in the principles of the theory of metals and their application to new materials and phenomena. Before hardly two years had elapsed when Mott and his senior assistant completed a book on *The Theory of the Properties of Metals and Alloys* (Mott and Jones, 1936), which—as Mott remarked in his autobiography—was 'based very much on Bethe's *Handbuch* article, and tried to extend its influence by sorting out the differences between real materials, and making approximations and using intuition whenever we liked' (Mott, 1986, p. 48). Besides their own work, such as their treatment of the Hume-Rothery rule, Mott and Jones included in the advanced theoretical chapter of their work the results obtained by Wigner and Seitz and the consequences derived therein. The reviewer of this book for the German journal *Physikalische Zeitschrift* praised it by saying that 'In this book the extremely pleasing and very successful attempt has been made to distribute the weight of the presentation equally among the theoretical derivations and the visual interpretation of the results,' and added:

> How important quantum mechanics promised to turn out for purely practical problems of metal science, emerges among other things from the chapters in which the influence of the concentration of valence electrons on the stability of the types of crystal structure (Hume-Rothery rule) is treated. One is surprised by the large extent to which statements can already be made in this direction without the smallest prerequisite of mathematical knowledge. (Laves, 1937, p. 922)

At this point, we should add that John Slater looked at the relatively simple mathematical treatment of his British colleagues a bit more skeptically; he especially criticized the following point:

> One got the impression in studying the work of Mott and Jones that they felt that the potential actually occurring in energy-band theory was a small perturbation, which could be handled by perturbation theory. This was not justified, but it affected the thinking of the English school of physicists enough so that even now most of them are trying to get valid results relating to energy bands from simplified models, rather than through the direct types of calculation which one can make with the methods now in use. (Slater, 1975, p. 191)

Quite uninfluenced by such objections—if they were expressed at that time at all—Mott proceeded to work on new tasks. In 1935, Ronald Gurney came from Manchester to Bristol and got Mott interested in the investigation of nonmetallic substances and those in which certain defects might disturb the ideal periodic crystalline order.[971] The previous decades had witnessed lots of experimental results, which disclosed new properties of such substances. For example, the phenomena of phosphorescence and luminescence had been investigated by Wilhelm Conrad Röntgen, Philipp Lenard, and later by Robert Pohl and Abraham Joffé. On the theoretical side, Adolf Smekal and Jakov Frenkel had proposed certain theoretical ideas for understanding the structure of 'imperfect' crystals which included disturbances and dislocations. In the 1930's, new concepts emerged from quantum mechanics to describe particular situations in such solids, such as 'the trapping of an electron by an extremely distorted part of the lattice' (Landau, 1933)—later called the 'polaron'—or the 'bound electron-hole pair' (Frenkel, 1936a), later called the 'exciton.'[972] Quantum mechanics was then finally ready to

[971] Ronald Gurney, born in 1898 at Cheltenham, England, studied at Cambridge University and joined the Cavendish Laboratory under Rutherford. After some years with Lawrence Bragg in Manchester and then in Bristol (1935–1941), Gurney went to the United States and worked on various war-related projects. From 1948 to 1950, he was a visiting professor at Johns Hopkins University in Baltimore. He died on 15 April 1953, in New York City.

[972] For details of this development, we refer to Hoddeson *et al.*, 1992, Chapter 4.

play a role also in understanding semiconductors, and the Bristol group took some lead in that enterprize. As Mott recalled several decades later:

> A semiconductor is not a metal; it contains a few electrons that are weakly held in position, so that as the material is warmed up the electrons become free. Alan Wilson had explained this in 1932 [see Section III.6], but there was plenty of work still to do. Some people may remember the wireless receiver of the twenties [1920s], in which one had to press a wire against a galena crystal and find a spot which "rectified" the current, that is, it allowed to pass only in one direction. I published a theory which stood the test of time on that [Mott and Littleton, 1938] and a good many other papers. Ronald Gurney and I set to work on a book, *Electronic Processes in Ionic Crystals*, which came out in 1940. This was inspired by the experimental work of Robert Pohl in Göttingen. I had met him in one of the conferences we organized in Bristol. (Mott, 1986, pp. 53–54)

Robert Wichard Pohl, a long-time colleague of Max Born and James Franck, indeed played a crucial role in the history of semiconductors, and Mott even considered him 'to be one of the true fathers of solid state physics.' The Bristol conference referred to dealt with the broad field of 'The Conduction of Electricity in Solids' and was held from 13 to 16 July 1937, under the joint auspices of the Physical Society of London and the University of Bristol. The lectures—published in the *Supplement* to Volume **49** of the *Proceedings of the Physical Society*—fell into three parts: While Parts II and III covered the topics 'Conduction in Alloys' and 'Conduction in Thin Films,' Part I contained papers devoted to 'Conduction of Non-Metals.' The very first contribution in Part I was presented by Robert Pohl, 'whose school in Göttingen has been for many years investigating photoconductivity in alkali halides,' Mott wrote in the introduction to the reports, and explained further:

> These crystals do *not* show photoconductivity if they are illuminated in their own absorption band; in order to obtain photoconductivity one must first colour the crystal by the addition of some impurity of defect, which gives a new absorption band. The crystals used by Pohl are coloured by heating in the vapour of the alkali metal, which gives the well-known yellow colour for rock salt crystals or blue for KCl. The crystal shows photoconductivity if irradiated in the new absorption band so obtained. This band is known as the F-band and the absorption centres as F-centres; their precise origin is at present uncertain though hypotheses to explain it are advanced in the papers by Gurney and Mott and in the subsequent discussion. (Mott, *Supplement to the Proceedings of the Physical Society* **49** (1937), p. v)

Indeed, in the presentation following Pohl's very detailed account of his findings (Pohl, 1937), Gurney and Mott suggested a tentative wave-mechanical theory of the phenomena. In particular, they assumed the following physical picture: '"By accident" an electron remains on one positive ion [of a crystal] for about 10^{-12} sec,' and 'then the medium around will have become polarized, the positive ions being displaced towards and the negative away from the electron' (Gurney and Mott, 1937, p. 32). Practically in this way a 'potential hole' was formed by the displaced medium, and the electron in the lowest state behaved like a 'trapped electron,' similar to Frenkel's idea of 1936. Gurney and Mott further noticed:

> An electron trapped in this way can only move one ion (*a*) to a neighbouring ion (*b*), if at the same time the surrounding ions move into new displaced positions about (*b*). The frequency with which such a process occurs may be shown to be very small at room temperature, so that we may assume that the trapped electrons are immobile. At high temperatures, however, thermal vibrations may occasionally raise an electron from its trapped position into the conduction band. (Gurney and Mott, *loc. cit.*, p. 33)

It seemed to Gurney and Mott that their mechanism indeed described the behaviour of Pohl's F-centres; especially, they found a 'broadening to be expected as the temperature is raised ... also in satisfactory agreement with experiment' (Gurney and Mott, *loc. cit.*, p. 34). Encouraged by their first success, Mott and Gurney went on to show, as they wrote in the preface of their later book on *Electronic Processes in Ionic Crystals*, 'that the phenomena observed in alkali-halides shed a great deal of light on the more complex behaviour of substances of greater technical importance, such as semi-conductors, photographic emulsions, and luminescent materials' (Mott and Gurney, 1940, p. ix). They discussed these items in detail in the last three chapters of their pioneering monograph.

After Adolf Hitler came to power in Germany in 1933, 'the number of theoretical physicists in England must have doubled through the influx,' Mott said, and recalled: '[Frederick] Lindemann took his Rolls-Royce to Germany and collected some of the best physicists for Oxford, completely reviving Oxford's Clarendon Laboratory.' (Mott, 1986, p. 50) While Lindemann thus succeeded in establishing the field of low-temperature physics at Oxford, Bristol also received an enormous strengthening of its theoretical group by the arrival of six immigrants (including Hans Bethe, Herbert Fröhlich, Walter Heitler, and the young Klaus Fuchs). Not all of them worked on solid-state physics, but so did another visitor, the Swiss physicist Gregory Wannier, who, after taking his doctorate under E. C. G. Stueckelberg at the University of Basle in 1935, went to Eugene Wigner in Princeton and spent the year 1938/39 at the University of Bristol. Back in Germany, Friedrich Hund could only deplore the lack of students and collaborators, which prevented him from forming a similarly active and successful school of solid-state physics at Leipzig.

(c) Low-Temperature Physics and Quantum Degeneracy (1928–1941)

In the most comprehensive account of the history of solid-state physics given thus far—the book *Out of the Crystal Maze*, which emerged from an international project (Hoddeson *et al.*, 1992)—one chapter deals with the development of 'collective phenomena,' which comprise all types of phase transitions, such as those from gas to liquid, liquid to solid, normal conducting to superconducting state, fluid to superfluid, and paramagnetic to ferromagnetic state, in short, all of those situations in which the latter phase arises from what one refers to as 'cooperative phenomena.'[973] Although in the 1920s and 1930s many problems of solid-state theory were explained on a quantum-mechanical basis, especially those which depended on the properties of a single electron, only a few of the 'collective' or 'cooperative' phenomena could then be approached with some success. The theoreticians devoted enormous efforts in the 1930's to describe the spectacular low-temperature behaviour of condensed matter, as exhibited by the phenomena of superconductivity and superfluidity that were explored in detail experimentally during the same period.

The story of low-temperature physics began with the production of liquid helium by Heike Kamerlingh Onnes in Leyden, which was achieved in 1908 and appropriately honoured with the Nobel Prize for Physics in 1913.[974] Three years later, he discovered a completely unexpected effect, namely, the sudden drop of the electrical conductivity in some metals, first in mercury at 4.15 K (Kamerlingh Onnes, 1911). This effect received the great attention of physicists and technicians during the following decades, though for a dozen years, the only place to investigate it was the cryogenic laboratory at Leyden.[975] In 1923, there followed Toronto, where John C. McLennan established his laboratory, and two years later the one at the *Physikalisch-Technische Reichsanstalt* (*PTR*) in Berlin with Walther Meißner as the leading physicist.[976] Meißner's programme after 1925 dealt with the question, 'whether all metals become superconducting' (W. Meißner, 1925,

[973] See Chapter 8 of *Out of the Crystal Maze* (Hoddeson *et al.*, 1992), which treats the development up to the late 1950s.

[974] See Kamerlingh Onnes's Nobel Lecture in *The Nobel Lectures in Physics* (Elsevier, 1967). Heike Kamerlingh Onnes was born on 21 September 1853 in Groningen, and entered the university of his hometown in 1870; then, he went to Heidelberg to study with Robert Bunsen and Gustav Kirchoff from October 1871 to April 1873. Upon his return to Groningen, he continued his studies there and received his doctorate in 1879. Following an assistantship at the Delft Polytechnic (1878–1882 with Johannes Bosscha), Kamerlingh Onnes was appointed professor of experimental physics and meteorology at the University of Leyden. Already in 1881, he concerned himself with the theory of liquids and approached van der Waals' law of corresponding states by means of kinetic theory, which he tried to verify experimentally in the succeeding decades. The cryogenic laboratory in Leyden became the cradle of low-temperature physics in the world, and Kamerlingh Onnes and his collaborators were recognized as *the* experts in that field. Kamerlingh Onnes himself received countless honours, national and international prizes, and memberships of academic societies. He died on 21 February 1926, in Leyden.

[975] Actually, as H. B. G. Casimir reported, 'the first observations were made by Kamerlingh Onnes' assistant Gilles Holst, who later became the founder and first director of the Phillips Research Laboratories, but the experiments were no doubt proposed and planned by Kamerlingh Onnes' (Casimir, 1973, p. 483, footnote). For details of the full story, see Casimir, 1983, pp. 165–166.

[976] At the *PTR*, the president Emil Warburg already ordered in 1913 the establishment of a '*Kältelabor* (Low Temperature Laboratory)' producing liquid hydrogen; by the end of that year, temperatures down to 20 K were reached, and then World War I interrupted further efforts. Work was again resumed in 1920 with the intention of obtaining liquid helium, since (unlike Canada) pure helium was not available and had to be extracted from air with the help of special apparatus. (For details, see Kern, 1994, pp. 148–155.)

Walther Meißner, born on 16 December 1882, in Berlin, studied at the *Technische Hochschule* in Berlin (1902–1907) and obtained his doctorate with a thesis on radiation theory under Max Planck. In 1908, he joined the *PTR*, where he organized the *Kältelabor* and, after the war (in which he served from 1915 to 1918), built the liquid-helium apparatus (1923–1925). In 1934, he was invited to accept a chair of technical physics at the *Technische Hochschule* in Munich. Meißner remained active in research far into the 1960s—after his retirement within the Bavarian Academy of Sciences—he died in Munich on 16 November 1974.

p. 691). His small group added a number of pure metals, e.g., tantalum, titanium, and niobium, and certain alloys and compounds (even nonconductors like copper sulfate) to the list of superconductors, but some metals, e.g., gold, did not join the list down to the lowest temperature (namely, 1.3 K) that could be reached. In the late 1920s and early 1930s, each of the two laboratories in Leyden and Berlin discovered an—as it would turn out, crucial—effect in low-temperature physics: On 17 December 1927, a paper of Willem Hendrik Keesom and M. Wolfke was received by the Amsterdam Academy of Sciences, entitled 'Two Different Liquid States of Helium,' which the authors called 'liquid helium I' and 'liquid helium II' (Keesom and Wolfke, 1928, p. 90); on 16 October 1933, Meißner and his collaborator Robert Ochsenfeld submitted to *Naturwissenschaften* a note on '*Ein neuer Effekt bei Eintritt der Supraleitfähigkeit* (A New Effect at the Onset of Superconductivity),' containing the observation that the magnetic field was completely driven out from the interior of the superconductor (which therefore must be a perfect diamagnetic substance), which was later called the 'Meißner-Ochsenfeld effect' (1933). Both of these experimental findings would largely influence the development of low-temperature physics in the succeeding years.

During the 1930s, the number of cryogenic laboratories increased again. First, the Mond Laboratory at the Cavendish in Cambridge, England, was completed and began research in 1933 (see Volume 4, p. 33). Peter Kapitza, its Russian-born director and collaborator of Ernest Rutherford's, devised in 1934 a new type of liquifier for helium by making use of 'explosive' adiabatic turbo-expansion (which was at least 10 times as effective as previous installations: Kapitza, 1934). Still in the same year, 1933, Franz Simon came as a refugee to the Clarendon Laboratory at Oxford and brought with him from Breslau a very simple liquifier suitable for helium as well.[977] However, in summer 1934, the Mond Laboratory lost Peter Kapitza, its director, while he was visiting the Soviet Union for vacation—as he did every year—but was not allowed to return to Cambridge. The Soviet authorities prevented him from returning to England, in order to have him build and direct a new institute of the U.S.S.R. Academy of Sciences in Moscow. The Soviet government even purchased a part of the Mond Laboratory equipment for that purpose, especially the large electromagnet for low- and normal-temperature experiments and other apparatus for £30,000. Thus, research could be continued in Cambridge—now under the direction of John Cockcroft—and begun in Moscow under Peter Kapitza.[978] Soon, Kapitza would make a major discovery, that of 'superfluid helium' in 1938.

[977] See the report of Kurt Mendelssohn (1964, p. 7), another refugee from Breslau, who installed the first helium liquifier at Oxford.

[978] Peter (Pjotr) Leonidovich Kapitza was born in Kronstadt near St. Petersburg on 9 July 1894, the son of the military engineer Leonid Kapitza and his wife Olga (*née* Strebnitskaia), a teacher. He graduated in electrical engineering (from Abram Joffé's department) in 1918 at the Polytechnical Institute, and was sent by his teacher in 1921 to Cambridge to work with Rutherford. Success in various topics of experimental research, notably, in the application of high magnetic fields, made it possible for him to have a leading role at the Cavendish—since 1930 the Messel Research Professor of the Royal Society (Fellow of the Royal Society in 1929) and the Director of the Mond Laboratory (1930–1934). The new Moscow 'Institute for Physical Problems' began to operate in late 1936 and soon achieved

On the other hand, his Institute for Physical Problems did not constitute the first place for low-temperature research in the Soviet Union. As early as 1930, Lew Schubnikow (or Lev Shubnikov, as his name would be transcribed later in Western scientific literature), who had acquired considerable expertise during a stay in Leyden, was invited to join the recently founded Ukrainian Physico-Technical Institute at Kharkov to establish a suitable laboratory there; already by the end of 1931, he produced liquid hydrogen, and in the following year, he obtained liquid helium according to Simon's method. After 1934, when Shubnikov had the Meißner apparatus available (which was installed by Meißner himself), a continuous supply of helium existed in Kharkov.[979] In Germany, the takeover of the *PTR* by Johannes Stark and the departure of Meißner in 1934—as well as the firing of Max von Laue as a theoretical advisor—considerably weakened the formerly so successful *Kältelabor*. Several years later, a new research centre was planned in Berlin–Dahlem as part of the *Kaiser Wilhelm-Institut für Physik*, which started its operation early in 1937. Peter Debye, who had been appointed director of the *Kaiser Wilhelm-Institut* in 1935, had already installed some apparatus allowing him to obtain low temperatures, based on his method of adiabatic demagnetization of paramagnetic quantities (Debye, 1926). In a report on the new institute—which, against the wishes of the Nazi minister of education, was also named the '*Max Planck-Institut*'—he emphasized 'two special fields of research, namely, in the first place investigations in the domain of nuclear physics with the help of very high voltages, and in the second place experiments at very low temperatures close to the absolute zero' (Debye, 1937, p. 257). In the latter field, Debye wished—as he wrote in another publication—to continue the experimental work of the former German emigrants Franz (Francis) Simon and Nikolaus (Nicholas) Kurti (see Debye, 1938, p. 85).[980]

As we have already mentioned, the low-temperature research in the 1930's concentrated very much on the detailed properties of two outstanding phenomena, superconductivity and superfluidity, of which the former had been known since 1911 and the latter became evident only in 1938. Theoretical considerations, both classical and quantum-theoretical, played a vital role in obtaining insight into their very nature, although it was only in the case of superfluidity that the efforts helped

considerable progress in low-temperature physics. During World War II, Kapitza headed the newly founded Department of Oxygen Industry; after the war, he entered into research on microwave generators and plasma physics. In 1978, Kapitza shared the Nobel Prize in Physics with Arno A. Penzias and Robert W. Wilson; he was cited 'for his basic inventions and discoveries in the area of low-temperature physics' (*Nobel Foundation*, ed., 1992, p. 416). He died in Moscow on 8 April 1984.

[979] Lev Shubnikov was born in 1901 in St. Petersburg, where he studied from 1918 to 1926 (at the university) and (after 1922) at the Physico-Technical Institute, obtaining a *Diplom* (with Ivan Obreimov, who later became the director of the Physico-Technical Institute at Kharkov). From 1926 to 1930, Joffé sent Shubnikov to work at the Leyden cryogenic laboratory. In Kharkov, the theoretician Lev Landau, initially a critic of his work, became a dear friend and collaborator. Shubnikov was imprisoned on 6 August 1937; he was sentenced to death and shot on 11 November of that year. (For a more detailed biographical sketch of his life, see Rotter, 1997a, b).

[980] Peter Debye's programme at the newly established Dahlem institute has been discussed by Kant, 1996, especially, pp. 236–242.

in obtaining some understanding within the framework of quantum mechanics. We shall first deal with the older discovery, superconductivity, for the explanation of which quantum-theoretical concepts were invoked from the very beginning. Notably, at the third and fourth Solvay Conferences of 1921 and 1924, respectively, in Brussels, the principal reports by Kamerlingh Onnes and others dealt with such interpretations. While in 1921 the question was asked as to whether Bohr's atomic model yielded the necessary 'coherence' of the conduction electrons, such that they would give rise to superconducting filaments (see the report of Kamerlingh Onnes), in 1924, Hendrik Lorentz and Owen W. Richardson proposed new ideas to explain this coherence.[981] These ideas would appear quite arbitrary, if not unnatural, when viewed with the following discovery of quantum mechanics. After the first successes of the new atomic theory in the situation of the normal electrical (and thermal) conductivity, Heisenberg and several other physicists turned their attention to solving also the puzzle of superconductivity, but the quite serious efforts of the foremost experts—including Felix Bloch, Lev Landau, Jakov Frenkel, Niels Bohr, and Ralph Kronig (besides Heisenberg himself)—did not attain the goal, as we have concluded in Section III.6 previously. New experimental observations in the late 1920s and early 1930s, such as the existence of superconductivity in impure substances or even nonconducting materials, the transition from the superconducting into the normal conducting state in a high magnetic field and a kind of hysterisis effect complicated the picture of the phenomenon.[982] With the availability of low temperatures in many different laboratories since the early 1930's, new theoretical efforts were ushered in, especially after the discovery of the Meißner–Ochsenfeld effect in 1933 provided a turning point in the history of superconductivity, although they did not lead to the desired solution of a microscopic quantum-mechanical description. The enormous difficulties which stood in the way of obtaining success in this specific topic might, of course, be easily understood: The physicists had first to disentangle the detailed phenomena observed and then develop a phenomenological, macroscopic description, before they could pass over to the real microscopic quantum-mechanical theory. Thus, the main progress in understanding superconductivity during the 1930s consisted in establishing a so-to-say semiclassical approach, which was worked out primarily in Berlin, Leyden, and Oxford.

In February 1933, Paul Ehrenfest communicated what would be his last paper for publication, entitled '*Phasenraumwandlungen im üblichen und erweiteren Sinn, classifiziert nach den entsprechenden Singularitäten des thermodynamischen Potentials* (Phase Transitions in the Usual and Extended Sense, Classified According to the Corresponding Singularities of the Thermodynamic Potential)' to the Amsterdam Academy.[983] In the summary of his paper, Ehrenfest wrote:

[981] See the reports, given in Mehra, 1975a, Chapters 4 and 5.

[982] For details, we refer to Hoddeson *et al.*, 1992, pp. 495–498, and Dahl, 1992, Chapters 6 and 7.

[983] On 25 September 1933, Ehrenfest went to the institution, where his youngest, mongoloid son was taken care of, drew a revolver, and first shot the child and then himself. (See Casimir, 1983, p. 148)

> The measurements of Keesom and his collaborators on the characteristic change of the specific heat of fluid helium and also of superconductors suggest to discuss a certain generalization of the concept of phase transition. The discontinuity curves of differently high order in the plane of the thermodynamical potential become the transition curves for the "transitions of first, second and higher order" between the two phases. In case of the usual transitions of first order the equation of Clapeyron applies to the jumps in the first differential quotient of the thermodynamic potential, i.e., between $S'' - S'$ and $v'' - v'$; in case of second-order transitions analogous equations between the jumps of the specific heat and the jumps of $\partial v/\partial T$ and $\partial v/\partial p$ are valid [where S, v, T and p denote the entropy, volume, temperature and pressure, respectively]. (Ehrenfest, 1933, p. 153)

Willem Hendrik Keesom and J. A. Kok (1932) had observed before a discontinuity of the specific heat at the transition temperature of superconducting tin, and the Leyden experimentalists reported similar results on the behaviour of liquid helium (see below). In a series of papers, Hendrik Casimir and Cornelius Jacobus Gorter, and Ehrenfest's student A. J. Rutgers, worked out the thermodynamics of superconductors (Gorter and Casimir, 1934a; Rutgers, 1934, 1936), which—together with certain electrodynamical assumptions (such as zero magnetic field within the superconductor)—provided a reasonable description of the observed phenomena.[984]

Gorter and Casimir presented these results at the first larger meeting on low-temperature physics, held during the 10th *Deutsche Physiker-und Mathematiker-Tag* at Bad Pyrmont in September 1934 (Gorter and Casimir, 1934b). Gorter and Casimir argued: 'Since an electron-theoretical treatment of superconductivity seems to encounter great difficulties still, and a really satisfactory treatment of the riddle of superconductivity is actually still missing, for the moment it may not be useless at all to attack the problem from the phenomenological side without embarking upon detailed electron-theoretical concepts'; they expected 'to gain in this way an insight into the requirements that have to be satisfied by a [microscopic] theory' (Gorter and Casimir, *loc. cit.*, p. 963). Then, they presented a two-fluid model, where a superconductor was imagined to consist of a normal-conducting and superconducting phase. Gorter and Casimir aroused the interest of a quite interested public; besides them, Peter Debye spoke on the methods to obtain very low temperatures, Klaus Clusius and Walther Meißner displayed new experimental results on superconductivity, and Willem Keesom exhibited results on the caloric behaviour of metals at low temperatures; Eduard Justi and Max von Laue discussed what they called 'the phase equilibrium of the third kind;' Arnold Eucken discussed the problem of phase transitions in general; Eduard Grüneisen and H. Reddemann analyzed electron- and lattice-conductivity; R. Schachenmeier proposed his electron-theoretical model of superconductivity; K. Clusius and E. Bartholomé reported on the properties of condensed heavy hydrogen; R.

[984] For a review of the thermodynamics of superconductors, see von Laue, 1938. Von Laue had begun earlier, in 1932, to formulate the electrodynamics of superconductors (1932a). For the background story, especially in Leyden, we refer to Casimir, 1983, Chapter 6.

Suhrmann and G. Barth discussed the high reflection of silver mirrors at very low temperatures; and finally R. Suhrmann and D. Dempster reported on the photoelectric effect of composite photocathodes at low temperatures. An especially lively discussion followed von Laue's talk on phase equilibria, in which Gorter, Friedrich Hund, Keesom, and Clusius participated. Keesom, in particular, defended Ehrenfest's concept of 'phase transitions of higher order' against von Laue's 'equilibria of higher order,' while Clusius supported the latter view by drawing attention to the situation in crystalline transformations. (See the reports in *Physikalische Zeitschrift* **35**, issue No. 23.)

Topics concerning low-temperature phenomena also formed part of the 'International Conference on Physics,' held in London in October of the same year.[985] About seven months later, the Royal Society organized 'A Discussion on Superconductivity and Other Low-Temperature Phenomena,' to which, besides British experts, those from Canada, France, Germany, and The Netherlands were invited. John C. McLennan of Toronto presented the opening address, in which he first spoke about the progress achieved since 1932 in liquefying helium (notably, the methods of Simon and Mendelssohn in Oxford and Kapitza in Cambridge); then he went on to discuss the recognition of the new properties of liquid helium (mainly in Leyden), and the efforts to reach still lower temperatures down to 0.0044 K with the method of adiabatic demagnetization (in Leyden and Oxford); and then he discussed new superconductors, as well as new experiments dealing with the superconductivity of thin films (in Toronto), and a series of other phenomena observed. In particular, McLennan drew attention to a noteworthy feature noticed by the Leyden experimentalists and its interpretation:

> De Haas and [H.] Bremmer have carried out an extensive series of measurements of the thermal resistances of metals at liquid helium temperatures, both supraconductors and non-supraconductors. For pure supraconducting metals below their transition points the thermal conductivity is increased when the electrical supraconductivity is interrupted by a magnetic field. Qualitatively this is not difficult to understand, for the electrons responsible for supraconductivity must be excluded from taking part in thermal phenomena. Quantitatively, however, there are great difficulties in reconciling these thermal conductivity measurements with other experiments. (McLennan *et al.*, 1935. p. 6)

Evidently, he concluded, that the rise of thermal resistance must be ascribed to the impurities and irregularities of the lattice, which also explain the residual electrical resistance. Finally, McLennan sketched the recent progress in the thermodynamical and electrodynamical description of phenomena in superconductors, emphasizing especially the contributions of Richard Becker and his collaborators (Becker *et al.*, 1933) and the most recent work of Fritz and Heinz London (1935) and well as that of Gorter (1935).

[985] The other part of the London conference was devoted to nuclear and high-energy physics—see Section IV.5.

After McLennan's introductory speech, John Cockcroft and David Shoenberg reported about the production of liquid helium and the ensuing experiments at the Mond Laboratory after the unexpected departure of Kapitza; then, Keesom spoke about the thermodynamical properties of helium and superconducting substances as discerned from the investigations at Leyden and from Meißner on the magnetic behaviour of superconductors. Léon Brillouin discussed the difficulties of a quantum-theoretical interpretation of the phenomena observed with superconducting materials. In contrast to normal conductivity, he said:

> Superconductivity arises in metals still containing some impurities, and shows a decidedly different character; so there was a first hypothesis to be introduced that superconductive metals should crystallize in undistorted lattices, the impurities gathering in separate spots, included in holes in the perfect lattice; so the supercurrent could flow through the regular lattice, ignoring the impurities. (Brillouin, in McLennan *et al.*, 1935, p. 19)

Since neither such a hypothesis had been substantiated empirically nor did impurities in general reduce superconductivity, Brillouin proposed the existence of 'a very peculiar type of energy-momentum curve, which had electrons with high kinetic energies and low velocities but insensible to thermic agitation,' and these might be present in face-centred cubic lattices (Brillouin, *loc. cit.*, p. 20). Brillouin argued further that every model of superconductivity had to satisfy a very general condition proposed by Felix Bloch, which 'is of great importance and practically forbids any interpretation of superconductivity within the frame of classical physics.' As he explained in detail:

> Let us suppose a current I to flow through a part of the metal (it might be flowing along the surface or some volume of the conductor); the energy of the metal will be E; we want to prove that E cannot be a minimum. If we apply a potential difference P between both ends of the conductor, then the energy of the system will be increased by a term $I \cdot P\,dt$, in a very short time interval dt; by changing the sign of P we can make this term positive or negative, hence we see that there is always a possibility of decreasing the total energy E, which cannot be a minimum. Bloch's calculation is just a translation, for wave mechanics, of this elementary result. (Brillouin, *loc. cit.*)

'Bloch's theorem,' as the result was called later, evidently implied in the classical view the existence of unstable currents, which contradicted the observed stable supercurrents.

After several participants, including the Oxford experimentalists Kurti, Simon, and Mendelssohn, had reported on further empirical findings in low-temperature physics, their theoretical colleague Fritz London talked at length about a new 'macroscopical interpretation of superconductivity' (F. London, in McLennan *et al.*, *loc. cit.*, pp. 24–33). He began by saying that 'it seems that the principal obstacle which stands in the way of understanding this phenomenon is to be sought in its customary macroscopical interpretation as a kind of limiting case of ordinary conductivity,' and quickly added: 'It is rigorously demonstrable that, on the basis of the recognized conceptions of the electron theory of metals, a theory of supraconductivity is impossible—provided that the phenomenon is interpreted in the usual way.' (F. London, *loc. cit.*, p. 24) If one would give up this conventional conception, Fritz London continued, 'the apparent contradiction to Bloch's theorem' might be avoided, because the latter 'deals with a system without external electric or magnetic field,' and he further stated:

> The macroscopical description I have developed together with H[einz] London shows that it is possible to work out this program to some extent and so to escape Bloch's dilemma. The supracurrent there appears as a diamagnetic current which is maintained by a magnetic field. In a permanent current in a ring the magnetic field is produced by the current itself. The most stable state of the ring has no current, unless an external field is applied. The states in which the ring possesses a permanent current are not states of lowest energy but are metastable under macroscopic conditions. (F. London, *loc. cit.*, p. 26)

Fritz London then outlined the theory, given in a joint paper with his brother Heinz, a former student of Franz Simon in Breslau.[986] The London brothers started from the fundamental equation

$$c\,\mathrm{curl}(\boldsymbol{\Lambda}\,\mathbf{j}_s) = -\mathbf{H}, \tag{717}$$

where $\mathbf{H}$ denoted the magnetic field vector, $\mathbf{j}_s$, denoted the superconducting current, and Λ denoted a positive constant characterizing the peculiar superconductor. Further, they noticed 'that the total supraconductor is regarded as a big diamagnetic atom and that the screening of an applied magnetic field is effected by volume currents instead of an atomic magnetization' (F. London, *loc. cit.*, p. 27). Moreover, the solution yielded an exponential decrease of the magnetic field in the interior of a superconducting body, with a penetration depth of $c\sqrt{\Lambda}$ of the order of 10^{-6} to 10^{-5} cm.[987] The current flowing in the surface layer determined by it would then shield the interior of the superconductor from the magnetic field and explain the Meißner–Ochsenfeld effect.

[986] Heinz London was born in Bonn on 7 November 1907, and grew up under the influence of his brother Fritz, who was seven years older (since their father, a university professor of mathematics, had died early). After graduating from a classical gymnasium, Heinz studied physics and chemistry at the University of Bonn and—after half a year of practical experience with the chemical firm of W. C. Heraeus in Hanau—at the *Technische Hochschule* in Berlin; from 1929 to 1931, he studied at the University of Munich and later completed his doctorate at the University of Berlin (1933), with a thesis published in 1934 and partly containing the ideas of Becker *et al.* (1933). In 1934, he joined his teacher Francis Simon and his brother Fritz at Oxford, with whom he collaborated; in 1936, he moved to take on a position at the H. H. Wills Laboratory in Bristol. During World War II, he worked on isotope separation, and later joined the Harwell Atomic Energy Research Establishment, where he continued his work on isotope separation and on low-temperature physics problems. He died on 3 August 1970, near Oxford.

[987] This fact, first published by Richard Becker *et al.* (1933), had been recognized independently by Heinz London.

In their detailed paper on '*Supraleitung und Diamagnetismus* (Superconductivity and Diamagnetism),' the London brothers had displayed the two different approaches that have to be used for the superconducting state and the normal state (coexisting in the superconductor according to the Gorter–Casimir two-fluid model), respectively (F. and H. London, 1935, especially, pp. 343–345). In the discussion at the Royal Society conference—three months after the submission of the paper cited above—Fritz London now outlined the sketch of a programme which provided 'a foundation of our macroscopical equations by the theory of electrons in metals' (F. London, in McLennan *et al.*, 1935, p. 31). In the case of normal conductivity, he claimed, the new theory would lead to the very weak diamagnetism of the Landau–Pauli type, but:

> Suppose the electrons to be coupled by some form of interaction in such a way that the lowest state may be separated by a finite interval from the excited ones. Then the disturbing influence of the field on the eigenfunctions can only be considerable if it is of the same order of magnitude as the coupling forces. (F. London, *loc. cit.*)

In a model calculation, Fritz London demonstrated how such an interaction would work and indeed give rise to the characteristic (phenomenological) relation between the vector potential **A** and the current **j** of a superconductor, namely,[988]

$$\mathbf{A} = -\Lambda c\,\mathbf{j}. \tag{717$'$}$$

In a set of further investigations, carried out with Max von Laue and his brother, Fritz London then developed the full electrodynamical theory of superconductors (von Laue and H. London, 1935; H. London, 1935; F. London, 1936, 1937). This macroscopic theory, together with the thermodynamical studies of the Leyden theorists and others (see the report of von Laue, 1938), then completed the phenomenological description of the general situation. Still, it would not account for all the details of observed phenomena, notably, in the Meißner–Ochsenfeld effects, as Kurt Mendelssohn recalled later:

> By hitting upon a simple technique of measurement, we [in Oxford] were able to make rapid progress, and we soon had results showing a whole spectrum of behaviour, from a complete Meissner effect in pure mercury to a complete freezing in a flux of alloys. In between, there were all the intermediate steps, showing clearly that the presence of even a small proportion of a second constituent caused radical departure from the ideal behaviour. Moreover, we found that, in those cases which differed from the ideal behaviour, there were two critical fields instead of one. There was the field at which the electrical resistance became normal, and for which we retained the name "threshold field" (now H_{c_2}), and a much lower value at which the magnetic flux first began to penetrate the sample. This we called the "penetration field" (now H_{c_1}). (Mendelssohn, 1964, p. 9)

[988] The observations demanded that in the case of a ring-shaped superconductor, the right-hand side of Eq. (717′) had to be increased by grad v, where v could be associated with a parameter in the quantum-mechanical eigenfunctions.

That is, during the period between 1934 and 1936, the concept of a different type of superconductor sneaked into the theoretical work of Gorter (1935) and Heinz London (1935), as well as into the experimental observations of Lev Shubnikov and his collaborators in Kharkov; the latter gave them the name 'type II superconductors.'[989]

Because of several reasons, the advance of superconductor research slowed down after 1936, for as Cornelius Gorter recalled:

> Among the first is the fact that the number of research workers in the field was small and that some of them almost simultaneously left it. Shubnikov disappeared, Mendelssohn concentrated a large part of his attention on the superfluid properties of helium II while ... I returned to magnetism. As to the properties of alloys, I feel that the lack of metallurgical facilities and experiences also weighed heavily. The rapid advance of the years 1932–1936 was consolidated by the appearance of Shoenberg's excellent monograph. But, though much further valuable work ... was carried out, this consolidation did not lead to a concentrated attack on the remaining problems before the outbreak of the war. (Gorter, 1964, p. 7)

The British (actually South African) David Shoenberg, author of 'the excellent monograph' (Shoenberg, 1938), actually collaborated for a while with the members of Kapitza's Institute in Moscow, where Lev Landau—who, unlike Lev Shubnikov—had escaped from the prosecution in the Ukraine in 1936 but would still be caught in 1938 in Moscow and imprisoned until freed a year later with the help and great efforts of his director Peter Kapitza—began to publish papers on the subject (Landau, 1937b, 1938b).[990]

Before concluding the story of superconductivity, a brief review should be given of two proposals in the late 1930's to obtain a microscopic description of the phenomenon, after such attempts had almost ceased completely in 1933.[991] Toward the end of 1936, John Slater turned his attention to the problem. In particular, he suggested 'that the superconducting state of metallic electrons may arise by application of perturbation theory to Bloch's theory' in the following way:

> The excited states of a metal, on the usual theory, form a continuum whose lower boundary is the normal state. It is shown that under some circumstances there are nondiagonal matrix components of energy states in this continuum, which would tend to depress a few of the lowest states below their normal positions. These special states of the metal would resemble a thermodynamic phase, stable only at the lowest

[989] For an early report of these investigations, see Gorter, 1935.

[990] For the story of Landau's life and work, see Mehra, 1990, and Meiman, 1990.

[991] In passing, we just refer to a proposal by R. Schachenmeier from Berlin: At the Bad Pyrmont meeting of September 1934, he drew attention to a theory which he had pursued for two years, and which rested on the hypothesis that 'of the two external electrons of a metal one stays in the vicinity of the atomic core, while the other is distributed over the entire metal and may be called the conductivity electron'—the latter, in the degenerate part of the spectrum, alone being responsible for superconductivity (Schachenmeier, 1934, especially, p. 968). Since he could not suggest any quantum-mechanical mechanism to create the superconducting phase, his theory played no role in further discussions.

temperatures, and having practically zero entropy, in agreement with present theories of superconductivity. They would also tend to have extremely low resistance, on account of the small concentration of energy levels per unit energy. It is therefore suggested that these states may constitute the superconducting state. (Slater, 1937a, p. 195)

As a consequence, Slater expected no superconductivity to exist for the alkali metals, and for Cu, Ag, and Au, and only at extremely low temperatures for W, Fe, Ni, and Pt. A further investigation of the idea in a later paper revealed, e.g., 'The wave functions correspond to electrons which can wander for some distance through the metal, but are held to a finite region by forces of interaction with positive ions'; hence they would 'carry no current in the ordinary way, for they correspond to the correlation of an electron and a positive ion, and these two move together' (Slater, 1937b, p. 214). The detailed calculations yielded results in apparent agreement with London's phenomenological theory; Slater even calculated magnetic transition fields having the right order of magnitude of a few hundred gauss.[992] A second attempt came from Munich, where Sommerfeld's doctoral student Heinrich Welker treated the 'diamagnetism of a free electron gas differently from the usual approach' (Welker, 1938, especially, p. 920). For this purpose, he especially added the following assumption: 'In contrast to a normal conductor, there should be required for the superconductor at least an energy of the order of magnitude $A = kT_c$ in order to remove the electron from the ground state.' (Welker, *loc. cit.*, p. 924) In a detailed presentation, submitted a year later to *Zeitschrift für Physik*, Welker worked out some quantum-mechanical aspects of his proposal; in particular, instead of an electron gas, he made use of an 'electron fluid,' which was created by the action of a magnetic exchange force—the latter giving rise to a velocity behaviour of the electrons which deviated from that of the usual metal electrons—and characterized by a critical temperature $T_c = 1$ K for the transition from fluid to gas (Welker, 1939, especially, p. 539). World War II forced Welker to abandon these efforts; he became rather involved in work on wireless telegraphy.[993]

As in superconductivity, research in the other main field of low-temperature

[992] Stimulated by Slater's approach, Hund published a paper, dealing with the magnetic behaviour of small metal pieces at low temperatures in a quantum-mechanical model; he concluded that if they have suitable dimensions, they exhibit 'at low temperatures and weak magnetic fields a region of strong diamagnetism' and 'are similar to superconductors.' (Hund, 1938, p. 114)

[993] Heinrich Welker was born on 9 September 1912, in Ingolstadt and studied mathematics and physics at the University of Munich from 1931 to 1935. In 1936, he obtained his doctorate and then served as Sommerfeld's assistant, receiving his *Habilitation* in 1939; then, he moved to the *Luftfunkforschungs-Institut* in Oberpfaffenhofen (1940–1945), although he remained associated simultaneously (from 1942 to 1944) with the physicochemical institute of Klaus Clusius at the University of Munich. After the war, he worked for Westinghouse, Paris (1947–1951), and then he took over the solid-state physics department of the Siemens–Schuckert Company in Erlangen (1951–1977), where he developed the new, so-called III–V compounds, to replace silicon as semiconductors. For his research, he won several prizes and honorary degrees; he was also elected president of the *Deutsche Physikalische Gesellschaft* in 1977. Welker died on 25 December 1981, at Erlangen.

physics, namely, the properties and applications of liquid helium, remained for two decades the monopoly of Kamerlingh Onnes' Leyden laboratory. The early investigations did not reveal any remarkable features of liquid helium, except that it did not become a solid down to the lowest temperatures that had been reached thus far, and perhaps it would still remain a liquid at absolute zero. Nevertheless, the Leyden physicists (and later also those in the Toronto laboratory) noticed some unusual features; e.g., liquid helium reached a maximum density at about 2.2 K, and exhibited other irregular behaviour at the same temperature (discovered in 1912 and 1925, respectively). Kamerlingh Onnes, known for his dislike of speculations, hesitated to emphasize these features too much and insisted on further experimental examination. Only after his death did Willem Keesom and M. Wolfke give an official summary of the situation with respect to helium at the meeting of the Amsterdam Academy of Sciences on 17 December 1927. In particular, they wrote in the introduction:

> When measuring the dielectric constant of liquid helium between the boiling point and 1.9 K on June 11th last, we observed that at a temperature almost corresponding with the one at which Kamerlingh Onnes had found a maximum in the density curve, the dielectric constant showed a sudden jump or at least a jump made in a very small temperature-region. The thought suggests itself that at that temperature the liquid helium transforms into another phase, liquid as well. If we call the liquid, stable at higher temperatures "liquid helium I," the liquid, stable at lower temperatures "liquid helium II," then the dielectric constant of liquid helium I should be greater than that of liquid helium II. (Keesom and Wolfke, 1928, p. 90)

While the repetitions of the experiment in the following days did not settle the last point completely, Keesom and Wolfke discussed the other known phenomena supporting their views on the two liquid-helium phases. Especially, they arrived at the conclusion that the measurements of density, specific heat, and surface tension (the latter two had been carried out between 1925 and 1926, when Kamerlingh Onnes was still alive) could be interpreted satisfactorily with the new idea.[994] Further experiments by Keesom and Wolfke in November and December 1927, which determined the cooling and heating curve of helium in the critical temperature region, definitely confirmed the existence of a transformation point, and the authors concluded: 'We think that it is most probable that we have to do here with two different states of liquid helium, which transform into each other'; that is, at 2.3 K, the following facts had to be acknowledged:

> Of those phases the liquid helium II (stable at lower temperatures) compared with helium I has a smaller density, a great heat of vaporization, a smaller surface tension, while the transformation liquid helium II $\rightarrow$ liquid helium I takes place with an absorption of heat, of which the amount can be valued for the present at 0.13 cal/gram. (Keesom and Wolfke, *loc. cit.*, p. 94)

[994] Many original papers on liquid-helium research have been reprinted with a proper introduction in Galasiewiez, 1971.

Under Keesom, who had directed the Leyden laboratory with Wander Johannes de Haas, the research on liquid helium flourished immensely.[995] Having succeeded in 1926 in solidifying helium (under an external pressure of 25 atmospheres to overcome the zero-point effects), he selected the study of the properties of liquid helium as the main field of his research. In 1932, with Klaus Clusius, he discovered an extremely sharp maximum in the specific heat curve, which they also related to the transition from helium I to helium II; like the changes of molecular rotation in solids, no latent heat was connected with this transition (Keesom and Clusius, 1932). A further study, carried out with his daughter Anna Petronella on the same topic, provided the proper name for the transition point in helium, namely: 'According to a suggestion made by Prof. Ehrenfest, we propose to call that point, considering the resemblance of the specific-heat curve with the Greek letter λ, the lambda point.' (W. and A. Keesom, 1932, p. 742)

Parallel to the experimental efforts at Leyden and Toronto—where John C. McLennan and collaborators observed, for instance, in 1932 that helium changed its outer appearance during the transition (the fluid became more placid at lower temperatures)—theoretical ideas emerged to explain the anomalous behaviour of liquid helium. Already in his Nobel lecture in 1913, Kamerlingh Onnes had speculated that the density maximum 'could be possibly connected with quantum theory' (Kamerlingh Onnes, 1967, p. 327). More than a decade later, Franz Simon indicated, in a footnote in a paper dealing with the processes to achieve the absolute zero of temperature, that Keesom's result should be connected with a degeneracy of liquid helium (Simon, 1927, pp. 808–809, footnote 4). Then, M. C. Johnson of the University of Birmingham studied the degeneracy question in helium theoretically by studying the empirical equation of state curve of liquid helium between 4 and 5 K, and concluded: 'It is shown that ... degeneracy would comprise 15 percent of the total departure of helium from the ideal gas laws at 4 and 5 K, the remainder being due to the true imperfection of intermolecular forces.' (Johnson, 1930, p. 170) In the discussion of Johnson's paper at a meeting of the Physical Society of London, John Edward Lennard-Jones stressed the 'great theoretical interest' of the investigation, though he also pointed out: 'The author considers only the Fermi-Dirac statistics, whereas the theory indicates that helium atoms should obey Bose-Einstein statistics. It would add to the value of his work if the author could consider the effect of the latter statistics on helium near the critical point.' (Lennard-Jones, in Johnson, *loc. cit.*, pp. 179–180)

[995] Willem Keesom, born on 21 June 1876, on the Frisian island of Texel, studied from 1894 to 1900 at the University of Amsterdam under Johannes Diderik van der Waals and Jacobus Henricus van't Hoff, then joined Kamerlingh Onnes as an assistant in Leyden (1900–1917), and obtained his doctorate in 1904. In 1917, Keesom became a professor of physics at the University of Veterinary Science at Utrecht, and in 1923, he succeeded Kamerlingh Onnes as professor in Leyden. He split the responsibilities for directing the low-temperature laboratory with de Haas; Keesom took over the cryogenic plant, while de Haas directed research on electrical, magnetic, and optical properties of matter at low temperatures. He retired in 1945 from his position at the University of Leyden and died on 3 March 1956, in Leyden.

In the mid-1930s, then, Fritz London (who maintained contact with Simon in Oxford) thought about a lattice structure of the diamond type to describe the properties of liquid helium, which Herbert Fröhlich picked up and worked out in detail in Leyden, concluding that the 'λ-point appears as a phenomenon similar to the transition point of metal alloys when the ordered phase passes over into the disordered one' (Fröhlich, 1937, p. 639). In 1938, while spending some time in Paris to establish himself there, Fritz London returned to a quite general discussion of the problem of Bose–Einstein condensation and its connection with the λ-point phenomenon of liquid helium, first in a short note dated 5 March published in the *Nature* issue of 9 April (F. London, 1938a) and then in a paper received by *Physical Review* on 12 October and published in the issue of 1 December (F. London, 1938b).[996] While Fritz London, in the earlier note, just criticized Fröhlich's interpretation of the helium transition and rather claimed that 'it seems difficult not to imagine a connection with the Bose-Einstein statistics' (F. London, 1938a, p. 643), he went ahead and proved this assertion in the following paper. He began by saying that Einstein's discovery of the condensation phenomenon in the ideal gas of massive Bose particles 'has not appeared in textbooks, probably because [George] Uhlenbeck in his [doctoral] thesis questioned the correctness of Einstein's argument,' and he continued:

> In discussing some properties of liquid helium I realized that Einstein's statement has been erroneously discredited; moreover, some support could be given to the idea that the peculiar phase transition ("λ-point") that liquid helium undergoes at 2.19 K, very probably has to be regarded as the condensation phenomenon of the Bose-Einstein statistics, distorted, of course, by the presence of molecular forces and by the fact that it manifests itself in the liquid and not in the gaseous phase. (F. London, 1938b, p. 947)

Fritz London now proposed 'a quite elementary condensation mechanism,' which he imagined to occur in an ideal gas below a certain temperature depending on the mass and the density of the atoms involved. Accordingly, two components existed, a condensed one, whose particles assumed zero momentum, and the excited one with the particles having a momentum distribution similar to the classical one. 'If one likes analogies, one may say that there is actually *a condensation, but only one in momentum space*, and not in ordinary space, i.e., an equilibrium of two phases, *one* containing the molecules N_0 of momentum zero and occupying in the space of momenta a zero volume; and *another one* showing a distribution over all momenta similar to that which is realized for $T > T_0$,' he concluded the display of his model (F. London, *loc. cit.*, p. 951). Fritz London devoted the remaining part of the paper to discussing its application to the problem of liquid helium, remarking that he would 'not insist here on details' of the conceptions worked out; on the other hand, the new phenomena discovered meanwhile by the Cambridge experimentalists seemed to confirm them in general.

[996] In fall 1938, Fritz London went to Duke University in North Carolina and was appointed in 1939 as a professor of theoretical chemistry (later physical chemistry).

On the whole, Fritz London's theoretical views were received positively both by experimentalists and theoreticians. Thus, when John Frank Allen and Harry Jones of the Mond Laboratory discovered what they called the 'fountain effect' in February 1938—i.e., the rise of the helium II-liquid in a bulb when heat flow was applied (Allen and Jones, 1938)—Ralph Fowler greeted the interpretation, though Keesom in Leyden remained reserved.[997] However, the experimental situation changed extremely rapidly, since in the *Nature* issue of 8 January, there had appeared two letters, one submitted by Peter Kapitza from Moscow and the other by Allen and A. D. Misener in Cambridge, who announced the discovery of a new physical property of helium II, which Kapitza called 'superfluidity' (Kapitza, 1938; Allen and Misener, 1938). Fritz London cited both of these notes in a review of 'The State of Liquid Helium Near Absolute Zero,' presented in December 1938 at a meeting of the American Chemical Society in Providence, Rhode Island (F. London, 1939).

In late 1936, Kapitza's Institute for Physical Problems finally got into action.[998] Work in the field of low-temperature physics was started, as Kapitza reported several years later in detail to his colleagues in the U.S.S.R. Academy of Sciences, based on some previous observations in Leyden and Cambridge (W. and A. Keesom, 1936; Allen, Peierls, and Zaki Uddim, 1937) on the heat conductivity (Kapitza 1941a).[999] They had indicated a high viscosity of helium II, but the experimental methods to determine that property had to be improved; and, as Kapitza noted:

> We were able to build a viscometer with a slit only half a micron wide, through which the helium was made to flow. The experiment was so designed as to avoid the adverse effect of turbulence to a considerable extent. Under such circumstances it became evident that the observed viscosity of helium II was at most a thousandth of the value previously found. (Kapitza, 1941a, English translation, p. 22)

Kapitza continued: 'We also managed to show that the value of viscosity obtained by us actually represented its possible upper limit, as in fact the actual value could have been anywhere below this limit,' and added: 'In other words, even our narrow slit did not fully eliminate the deleterious effect of turbulence.' In any case, the successes of Kapitza's laboratory in early 1938 'aroused considerable discussion and criticism.' (Kapitza, *loc. cit.*)

The main difficulty to arrive at a consistent value of helium's viscosity actually

[997] See Brush, 1983, pp. 177–178.

[998] John Cockcroft, Director of the Mond Laboratory at Cambridge, had negotiated the contract with the Soviet Government, and in winter 1935/36, the equipment that had been purchased from England had been transported to Moscow; in summer 1936, Cockcroft had finally helped in installing it properly (see Hartcup and Allibone, 1984, pp. 75–77; see also the letter from Kapitza to Rutherford, dated March 1936, quoted in Badash, 1985, pp. 103–110).

[999] These publications had also initiated the experiments on the flow of liquid helium in Cambridge (Allen and Misener, 1938; Allen and Jones, 1938).

consisted in disentangling the laminar and turbulent flows of liquid helium. Thus, Kapitza wrote in his letter to *Nature*, dated 3 December 1937:

> In an attempt to get laminar motion the following method was devised. The viscosity was measured by the pressure drop when the liquid flows through the gap between the disks (1) and (2); these discs were of glass and optically flat, the gap between them adjustable to by mica distance pieces. The upper disc (1), was 3 cm in diameter with a central hole of 1.5 cm diameter, over which a glass tube (3) was fixed. Lowering and raising this plunger in the liquid helium by means of the thread (4), the level of the liquid column in the tube (3) could be set above or below the level (5) of the liquid in the surrounding Dewar flask. The amount of flow and the pressure was deduced from the difference of the two levels, which was measured by a cathedometer. (Kapitza, 1938, p. 74)

The observed results 'were rather striking,' yielding a viscosity of helium II of at least 1,500 times smaller than that of helium I. Upon some further considerations, Kapitza finally concluded:

> We are making experiments in the hope of still further reducing the upper limit to the viscosity of liquid helium II, but the present upper limit (namely, 10^{-9} c.g.s. units) is already very striking, since it is more than 10^4 times smaller than that of hydrogen gas (previously thought to be the fluid of least viscosity). The present limit is perhaps sufficient to suggest, by analogy with superconductors, that the helium below the λ-point enters a special state which might be called a "superfluid." (Kapitza, *loc. cit.*)

While Cambridge's Mond Laboratory team arrived at a similar conclusion in their letter, dated 22 December and published right after Kapitza's (Allen and Misener, 1938), some criticism (as mentioned above) arose from the previously observed property of helium II to creep as a thin film over the walls of vessels; hence, in the Moscow viscosimeter a too low value for the superfluid helium should be measured. As Kapitza objected later: 'It is noteworthy, however, that this criticism, which originated from scientists in the USA and Canada, disregarded the fact that helium can creep in a thin film the thickness of which, as measured by [I. K.] Kikoin and [P. P.] Lazarev, is less than one hundredth of a micron, and only when its viscosity is one million times less than the limit established by us,' and concluded: 'Thus it turned out that the criticism of the high fluidity of helium was based on a phenomenon the explanation of which required an even greater fluidity.' (Kapitza, 1941a, English translation, p. 23)[1000] In the following years, Kapitza worked with his collaborators on removing the contradictions in explaining the conduction properties of helium II and clarifying the mechanism of the motion of this fluid in capillary tubes; in a detailed memoir, entitled 'The Study of Heat Transfer in Helium II,' the results were summarized in the following sentences:

[1000] Also Keesom, together with G. E. Macwood, examined the viscosity of liquid helium and observed a strong decrease of the lambda point (Keesom and Macwood, 1938).

> Keesom and Keesom showed that liquid helium II in capillary tubes possesses an unusually large heat conductivity which, by analogy to superconductivity in metals they named "superheat-conductivity." In opposition to this view the author put forward a hypothesis which held that this abnormal heat conductivity was not due to some exceptional thermal property of helium II but to heat transferred by convection currents whose presence can be anticipated owing to the exceptionally high fluidity of liquid helium II, and the author suggests that it should be named "superfluidity." That the heat conductivity is due to these convection currents is established by the experiments described.... In this way, the author came to the conclusion that the heat conductivity of helium II is due only to the high velocity of the flow of the helium in the thin film which is possible owing to its "superfluidity." (Kapitza, 1941b, p. 181)

In 1938, the theoreticians had to respond quickly to the changing situation in helium II. While a group from Amsterdam concentrated on explaining the original observations by the Keesoms (1936) by a particular mechanism, which they assumed to operate in degenerate ideal Bose–Einstein gases (Michels, Bijl, and de Boer, 1938) and which did 'not seem to be quantitatively in agreement [with] the latest publications about the flow of liquid helium' (Michels, Bijl, and de Boer, *loc. cit.*, p. 124),[1001] Laszlo Tisza, a Hungarian physicist then working experimentally at the *Collège de France*, discussed with Fritz London (also in Paris) the latter's theory of gas degeneracy in liquid helium and applied it to the transport phenomena in helium II.[1002] For this purpose, like London, he considered helium below the λ-point as consisting of two independent fluids, where the atoms of one component occupy excited states and those of the other condense in the ground state; the latter form the superfluidity and do not participate in the transport phenomena. As a consequence, the viscosity of liquid helium would arise entirely from the excited atoms, and the superfluid component could flow through very thin capillaries on account of its zero viscosity (thus, giving rise to the observed 'fountain effect': Tisza, 1938a). Kurt Mendelssohn from Oxford recalled: 'When Tisza's paper was published, London was at first furious because he deplored the rash use of his own cautious suggestion.' (Mendelssohn, 1977, p. 258) Fritz London, the experienced theorist, felt the difficulty of simultaneously having two fluids together, which consisted of the same type of atoms that should be indistinguishable in principle; moreover, the properties of the superfluid assumed by Tisza would not follow from Einstein's theory of ideal, degenerate gases, though the explanation of the empirical data appeared to be quite promising. Unshaken by such arguments, Tisza went ahead and submitted toward the end of the year two

[1001] In a later paper, the same authors modified the description of their mechanism to include the phenomenon of superfluidity (Bijl, de Boer, and Michels, 1941).

[1002] Laszlo Tisza was born on 7 July 1907, in Budapest and studied at the universities of his home town, Göttingen (1928–1930) and Leipzig (1930), obtaining his doctorate in 1932. After that, he spent two years at the Physico-Technical Institute in Kharkov (1935–1937), and then three years in Paris (1937–1940). In 1941, he went to the United States and obtained a professorship at MIT, where he had a very productive and distinguished career.

short notes to the *Académie des Sciences* in Paris (Tisza, 1938b, c). In particular, he assumed that the inhomogeneities of temperature might produce inhomogeneities of the densities and pressures of the two phases; further, he predicted the existence of 'temperature waves' propagating with the velocity

$$v = \sqrt{\frac{kT}{m}\left[1 - \left(\frac{T}{T_0}\right)^5\right]} \tag{718}$$

(Tisza, 1938b, p. 1036), which were discovered several years later by Vasilii Peshkov in Kapitza's Institute for Physical Problems and called the 'second sound' (Peshkov, 1944). This ingenious interpretation of superfluidity as a Bose-condensation phenomenon encountered serious criticism for some time. Notably, Lev Landau, in the introduction of his own paper on the subject, wrote:

> L. Tisza suggested that helium II should be considered as a degenerate ideal Bose gas. He suggested that the atoms found in the normal state (a state of zero energy) move through liquid without friction. This point of view, however, cannot be considered as satisfactory. Apart from the fact that liquid helium has nothing to do with an ideal gas, atoms in the normal state would not behave as a "superfluid." On the contrary, nothing could prevent atoms in a normal state from colliding with excited atoms, i.e., when moving through the liquid they would experience a friction and there would be no superfluidity at all. In this way the explanation advanced by Tisza not only has no foundation in his suggestions but is in direct contradiction with them. (Landau, 1941a, p. 71)

Landau joined Kapitza's Institute for Physical Problems in 1937, thereby escaping from the Stalinist purges in Kharkov (which had cost his friend Shubnikov his life). He immediately began to publish, especially the two-part paper entitled '*Zur Theorie der Phasenumwandlungen* (On the Theory of Phase Transitions)' (Landau, 1937a). In the second part, he discussed in particular the nature of liquid crystals and contemplated about the possibility that liquid helium might be represented by such a liquid crystal (raising, however, certain doubts against this assumption: Landau, *loc. cit.*, especially, English translation, in Landau, p. 215). Landau was arrested on the charges of espionage in April 1938, and was freed only a year later with the heroic assistance of Kapitza, and slowly got back into scientific work, now being concerned with research on problems of nuclear and high-energy physics. His extensive investigation on the theory of helium II constituted the first publication concerned with the central programme of Kapitza's institute (Landau, 1941a).[1003] After rejecting the London–Tisza description of liquid helium below the λ-point as an ideal Bose–Einstein gas, Landau instead proposed to derive the properties of the superfluid from a consistent quantum-mechanical approach to a

[1003] A smaller note, received by *Physical Review* on 23 June 1941, contained some of the results of his later paper (Landau, 1941b).

fluid, which was formulated in Sections 1 and 2 of his memoir (Landau, *loc. cit.*, pp. 71–76). In particular, while considering the energy spectrum of the quantum liquid in Section 2, Landau found for the excited states the results:

> Every weakly excited state can be considered as an aggregate of a number of single "elementary excitations." As far as the excited levels of the potential spectrum are concerned, the potential internal motions of the liquid are longitudinal waves, i.e., these motions are sound waves. Therefore, the corresponding elementary excitations are simply sound quanta, i.e., phonons. The energy of the phonons is known to be a linear function of their momentum p:
>
> $$\varepsilon = c \cdot p, \qquad [(719)]$$
>
> c being the velocity of sound. Thus, at the beginning of the potential spectrum, the energy is proportional to the first power of the momentum.
>
> An "elementary excitation" of the vortex spectrum might be called a "roton."† (Footnote†: This name was suggested by I. E. Tamm.) Those special reasons which stipulate a linear dependence of ε on p for phonons do not exist for rotons. For small momenta p the energy of the roton can be simply expanded in powers of p; in view of the isotropy of the liquid the expansion of the scalar ε in powers of the vector p only contains terms with even powers, so one may write
>
> $$\varepsilon = \Delta + \frac{p^2}{2\mu}, \qquad ((720)]$$
>
> where μ is an "effective mass" of the roton ... [and] Δ large compared with kT (at low temperatures only when the aggregate of rotons can be treated as a gas). (Landau, *loc. cit.*, pp. 75–76)

With these theoretical arguments, Landau obtained for the heat capacity of the liquid helium at very low temperatures, i.e., definitely below the λ-point, Debye's T^3-law plus a small roton correction, which accounted well for the available data—though the predicted magnitude seemed to be too small by far (Section 3). On the other hand, he proved in a straightforward manner that limiting velocities for the liquid existed, below which neither phonons ($V < c$) nor rotons ($V < \sqrt{2\Delta/\mu}$) would occur; hence, he concluded: 'This means that the flow of the liquid does not slow down, i.e., helium II discloses the phenomenon of superfluidity.' (Landau, *loc. cit.*, Section 4, p. 78) In Section 5, Landau then demonstrated how to explain in his approach the two-fluid picture of Tisza formally. He also worked out the flow of superfluid helium through capillaries, in agreement with 'the recent ingenious experiments made by P. L. Kapitza' (Landau, *loc. cit.*, Section 6). In the last two sections, Landau considered the equations describing the propagation of sound in liquid helium and the analogy of the formulae describing superfluidity with those for the superconductivity current. Finally, he stated:

> As in helium II we come to the conclusion that the superconducting current must not transfer heat. This is supported by the fact that the thermoelectric phenomena are absent in superconductors. (Landau, *loc. cit.*, p. 90)

Landau's theory can be considered as the last achievement in the theory of low-temperature phenomena before World War II got into full swing. In spite of later criticism (see the Epilogue), which resulted in a series of improvements in the work on liquid helium both in the Soviet Union and in the West, it eventually gained for Landau the Nobel Prize in Physics for 1962.

(d) Toward Astrophysics: Matter Under High Pressures and High Temperatures (1926–1939)

On 10 December 1946, Percy W. Bridgman of Harvard University received the Nobel Prize in Physics 'for the invention of an apparatus to produce extremely high pressures, and for the discoveries he made therewith in the field of high-pressure physics' (citation in Bridgman, 1964, p. 47). Since his early investigation in 1905 of the influence of high pressure on certain optical phenomena, Bridgman had devoted his life to discovering the behaviour of matter under high pressures, and successfully extended the existing limit of 3,000 kg/cm^2 first to 20,000 kg/cm^2 and finally to 500,000 kg/cm^2.[1004] Besides constructing his brilliant apparatus by a skillful use of resources and techniques, Bridgman investigated both the solid and fluid states of matter under these high pressures, discovering new modifications (such as water in solid form different from ice or polymorphous states of several substances, e.g., of phosphorous) and observing the physical properties (such as the electrical resistance or elasticity) of various materials. The January 1935 issue of *Reviews of Modern Physics* contained right at the beginning a report on 'Theoretically Interesting Aspects of High Pressure Phenomena,' which Bridgman opened with the words:

> Until very recently the condensed phases of matter, solid and liquid, have appeared too complicated to make it worthwhile to spend much effort in acquiring an understanding of them. . . . But now our understanding of the atomic, as distinguished from nuclear, phenomena presented by matter in its rarefied states is rapidly becoming satisfactory, and in a sense exhausted, so that the attack on the problem of condensed states is obviously next on the program. . . . The condensed state, *par excellence*, is obviously presented by matter under high pressure, so that, to say the least, our understanding of the condensed state cannot be regarded as satisfactory until we can give an account of the effect of pressure on every variety of physical phenomena. This we can at present do in very few cases indeed. (Bridgman, 1935, p. 1)

[1004] Percy W. Bridgman was born on 21 April 1882, in Cambridge, Massachusetts, and entered Harvard University in 1900 to study physics. Upon receiving his Ph.D. in 1908, Bridgman joined the Harvard faculty and stayed there for the rest of his scientific career (1910 instructor, 1919 assistant professor, 1926 Hollins Professor of Mathematics and Natural Philosophy, 1950 Higgins Professor). He died on 20 August 1961, in Randolph, New Hampshire.

In the above-mentioned review paper, Bridgman discussed in some detail the various aspects of high-pressure phenomena on the basis of modern atomic theory, beginning in Section II by considering the atomic changes. Here, the virial theorem—first formulated by Walter Schottky (1920) and extended by Max Born, Werner Heisenberg, and Pascual Jordan (1926) to quantum mechanics, and finally applied by John Slater (1933) to atomic problems—led to an equation for solids. Thus, the volume change of solids and fluids could be described, if a reasonable law of forces in atoms was assumed (Sections III–V). In addition, the periodic relation obeyed by the compressibility of the chemical elements, and the compressibility of single crystals seemed to work out (Sections VI and VII). In Section VIII, Bridgman turned to the consequences of cases of the two-phase equilibrium between two condensed phases, either solid–liquid or solid–solid, as produced by the pressure. Then, he approached irreversible changes under pressure (Section IX), the discontinuities and transitions (occurring) of the second kind (Section X), as well as the changes observed in electrical resistance, thermoelectric phenomena, thermal conductivity (Sections XI–XIII), and in the viscosity of fluids (Section XIV); he also studied the condition of rupture in solids (XV). 'Finally,' in the last Section XVI, he declared, 'we may indulge in a few perfectly frank speculations as to what sorts of effects may be expected at pressures very much higher than those yet reached in the laboratory,' and added:

> There is no natural upper pressure, nor there is any limit to the amount of energy which can be imparted to a substance by compressing it; in the stars there are perfectly stupendous pressures of the order of billions of atmospheres, and we know that sometimes under such conditions matter is consolidated to densities of the order of 100,000—the field thus offered for speculations is a fascinating one. (Bridgman, 1935, p. 31)

In these speculations, Bridgman stressed, the quantum-mechanical principles, such as Pauli's exclusion principle or Heisenberg's uncertainty relation, played a decisive role.

The theory of the stars and their structure, to which Bridgman referred above, had—since more than a decade—been fostered especially by Arthur Stanley Eddington, the famous Cambridge astronomer (see Eddington, 1921b and 1926). In the mid-1920's, Eddington's younger colleague, the Cambridge theoretical physicist Ralph Fowler, showed some interest in the problem (e.g., Fowler, 1925c; Fowler and Guggenheim, 1925). Clearly, the temperatures and pressures within stars had to reach extraordinary values; for example, one assumed temperatures of millions of degrees to exist in the centre of an ordinary star, while the high pressures in cold stars should create enormous densities. 'The accepted density of matter in stars such as the companion of Sirius is of the order of 10^5 gr./c.c.,' Fowler began the report of his investigation, published in the 10 December 1926 issue of the *Monthly Notices of the Royal Astronomical Society of London* and entitled 'On Dense Matter,' and then pointed out 'that densities up to 10^{14} times that of terrestrial materials may not be impossible' (Fowler, 1926, p. 114). Now in what the astronomers called the 'white dwarfs,' wherein the temperatures were known to be comparatively low, there arose—according to Eddington (1926, §117)—a paradox: The star had emitted, before approaching the dwarf state in its evolution, so much energy that it now possessed less than the same amount of matter (consisting of normal atoms) that it would have in the same volume at absolute-zero temperature if the classical gas equation were valid. However, Ralph Fowler was convinced that Enrico Fermi's new gas theory (statistics) must rather be used to describe matter in stars, and claimed:

> When this form of statistical mechanics is adopted, it at once appears that the suggested difficulty resolves itself, and there is really no difficulty at all.... When the correct relation [between energy and temperature] is substituted, it is found that the limiting state of such dense stellar matter is one in which the *energy* is still, as it must be, excessively great, but the *temperature* is zero! Since the temperature determines the radiation, radiation stops when the dense matter has still ample energy to expand and form normal matter if the pressure happens to be removed. As the dense matter radiates its energy away, the number of its possible configurations rapidly falls, and therewith the temperature. The absolutely final state is one in which there is only one possible configuration left. Temperature then ceases to have any meaning, for the star is strictly analogous to one gigantic molecule in its lowest quantum state. We may call the temperature then zero. (Fowler, 1926, p. 115)

Fowler now proved that indeed, by considering the dense stellar matter to be represented by an assembly of free electrons and bare nuclei—both of which obeyed Fermi statistics—of net charge zero (and neglecting the electrostatic forces between electrons and nuclei), the particles could still retain kinetic energy while the temperature dropped to zero: Namely, the gas was in a degenerate state for the low temperatures existing in white dwarfs and became normal (i.e., obeyed classical gas laws) only for temperatures of the order of 10^9 degrees (much higher than, say, existing in the companion of Sirius). As we have mentioned earlier, Fowler's paper was the first application of Fermi's statistics to a physical problem, but soon Wolfgang Pauli and especially Arnold Sommerfeld in Germany would establish its validity in the terrestrial problem of metal electrons. Three years later, in 1929, the celestial branch of quantum mechanics would attract a young man far away from England and Germany—this was Subrahmanyan Chandrasekhar in India—who carried the theory of white dwarfs started by Ralph Fowler to the next stage.

When Arnold Sommerfeld visited Madras, India, in fall 1928 and lectured to science students at the Presidency College, Chandrasekhar was among them. He listened to the famous visitor from Germany, and did even more, as he recalled decades later:

> I went to visit him in the hotel and told him that I was interested in physics and would like to talk to him. He asked me to see him the following day, and so I went. He asked me how much I had studied. I told him that I had read his *Atomic Structure and Spectral Lines*, an English translation [of Sommerfeld, 1922d]. He promptly told me that the whole of physics had been transformed after the book had been written and referred to the discovery of wave mechanics by Schrödinger, and the new devel-

> opments due to Heisenberg, Dirac, Pauli and others. I must have appeared somewhat crestfallen. So he asked me, what else did I know? I told him that I had studied some statistical mechanics. He said, "Well, there have been changes in statistical mechanics, too," and he gave me the galley proofs of his paper on the electron theory of metals, which had not yet been published. (Chandrasekhar, in Wali, 1991, pp. 61–62)

Actually, far from being discouraged, the 18-year-old Chandrasekhar (later to be called 'Chandra' by friends, acquaintances, and students alike) plunged into Sommerfeld's new work. He found that he had no greater problems in understanding its contents; hence, he looked for a further application of the new statistics.[1005] In January 1929, he completed his first paper on 'The Compton Scattering and the New Statistics' and sent it to Ralph Fowler, whose work on the Fermi statistics in stellar matter he had come across; Fowler and Nevill Mott studied the manuscript, made a few suggestions regarding style, approved it, and with the author's agreement, communicated it in June 1929 to the *Proceedings of the Royal Society of London*, where it was promptly published (Chandrasekhar, 1929). Encouraged by the positive response from England, Chandrasekhar continued the investigation of statistical problems and submitted further papers to British journals, which were also published. At home, the young scholar attended the meetings of the Indian Science Congress at Madras (in January 1929, where he presented the results of his first investigation) and Allahabad (in January 1930). Armed with a Government of India scholarship, he went to England, got admitted to the Trinity College in Cambridge, and began to attend the lectures of Paul Dirac, Arthur Eddington, Ralph Fowler, and other celebrities at the University of Cambridge. Fowler, who effectively helped Chandrasekhar in overcoming bureaucratic obstacles, appreciated his previous work and promised to send his new work to Edward Arthur Milne in Oxford for consideration.

Before leaving for England, Chandrasekhar had made an attempt to combine Fowler's ideas on the application of the Fermi statistics to dense white dwarfs with Eddington's grand theory of stellar constitution.[1006] In particular, by taking into

[1005] Subrahmanyan Chandrasekhar was born on 19 October 1910, in Lahore, India, the son of Chandrasekhara Subrahmanyan Ayyar and nephew of Chandrasekhara Venkata Raman. He entered Presidency College, Madras, in 1925 and studied physics and mathematics, and completed his degree in spring 1930. Besides Sommerfeld in 1928, he looked after another distinguished visitor—Werner Heisenberg—to Madras in 1929, and discussed his first research papers with the latter (see Chandra's biography by Wali, 1991, p. 64). After passing his final examinations at Presidency College, he applied for a Government of India scholarship to continue his studies in England; he received the scholarship and went to Cambridge and got access to work under Ralph Fowler's guidance at Trinity College. Upon receiving his Ph.D. in late 1933, Chandra obtained a fellowship at Trinity College. Two years later, he accepted an offer (from the astronomer Otto Struve) to work at the University of Chicago; having just been married in India, the Chandrasekhars went to the Yerkes Observatory, Williams Bay (on Lake Geneva, Wisconsin), where the University of Chicago's astronomy department was then housed. In 1952, Chandra was also appointed as a professor of physics in the Institute of Nuclear Studies, as a colleague of Enrico Fermi and other distinguished physicists. He died on 21 August 1995, in Chicago. (For Chandrasekhar's biography, see Wali, 1991, and the obituary by Brown *et al.*, 1995)

[1006] Eddington's book on stellar constitution (1926) had been studied by Chandrasekhar in India and was amongst the books he took with him to England (Wali, 1991, p. 76).

account the relativistic increase of the electron's mass in Fermi's statistics, he had hit upon a surprising result: There was evidently a limit to the mass of a star that could evolve into a white dwarf. Upon arrival in Cambridge, he discussed the situation with Fowler and later with Milne (in Oxford); the latter seemed to be pleased because Chandrasekhar supported his own conception of stellar evolution (see, e.g., Milne, 1930), which partly deviated from Eddington's by assuming the existence of inhomogeneous structures in stars. Thus, he quickly communicated one of Chandrasekhar's papers on the subject, entitled 'The Highly Collapsed Configurations of a Stellar Mass,' to *Monthly Notices* (where it appeared in the March issue: Chandrasekhar, 1931b). Chandrasekhar indeed concentrated his attention upon 'the development of Milne's theory of collapsed configurations a step further' (Chandrasekhar, *loc. cit.*, p. 456) and considered in detail three cases: (i) the relativistic-degenerate Fermi gas; (ii) the nonrelativistic degenerate case; and (iii) the essentially homogeneous case. Thus, he arrived at three different situations, characterized by the mass M of the star, namely, the classes: I. $M \leq 0.61 \odot \beta^{-3/2}$ [with $\odot$ denoting the mass of the sun, and β the quantity $1 - (\kappa L/4\pi cGM)$, where κ, L and G are, respectively, opacity and luminosity of the star and Newton's gravitational constant]; II. $0.61 \odot \beta^{-3/2} < M \leq 0.92 \odot \beta^{-3/2}$; and III. $M > 0.92 \odot \beta^{-3/2}$. The first situation represented a polytropic case, and the second gave rise to a composite case of a degenerate envelope surrounding a homogeneous core. 'To apply the above classification to the known white dwarfs,' Chandrasekhar concluded the following: 'O_2 Eridani, Procyon B and van Maanen's star possibly belong to Class I. That the companion of Sirius is Class II, is also likely.' (Chandrasekhar, *loc. cit.*, p. 465)

Milne expressed much less satisfaction with the main result of the original consideration of the degenerate relativistic Fermi gas, which Chandrasekhar had formulated in the short note on 'The Maximum Mass of Ideal White Dwarfs.' After many arguments with Milne, Chandrasekhar sent it in November 1930 to the American *Astrophysical Journal*, where it appeared in the March issue (Chandrasekhar, 1931a). The central argument was the following: The pressure P of a relativistic Fermi gas may be described by the equation

$$P = K\rho^{4/3}, \tag{721}$$

with ρ denoting the density and K a constant (equal to 3.619×10^{14} c.g.s. units). 'We can now immediately apply the theory of polytropic gas spheres for the equation of state given by [Eq. (721)], where for the exponent γ we have $\gamma = \frac{4}{3}$ or $1 + \frac{1}{n} = \frac{4}{3}$ or $n = 3$,' Chandrasekhar wrote (Chandrasekhar, *loc. cit.*, p. 82) and added the relation (due to Eddington, 1926, p. 83),

$$\left(\frac{GM}{M'}\right)^2 = \frac{(4K)^3}{4\pi G} \tag{722}$$

(with G, Newton's gravitational constant; M, the mass of the star; and M', a number of the order of 1) which yielded the result for the mass

$$M = 1.822 \times 10^{33} = 0.91\odot. \tag{723}$$

'As we have derived this mass of the star under ideal conditions of extreme degeneracy, we may regard 1.822×10^{33} as the maximum mass of an ideal white dwarf,' Chandrasekhar concluded (Chandrasekhar, 1931a, p. 82).

Unlike Chandrasekhar, Milne and Eddington (both renowned for their contributions to the topic of stellar structure) did not take the result very seriously. They could easily point to the fact that the question as to which equation of state had to be applied in the problem had not yet been decided at the time. Thus, Edmund C. Stoner, who had worked since several years on Fermi gases, argued that 'concentrations which are of astrophysical interest (corresponding roughly to densities of the order of 10^5 to 10^8) happen to fall in a range where neither [i.e., the relativistic nor the nonrelativistic] equation can be strictly applied—possibly *because* this is a transition region' (Stoner, 1932, p. 651). On the other hand, as we have mentioned, Eddington and Milne carried on a vigorous debate during these years about the true model which should describe stellar structure, and each of them sought to win Chandrasekhar to his side: One preferred 'composite' models (Milne, 1930), while the other insisted on perfect gas models, in order to account for all specific, observed properties, such as the opacity of stellar matter or the surface temperatures.[1007] Chandrasekhar went on to work out the consequences of his relativistic theory in a detailed paper, entitled 'Some Remarks on the State of Matter in the Interior of Stars' and submitted in September 1932 from Copenhagen to *Zeitschrift für Astrophysik* (Chandrasekhar, 1932). There, he demonstrated, in particular, 'that for *all centrally condensed stars of mass greater than* $\mathscr{M}$ [which was about 1.2 times his critical mass, Eq. (723)], the perfect [i.e., non-degenerate] *gas equation of state does not break down, however high the density may become, and the matter does not become degenerate*,' and: '*An appeal to the Fermi-Dirac statistics to avoid the central singularity cannot be made*.' (Chandrasekhar, *loc. cit.*, p. 324) Hence, he turned against the models of both Eddington and Milne, and rather stated a problem at the end of the paper:

> We may conclude that great progress in the analysis of stellar structure is not possible before we can answer the following question: *Given an enclosure containing electrons and atomic nuclei (total charge zero) what happens if we go on compressing the material indefinitely?* (Chandrasekhar, *loc. cit.*, p. 327)

In spite of such controversies, however, Chandrasekhar got along quite well with his 'opponents,' and occasionally even wrote a paper with Milne. At the same

[1007] Milne claimed that Chandrasekhar's relativistic theory of white dwarfs contradicted certain conclusions derived from his model; he thought that Chandrasekhar had not investigated the problem 'to the bitter end' (see Wali, 1991, p. 121).

time, he travelled around Europe—e.g., to Göttingen in 1931, to Copenhagen in fall 1932 and to Liège in March 1933 (to deliver lectures on stellar atmospheres)—and got to know personally many of the prominent quantum physicists (including Niels Bohr and Max Born). On 20 June 1933, he passed the orals of his Ph.D. examination at Cambridge University (with Fowler and Eddington as examiners), and in fall of that year, he competed for and won a fellowship of Trinity College. Milne expressed to him his 'intense pleasure,' and wrote:

> I hasten to send you my heartiest congratulations. I am very proud to have been associated with you in some of your work, and the satisfaction at your success is a very personal one. (Milne to Chandrasekhar, 9 October 1933, quoted in Wali, 1991, pp. 109–110)

Upon Milne's nomination, Chandrasekhar became a Fellow of the Royal Astronomical Society and then began to attend the Society's meetings regularly. In summer 1934, he travelled again, this time to the Soviet Union to meet especially two colleagues, the theoretical physicist Lev Landau and the astronomer Viktor A. Ambartsumian. Landau had, in a paper 'On the Theory of Stars' (published in the *Physikalische Zeitschrift der Sowjetunion*), also derived a critical mass of white dwarfs of about the same magnitude as had Chandrasekhar (Landau, 1932). However, he had not taken the conclusion too seriously, arguing that the stars may contain condensed and noncondensed states separated by unstable regions which would change the whole situation. As his biographer wrote, Chandrasekhar's visit to the U.S.S.R. turned out to be quite stimulating:

> During his week's stay at Leningrad, Chandra gave two lectures at Pulkovo Observatory to large audiences. One of the two lectures was about his work on white dwarfs and the limiting mass, which had attracted little or no attention in Cambridge. Ambartsumian suggested investigating the problem in greater detail by avoiding some of the approximations Chandra had resorted to and working out the exact theory. As Chandra recalls, it was this remark of Ambartsumian, his interest and encouragement, that made him take up the subject again after his return to Cambridge and follow it to its conclusion. (Wali, 1991, p. 117)

In fall 1934, Chandrasekhar indeed went on to work seriously on the problem, and Eddington supported these endeavours with 'a great deal of interest,' and even provided him with a new hand calculator to carry out numerical work (Wali, *loc. cit.*, p. 127). He still expected that Chandrasekhar would eventually arrive at the result that every star, no matter what its mass, could become a white dwarf. But the investigations ended differently and reestablished the original result of 1930.

By the end of 1934, Chandrasekhar submitted two papers to the *Monthly Notices of the Royal Astronomical Society* containing the latest conclusions: The first of these papers continued the topic of the 1931 investigation (on 'The Highly Collapsed Configurations of a Stellar Mass': Chandrasekhar, 1935a; see also the later paper: Chandrasekhar, 1935c); the second paper contained the most detailed discussion of ‘Stellar Configurations with Degenerate Cores’ (Chandrasekhar, 1935b). He subsequently received an invitation to present these matters in the January meeting of the Society. Some fifty years later, he still remembered the events of that dramatic meeting quite vividly:

> I knew the assistant secretary, a Miss Kay Williams, rather well, and she used to send me the program ahead of the meeting. On Thursday evening I got the program and found that immediately after my paper Eddington was giving a paper on “Relativistic Degeneracy.” I was really very annoyed because, here Eddington was coming to see me every day, and he never told me he was giving a paper.
>
> Then I went to College and Eddington was there. Somehow I thought Eddington would come to talk with me, so I did not go over to talk with him. After dinner I was standing by myself in the combination room where we used to have coffee, and Eddington came up to me and asked me, “I suppose you are going to London tomorrow?” I said, “Yes.” He said, “You know your paper is very long. So I have asked Smart [the Secretary of the Royal Astronomical Society] to give you a half hour for your presentation instead of the customary fifteen minutes.” I said, “That’s very nice of you.” And he still did not tell me that he too was presenting a paper. So I was a little nervous as to what the story was.
>
> The next day at the Burlington House [the headquarters of the Royal Society, where the meeting took place], at the usual tea before the meeting, [William Hunter] McCrea and I were standing together and Eddington came by. McCrea asked Eddington, “Well, Professor Eddington, what are we to understand by ‘Relativistic Degeneracy?’ ” Eddington turned to me and said, “That’s a surprise for you,” and walked away. (Chandrasekhar, in Wali, 1991, p. 124)

Indeed, on the following day, 11 January 1935, after Chandrasekhar’s talk summarizing the results of his ‘generalized standard model’ of stars with degenerate cores (Chandrasekhar, 1935b), Milne made a short comment—as the previous ‘usual standard model’ was his own—and then the President of the Society invited Eddington to speak. Eddington began his talk by saying:[1008]

> Dr. Chandrasekhar has been referring to degeneracy. There are two expressions commonly used in this connection, “ordinary” degeneracy and “relativistic” degeneracy, and perhaps I had better begin by explaining the difference. They refer to formulae expressing the electron pressure P in terms of the electron density σ. For ordinary degeneracy $P_e = K\sigma^{5/3}$. But it is generally supposed that this is only the limiting form at low densities of a more complicated relativistic formula, which shows P varying as something between $\sigma^{5/3}$ and $\sigma^{4/3}$ at the highest densities. . . .
>
> Chandrasekhar, using the relativistic formula which has been accepted for the last five years, shows that a star of mass greater than a certain limit M remains a perfect gas and can never cool down. *The star has to go on radiating and contracting and contracting and radiating until, I suppose, it gets down to a few km. radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace*. (See Wali, 1991, p. 125)

[1008] Eddington’s paper on ‘Relativistic Degeneracy’ was published before the two papers of Chandrasekhar (1935a, b) in the *Monthly Notices* (Eddington, 1935). The wording of his talk on 11 January 1935, is quoted from the report in the journal *Observatory*.

Eddington then went on to state that he felt that this result was wrong and '*I think there should be a law of Nature to prevent a star from behaving in this absurd way!*' However, he admitted: 'If one takes the mathematical derivation of the relativistic degeneracy formula as given in astronomical papers, no fault is to be found.' Hence, one had to look 'deeper into its physical foundation,' he continued, and arrived at the conclusion: 'The current formula is, based on a partial relativity theory,' and 'if the theory is made complete the relativity corrections are compensated, so that we come back to the "ordinary" formula.' (Wali, *loc. cit.*, pp. 125–126)[1009]

Evidently, Eddington's presentation and mathematical arguments stunned everybody, but Professor Stratton, the Chairman of the meeting, did not allow any discussion. Chandrasekhar was extremely unhappy about the whole procedure, and even more so about the reaction of his colleagues. Perhaps he could have understood that Milne was euphoric about Eddington's conclusion, since he now felt 'his own idea that every star had an adequate core must be valid' (see Wali, 1982, p. 6), but the others simply remained silent. Back in Cambridge, even Fowler did not take Chandrasekhar's side strongly. So Chandrasekhar wrote a letter for help to Léon Rosenfeld, his friend who was now in Copenhagen:

> Yesterday I gave an account of my work at the Royal Astronomical Society and after my paper Eddington sprang a surprise on everyone by saying that the method of derivation of [Eq. (721)] was all wrong, that "Pauli's principle" refers to electrons as being stationary waves and that the use of the relativistic expression for energy is a misunderstanding.... If Eddington is right, my last four months' work all goes in the fire. Could Eddington be right? I should very much like Bohr's opinion. Please consult him on the matter as soon as you possibly can and reply to me by air mail. (Chandrasekhar to Rosenfeld, 12 January 1935, quoted in Wali, 1991, p. 129)

Rosenfeld acted quickly and wrote back immediately, reporting the results of a joint discussion of the problem between Bohr and himself. They had arrived at the conclusion that the exclusion principle could be applied both to electrons represented by standing or progressive waves in a given volume: 'These two cases become equivalent in the limit, considered by you, of an (asymptotically) infinite volume, and both yield ... precisely the expression you have used in your equation,' and 'further this expression is relativistically invariant.' (Rosenfeld to Chandrasekhar, 14 January 1935, *loc. cit.*)[1010] In a second letter on the same day, Rosenfeld added: 'It seems to us as if Eddington's statement that several high speed electrons might be in one cell of the phase space would imply that to another observer several slow speed electrons, in contrast to Pauli's principle, would be in the same cell.' (Rosenfeld to Chandrasekhar, *loc. cit.*, p. 130) Chandrasekhar, however, wanted more; after Eddington presented another talk on his interpreta-

[1009] For details of the mathematical derivation, we refer to Eddington, 1935.

[1010] Chandrasekhar would carry out later the details of the calculation together with Christian Møller from Copenhagen (Møller and Chandrasekhar, 1935).

tion of relativistic degeneracy in the Cambridge colloquium, he got hold of the latter's manuscript and forwarded it to Rosenfeld, requesting for 'an authoritative pronouncement' from Niels Bohr. Being quite exhausted from other work, Bohr passed it on to Wolfgang Pauli, who simply declared Eddington's arguments to be 'wishful thinking, in his attempt to fit the exclusion principle to what *he* wanted in astrophysics,' and Paul Dirac expressed a similar opinion to Chandrasekhar from Princeton (see Wali, *loc. cit.*, pp. 131–132). But the older generation of astrophysicists remained unshaken. Milne even wrote to Chandrasekhar:

> Your marshalling of authorities such as Bohr, Pauli, Wilson, etc., impressive as it is, leaves me cold. If the consequences of quantum mechanics contradict very obvious, much more immediate, considerations, then something must be wrong either with the principles underlying the equations of state derivation or with the aforementioned general principles.... To me it is clear that matter cannot behave as you predict. (Milne to Chandrasekhar, 24 February 1935, quoted in Wali, *loc. cit.*, p. 132)

The opinions of Milne and Eddington influenced the members of the professional community at the Paris meeting of the International Astronomical Union in July 1935, where Eddington again declared Chandrasekhar's relativistic theory of white dwarfs to be wrong. The quantum physicists did not have a strong voice in this community, even when they became active—unlike Bohr, Dirac, and Pauli, who showed no interest in the details of the astronomical problem—as did Rudolf Peierls (then at the Mond Laboratory in Cambridge).[1011] As Peierls recalled: 'I did not know any physicist to whom it was not obvious that Chandrasekhar was right in using relativistic Fermi-Dirac statistics, and who was not shocked by Eddington's denials of the obvious,' and added:

> It was therefore not a question of studying the problem, but of countering Eddington.
>
> It was for this purpose that I wrote my paper in the *Monthly Notices* [Peierls, 1936]. The simplest way to derive the equation of state is to use cyclic boundary conditions, which allow the use of progressive waves. This was one of the points criticized by Eddington, and therefore I looked for a simple proof that, for a large enough system, the use of the cyclic boundary condition was justified. (Peierls to Wali, 5 May 1983, quoted in Wali, *loc. cit.*, p. 135)

Needless to say, Eddington remained unconvinced and repeated his opinion on relativistic degeneracy as late as August 1939 (at the 'Conference on White Dwarfs and Supernovae' in Paris). But, on this occasion, Chandrasekhar, now a well-established professor of astronomy at the University of Chicago and author of the monograph on *An Introduction to the Study of Stellar Structure* (1939), was

[1011] Ralph Fowler, in spite of his general support for Chandrasekhar, nevertheless added in the second edition of his *Statistical Mechanics* (in Section 16.34) a reference to Eddington's arguments (see Fowler, 1936, p. 652, footnote II). He avoided taking a clear stand against Eddington, the famous astronomer.

allowed to present his different conclusions in detail. We shall come back to these later; here, we just mention the fact that Eddington, the previous champion of relativity theory, kept on battling against 'the widespread misapprehension as to the application of the Lorentz transformation to quantum theory,' as found in 'the literature of modern atomic physics, and in conversation with theoretical physicists'—thus, he had written a little earlier (Eddington, 1939, p. 186). He especially criticized two procedures:

> In most quantum investigations with a practical application the coordinates are relative space coordinates ξ, η, ζ coupled with a progressive time coordinate, so that (ξ, η, ζ, it) is not a 4-vector. Nevertheless, conditions of Lorentz-invariance are applied by many authors. Alternatively, they attempt to base the investigation on wave functions of non-relative coordinates x, y, z, t. It is here pointed out that such wave functions give no information about eigenstates, and that there is no means of deriving wave functions of ξ, η, ζ from those of x, y, z. (Eddington, *loc. cit.*, pp. 193–194)

A couple of years later, Dirac, Peierls, and M. H. L. Pryce gave a detailed and careful answer to the above arguments of Eddington. The physicists showed in particular that the dynamics of atomic systems did allow one to perform correctly all of the procedures he had criticized. Thus, they simply confirmed the general opinion of the community of physicist, namely: 'Eddington's system of mechanics is in many important respects completely different from quantum mechanics.' (Dirac, Peierls, and Pryce, 1942, p. 193). Eddington, thus attacked, protested: 'The [quantum-mechanical] theory is perhaps more self-consistent than it appeared to be; but, on the other hand, the pressing need for amendment becomes too plain to be overlooked.' (Eddington, 1942, p. 201)

The astronomical problems addressed in the dispute between Chandrasekhar and Eddington—in spite of their fundamental importance for the application of relativistic quantum mechanics—represented only one aspect of a larger field of physics which Friedrich Hund treated in the mid-1930's as 'matter in extreme conditions' (*Materie in extremen Zuständen*') or 'matter under very high pressures and temperatures' ('*Materie unter hohen Drucken und Temperaturen*'). This was the entry in Hund's diary (*Wissenschaftliches Tagebuch*), dated 22 January 1936, or the title of a review article composed that summer and published in *Ergebnisse der exakten Naturwissenschaften* (Hund, 1936b). Hund approached this field—which embraced the terrestrial problems investigated by Percy Bridgman (whom he knew personally from his visit to the United States in 1929) and the astrophysical issues discussed by Chandrasekhar, Eddington, and Milne—first in a note in his diary, dated 19 October 1935, where he stated: 'The question concerning the state of the earth's interior is identical with the question concerning the equation of state in a region, which should be treated according to the Thomas-Fermi method.' Very probably, Hund was stimulated to embark upon this topic by a remark which John Slater and Harry Krutter had made at the end of their paper on 'The Thomas-Fermi Method for Metals,' namely:

> One further field in which the method might be advantageous is investigating the limiting behavior of matter under high pressure, as is found particularly in astrophysics. Stellar material, either at low temperature and very high density as in dense stars, or at high temperature or more normal density, as in hot stars, could be approximated as in the present paper, and a much better approximation to the equation of state could be found than has been so far obtained. (Slater and Krutter, 1935, p. 568)

The steps taken by Hund, who especially developed a programme in 1936 to cover the above-mentioned field, may be easily recognized by looking at the brief entries in his diary (*Tagebuch*). There, we find in particular:

22.1. [1936]. Matter in extreme states (*Materie in extremen Zuständen*): For a large domain an ideal gas of electrons.

12.2–17.2 [1936]. Fruitless efforts to establish the transition from Slater-Krutter to the electron gas.

18.2. [1936]. The transition from Slater-Krutter to the electron gas is possible.

25.2. [1936]. Stellar structure at $T = 0$ with the equation of state $\rho = \rho_0$ $(p \leq p_1)$, $\rho = \varepsilon p^{3/5}$ $(p \geq p_1)$ leads to a universal differential equation with fixed initial values in the inner region $p \geq p_1$ and to a closed solution in the outer region.—To obtain the structure of cold stars, one has to achieve: numerical transition from Slater-Krutter to the electron gas plus a numerical solution of the differential equation.

16.3. [1936]. With $\rho \sim p^{3/5}$ alone [there follows]: mass times volume = constant (Flügge also got this).

27.3 [1936]. Domains of states: dense matter, electron gas, ordinary gas, condensate, radiating cavity.

15.4. [1936]. Cold stars have a relation $R(M)$, where R is not larger than several earth radii [and M denotes the mass]. Numerical calculations [can be] carried out with the help of [Robert] Emden's equation. The large planets do not satisfy the $R(M)$ relation, among the white dwarfs only van Maanen's star does.

The state-diagram still contains the relativistic electron gas and the region of nuclear transformations ($T > 10^{10}$ [K]; $p > 10^{24}$ atmospheres). The stellar energy can only arise from $T > 10^{10}$ [K], otherwise the densities must be very large.

17.4. [1936]. Estimate with a polytropic change of state $p \sim \rho^{(n-1)/n}$, and observed radii and masses yields only for n (nearly 5) sufficiently high temperatures in the centre, especially for giant stars. In the main sequence, the more luminescent stars seem to have a smaller pressure in the centre.

18.4. [1936]. Nuclei consisting of many heavy particles dissociate already below the temperatures corresponding to the dissociation potential.

22.4. [1936]. Energy production in equilibrium (of nuclear transformations) corresponds to $T > 10^9$ [K] or $p \geq 10^{20}$ atmospheres.

25.4. [1936]. The influence of [opacity] κ and η (energy production); increase of η with T reduces n; decrease of κ with T increases n. Empirically $n = \frac{1}{2}$ to 5 fit [the data], $\kappa\eta$ decreases more slowly than T^{-2}.

Draft of a report on matter in stars:

1. Diagram of states, energy constant;
2. Cold star;

3. First orientation with polytrope, large n, conclusion for $\kappa\eta$;
4. Meaning of the ML [mass-luminosity] relations.

29.4. [1936]. Similarity law for stellar structure with $p \sim \rho T$; $\kappa \sim \rho/T^{\nu}$; $\varepsilon \sim T^2$ yields two LRM-relations. Empirical LM-relation with $\nu = 3$; empirical MR-relation with large λ.

1.5. [1936]. A theory with an ideal gas must assume that $\varepsilon \sim 1/p^3$ (!), in order to understand the main parts of the [Hertzsprung-] Russell diagram.

19.5. [1936]. (Talked in the colloquium about the constitution of stars.)

27.5. [1936]. The energy "pairs" contribute at most some hundredths of the total energy of matter, and do so only if $kT \approx mc^2$.

23.6. [1936]. (Colloquium on MLR-relation of stars.)

8.7. [1936]. If for temperatures below that of nuclear dissociation the pressure surpasses a certain limit ($\approx 10^{27}$ atmospheres), then matter transforms into neutrons.

9.7. [1936]. In the region, where also the heavy particles are degenerate, the situation is more complicated.

10.7. [1936]. The correct discussion yields also in this region, for about 10^{22} [atmospheres], the sudden transition into neutrons. In the transition region matter is rather compressible.

11.7. [1936]. There exist three regions: neutrons dominant at high pressure (and not too high temperature); nuclei and electrons dominant at low pressure and low temperature; protons and electrons dominant at high temperature (especially at low pressure)—pair production has not been considered. For very high pressures (without forces between neutrons) [there follows] $p = \hbar c(\rho/M)^{4/3} \rightarrow \frac{1}{3}\rho c^2$; hence matter is very compressible. Chandrasekhar's catastrophe can only be avoided by the emission of liberated gravitational energy at the surface.

18.7. [1936]. For very high pressures the general equation of state is $p = \frac{1}{3}\rho c^2$. Domains, where forces between particles or pair creation play a role, extend only to a very small extent. Also, the proton region is small.

22.7. [1936]. In the electron gas, the electrical conductivity is at least as large as a metal having the same temperature. [D. S.] Kothari's calculation must be supplemented, for small pressures and lower temperatures, since there occurs an ordering of nuclei.

23.7. [1936]. With the help of this conductivity, the same opacity is obtained phenomenologically as by [R. C.] Majumdar.

26.7.–7.8. [1936]. Report on matter under high pressures and temperatures (*Materie unter hohen Drucken und Temperaturen*) composed for the *Ergebnisse*. (Hund, *Tagebuch*)

The topics sketched here were organized by Hund in his review article for the *Ergebnisse* (1936b) into three parts: Part I dealt with the equation of state; Part II treated additional physical properties (such as energy content, electrical and thermal conductivity, the absorption of light, and the energy transport); and Part III described the behaviour of matter in planets and stars, where especially high pressures and temperatures were expected to exist. As Hund emphasized in the introduction, in terrestrial laboratories, matter could so far only be studied at temperatures under some 1,000 degrees (K) and pressures up to 10,000 atmospheres; hence, one had to turn to celestial bodies if one wanted to observe more extreme situations. On the other hand, he also noticed that:

> Physics today is able to provide ample knowledge about the behaviour of matter under such [extreme] conditions. In these domains, in which matter may be assumed to consist of atomic nuclei and electrons, the laws of its constitution are completely known. Further, we are certain that these laws are still valid for a while when one pushes into regions of more extreme pressures and temperatures, where atomic nuclei do not remain unaltered; today's physics does not yet know completely the forces that exist between the nuclear constituents, though the energy values of many [nuclear] states depending on these forces have been revealed. (Hund, 1936b, p. 190)

In approaching the physics of the interior of stars, where the highest temperatures and pressures observed in nature dominated, Hund could indeed count on the validity mainly of the fundamental principles and laws of quantum mechanics and relativity theory to explain the astronomical observations, notably, also on the application of Coulomb's law between the charged constituents of matter down to distances of the order of nuclear radii (i.e., 10^{-13} cm) and on Pauli's exclusion principle for the statistical behaviour of electrons and nuclear constituents. In addition, he made use of results derived by other physicists on the various properties of condensed matter—e.g., of 'A Note on the Transport Phenomena in a Degenerate Gas' by D. S. Kothari, who had dealt with these phenomena in a degenerate gas, with the goal of applying the results to stellar conditions (Kothari, 1932)—or to atomic nuclei—in particular, he cited the papers such as those of George Gamow (1928a) and Fritz Houtermans (1930)—or to radiation processes and related physical topics.[1012] Hund derived his astronomical knowledge from the most recent literature; he especially cited the papers of Eddington, Milne, and Chandrasekhar (most of which we have mentioned in the foregoing). In the review for the *Ergebnisse* and in a later brief report at the 12th *Deutsche Physikertagung* in Bad Salzbrunn (Hund, 1936c—which was confined to the problem of the equation of state), Hund did not attempt to derive new laws but rather demonstrated how the known quantum-mechanical description, eventually augmented by relativistic features, of atoms and atomic nuclei and their constituents provided 'a basis for the theoretical treatment of the behaviour of matter under non-terrestrial high pressures and temperatures up to a limit lying several orders of magnitude higher than the existing one—based on our knowledge—in stars, and up to a temperature limit exceeding several orders of magnitude than those existing in the interior of stars.' (Hund, *loc. cit.*, p. 853) For this purpose, he developed special approximation methods in order to interpolate between the various idealized formulae previously derived and to fit the available (terrestrial and celestial) data. Thus, he achieved a quite impressive, at least semiquantitative, complete overview of the essential features of matter under extreme conditions.

The main goal achieved by Hund in his review article was the systematic organization of all experimental and theoretical information required to establish different domains characterized by the specific behaviour of condensed matter, as

[1012] Hund also referred to the Göttingen doctoral thesis publication of his assistant Siegfried Flügge on the role of neutrons in the structure of stars (Flügge 1933).

dependent on pressure (or density) and temperature. Thus, by carefully considering the various equations of state proposed until then—from the ideal gas equations (for classical and quantum-mechanical particles, especially those obeying Fermi statistics) to the ultrarelativistic degenerate gas equation employed by Chandrasekhar, the corresponding equation for neutrons, and the situation involving nuclear reactions and electromagnetic radiation of all frequencies—Hund arrived at the following conclusions:

> For lower temperatures and pressures, we obtain the usual condensed (solid or fluid) state; for higher temperatures, we arrive first at a gas consisting of molecules or atoms, and then at an ionized gas of electrons and atomic nuclei, and finally the radiation proportional to T^4 dominates.... If we pass over from the usual condensed state to higher pressures, then at first the compressibility is low. The (zero-point) energy of the electron states ... notably rises strongly for higher pressures. When we have arrived at bodies consisting not of atoms but of electrons and atomic nuclei (i.e., [at pressures] beyond 10^8 atmospheres), the compressibility rises to [that of] a degenerate electron gas [which is] continuously connected with both the usually condensed state and with the nondegenerate electron gas of highly ionized matter. Now the ideal electron gas containing atomic nuclei constitutes the state of matter within a wide domain of pressure and temperature—as a nondegenerate, a degenerate, or (beyond 10^{17} atmospheres) as a relativistically degenerate gas. This fact leads to a great uniformity and simplicity of the behaviour of matter [in that wide domain]: for low densities, the domain is limited by the gas becoming nonideal because of the Coulomb forces, which ultimately results in the formation of atoms in the gas or to the condensed state; for high densities it may be limited by the increase of non-Coulomb forces (responsible for the nuclear structure) between the particles, which may give rise to some sort of van der Waals transition. But before one reaches such high densities ... because of the high zero-point energy of electrons, it becomes favourable above 10^{23} atmospheres if matter transforms into neutrons, hence electrons catch protons from nuclei and unite to form neutrons. Thus we get into the region of an ideal gas of neutrons, being either degenerate [for lower temperatures] or nondegenerate [for higher temperatures]. In the case of still higher pressures (say, above 10^{23} atmospheres), we are not certain anymore whether the forces between neutrons can be neglected. (Hund, 1936c, p. 853)

Hence, the whole domain to which modern physics could be applied to determine the properties of matter extended up to pressures of about 10^{26} atmospheres and temperatures of 10^{12} K.

What Hund expressed in his lecture at the physics meeting of September 1936 in Bad Salzbrunn as a grand vision, he displayed in Part I of his article in the *Ergebnisse* in greater mathematical detail. For example, he showed how for high temperatures the separation between atoms, on the one side, and mixtures of electrons, on the other side of the phase diagram, turned out to be independent of the ionization potential. Then, he derived for the transition from quenched atoms to the electron gas, *à la* Slater and Krutter, a simplified equation of state 'by putting $\rho = \rho_k$ (constant) and assuming for high pressures the relation of the electron gas [to describe the situation] such that $\rho(p)$ remains continuous,' and noted:

'The transition depends sensibly on the temperature only if the electron state is not degenerate anymore, i.e., if kT approaches the atomic energy values.' (Hund, 1936b, p. 196) He further considered the situations at the borderline between nondegenerate electron gas and the nonrelativistic degenerate electron gas, and between the nonrelativistic degenerate and the relativistic degenerate electron gases, including the triple point between these regions; he concluded these considerations by looking at the borderlines in the phase diagram between the condensate and the degenerate electron gas, and between the condensate and the nondegenerate electron gas and the related triple point. These items did not exhaust the possible states and their connections; hence, Hund proceeded to consider the case of the neutron gas, where he treated the nonrelativistic, nondegenerate, and degenerate states, on the one hand, and the relativistic state existing under extremely high pressures, on the other. Then, he discussed the phenomena in the domain of nuclear transitions in thermodynamic equilibrium and away from it; depending on the particular situation (degeneracy or not, relativistic or not), a qualitative description of those regions followed where there existed either protons and electrons or neutrons, while for temperatures smaller than R/k (with R the binding energy per nuclear constituent) nuclei played the essential role. Finally, Hund considered the influence of electromagnetic radiation and the possibility of pair-creation, which occurred only at higher temperatures.

Starting from the various equations of state, Hund turned in Part II in his review article in the *Ergebnisse* to derive conclusions concerning certain physical properties of matter under unusual conditions, such as energy content, electrical and thermal conductivities, opacity (i.e., the absorption of light), and the energy transfer. All of these properties entered crucially in the physical description of the internal structure of celestial bodies (which he sketched in Part III), from the planets to the ordinary stars (on the so-called main sequence, like the sun), including the relation between their radius, mass, and luminosity. In particular, he found: 'It seems that the empirical mass-luminosity relation of the usual stars constitutes an expression for the far-reaching uniformity of stellar matter with respect to the equation of state and the law of energy transfer.' (Hund, *loc. cit.*, p. 225) Finally, he presented the case of white dwarfs very much along the lines of Chandrasekhar's theory, stating in particular: 'As the *possible final state of stellar evolution* we may thus expect stars of moderate mass and very high densities.' On the other hand, he also suggested to examine the singularity derived by Chandrasekhar for stars of higher mass, and speculated: 'The stars of great mass might avoid shrinking to small radii ... by radiating away the gravitational energy produced by contraction.' (Hund, *loc. cit.*, p. 227)

In 1935, Eddington had characterized exactly this shrinkage of massive stars to more or less zero radius as the 'absurd' behaviour of stars. Chandrasekhar, on the other hand, had proposed an escape from this situation in the case that the massive star would lose as much of its matter until the limit was reached (Chandrasekhar, 1935b, p. 257). Actually, the situation was a little more complicated, as there existed two critical masses, the mass M_3, identical with the limit given in Eq. (723), and another value $\mathscr{M}(\approx 1.2M_3)$ defined earlier as follows:

> For all centrally condensed stars of mass greater than $\mathscr{M}$, the perfect gas equation of state does not break down, however high the density may become, and the matter does not become degenerate. An appeal to the Fermi-Dirac statistics to avoid the central singularity cannot be made. (Chandrasekhar, 1932, p. 324)

With the two critical masses Chandrasekhar had then discussed, in his detailed papers of 1935, the complete behaviour of stars (of arbitrary mass) in the last stages of their lives (Chandrasekhar, 1935a, b, c). At the 1935 conference of the International Astronomical Union in Paris, Chandrasekhar had not been allowed to present even a short account of these results; however, four years later, at the international 'Conference on White Stars and Supernovae' (actually, the last such meeting before the outbreak of World War II), he was invited to speak on the subject, and Chandrasekhar made a clear presentation of the whole situation:

> For stars of mass less than M_3, we can tentatively assume that the completely degenerate state represents the last stage of the evolution of stars—the state of complete darkness and extinction. These completely degenerate configurations with $M < M_3$ are of course characterized by finite radii.
>
> For $M > M_3$ no such simple interpretation is possible. The problem that we are faced with can be stated as follows:
>
> Consider a star of mass greater than $\mathscr{M}$ and suppose that it has exhausted all its sources of subatomic [i.e., nuclear] energy—hydrogen in this connection. The star must then contract according to the Helmholtz-Kelvin time scale. Since degeneracy cannot set in, in the interior of such stars, continued and unrestricted contraction is possible, in theory.
>
> However, we may expect instability of one kind or another (e.g., rotational) to set in long before, resulting in the "explosion" of the star into smaller fragments. It is also conceivable that the star may decrease its mass below M_3 by a process of continual ejection of matter. The Wolf-Rayet phenomenon is suggestive in this connection.
>
> For stars of masses $M_3 < M < \mathscr{M}$ there exist other possibilities. During the contractive stage, such stars are likely to develop degenerate cases. If the degenerate cases attain sufficiently high densities (as is possible for these stars) the protons and electrons will combine to form neutrons. This would cause a sudden diminution of pressure resulting in the collapse of the star onto a neutron core giving rise to an enormous liberation of gravitational energy. This may be the origin of the Supernova phenomenon. (Chandrasekhar, quoted in Wali, 1991, p. 136)

In spite of the fact that Eddington still argued against the first scenario, Chandrasekhar's presentation would win in the future. Almost 45 years later, on 10 December 1983, he would receive the Nobel Prize for Physics 'for his theoretical studies of the physical processes of importance to the structure and evolution of stars.'[1013] Thus, the first application of relativistic quantum mechanics to decipher a central problem of celestial physics was ultimately honoured properly.

In the years following the theoretical discovery of the critical mass of white dwarfs, i.e., from 1932 to 1938, the detailed picture was found of the nuclear processes which provided the stars their energy before they reached their final states. We shall not enter here into this fundamental aspect of astrophysics, which was based on the stormy progress in nuclear theory in those days—however, certain items will be dealt with in the next section—but rather deal with some further investigations by theoretical physicists on the last stages of stellar evolution.[1014] Again, Chandrasekhar thought about the latter problem when he discussed his relativistic mass-limit theory in spring 1935 with John von Neumann, who was visiting Cambridge at that time. He recalled that von Neumann was 'rather lonely' and:

> He used to come to my rooms often. Naturally we discussed Eddington's objections. John said, "If Eddington does not like stars to recede inside the Schwarzschild radius, one probably should try to see what happens if one uses the absolute, relativistic equations of state." We started working on that together, but to go on we had to study equilibrium conditions within the framework of general relativity. Soon John left Cambridge and forgot the problem, and I got sufficiently discouraged with the situation to leave the problem alone. (Chandrasekhar, quoted in Wali, 1991, pp. 143–144)

While the Chandrasekhar–von Neumann discussions only led to (posthumously published) notes of von Neumann (1963, pp. 175–176)—which Chandrasekhar worked into his later book on stellar structure (1939, pp. 332–349)—J. Robert Oppenheimer and his students investigated several aspects of the stability problem in a set of papers published in 1938 and 1939.

In the first note, which Oppenheimer and Robert Serber submitted in September 1938 to *Physical Review*, they were interested in the source of the unusually large radiation of stars like Capella. Upon considering the possibility of obtaining energy from several nuclear processes, such as the formation of deuterons from protons or the proton capture by nuclei of elements lying between carbon and oxygen, Oppenheimer and Serber concluded 'that for these [very luminescent stars] either one would have to involve other and readier nuclear reactions, with a correspondingly reduced scale, or one would here be led, as in earlier arguments of Milne, to expect serious deviations from the Eddington model' (Oppenheimer and Serber, 1938, p. 540). In this context, they referred to the idea of a condensed

[1013] See the citation in *Nobel Foundation*, ed., 1993, p. 133.

[1014] An early consideration, dealing with the relation between a process of nuclear fusion, $^{7}\mathrm{Li} + {}^{1}\mathrm{H} \rightarrow 2\,{}^{4}\mathrm{He}$, and the internal temperature of stars was provided by George Gamow and Lev Landau (1933). Later on, especially Carl Friedrich von Weizsäcker (1937b, 1938b), George Gamow (1938a), and Hans Bethe (1939) treated the nuclear energy production in normal stars. For more details, see the next section.

neutron star, as discussed by George Gamow (1937, pp. 234–235)—and others, including Hund (see above)—and to Lev Landau's claim that the gradual growth of such a core would release enormous amounts of gravitational energy (Landau, 1938a). Landau's conclusion about a limiting lower mass for this core—by requiring that the sum of the gravitational and kinetic energies per particle of the core should be lower than the energy per particle in stable nuclei, he had derived a value of 0.001 solar mass—was now contested by Oppenheimer and Serber. They argued that the neutron's free energy in the core must be less than in the nucleus, in order to establish stability, and concluded therefrom a limiting mass of $\frac{1}{6}$ that of the sun, which they also confirmed by a rigorous evaluation of the equation of state.[1015] However, this minimum core mass was reduced to about $\frac{1}{10}$ of the solar mass if there existed 'forces between the neutrons of the spin-exchange saturating type $(\sigma\sigma')$,' and even much further to a few percent of the solar mass if another charge-independent nuclear force was assumed. Finally, Oppenheimer and Serber arrived at the result 'that forces of the often assumed spin exchange type preclude the existence of a core of stars with mass comparable to that of the sun' (Oppenheimer and Serber, 1938, p. 540).

While the above-mentioned note seemed to be motivated by Oppenheimer's interest in the nature of nuclear forces, he approached in the following paper written with G. M. Volkoff—and submitted in early January 1939 to *Physical Review*—the detailed properties of neutron stars, 'notably the gravitational equilibrium of masses of neutrons, using the equation of state for a cold Fermi gas and general relativity.' They then found:

> For masses under $\frac{1}{3}\odot$ [i.e., $\frac{1}{3}$ of the mass of the sun] only an equilibrium solution exists, which is approximately described by the nonrelativistic Fermi equation of state and Newtonian gravitational theory. For masses $\frac{1}{3}\odot < m < \frac{3}{4}\odot$ two solutions exist, one stable and quasi-Newtonian, one more condensed and unstable. For greater masses there are no static equilibrium solutions. (Oppenheimer and Volkoff, 1939, p. 374)

That is, Oppenheimer and Volkoff now explored in some detail whether the general idea, that 'in sufficiently massive stars after all thermonuclear sources of energy, at least for the central material of the star, have been exhausted a condensed neutron core would be formed,' was 'correct for arbitrarily heavy stars'; and they did confirm here the fact that there was 'an upper limit to the possible size of the core' (Oppenheimer and Volkoff, *loc. cit.*, p. 375). Of course, it was known that Chandrasekhar and Landau had proved the existence of a similar limit before (in 1931 and 1932, respectively), but these people had derived it from the

[1015] In a talk presented at the Washington, D.C., meeting of the American Physical Society in April 1938, George Gamow and Edward Teller had claimed that stars cannot really have a core (besides an outer part in which the usual gas laws apply) because the equilibrium conditions would then give rise to densities and temperatures close to the core, which were too high and would disagree with the observed radiation (Gamow and Teller, 1938b, p. 930).

relativistic equation of state for electrons, while Oppenheimer and Volkoff now did so for stars obeying—because of their larger masses—a nonrelativistic degenerate-gas equation. Further, they replaced in this treatment the Newtonian gravitational theory by the general relativistic one.

In order to obtain the general relativistic gas equation, Oppenheimer and Volkoff considered the equilibrium of spherically symmetric distributions of matter and derived two relations between the pressure p and the density ρ; i.e.,

$$\frac{dp}{dr} = \frac{p + \rho(p)}{r(r - 2u)} [4\pi pr^3 + u] \tag{724}$$

and

$$\frac{du}{dr} = 4\pi\rho(p)r^2, \tag{725}$$

with u denoting a variable connected with Karl Schwarzschild's famous solution of the spherical problem in general relativity.[1016] They commented:

> Equations [(724)] and [(725)] form a system of two first-order equations in u and p. Starting with some initial values $u = u_0$, $p = p_0$ at $\mathrm{r} = 0$, the two equations are integrated simultaneously to the value $r = r_b$ where $p = 0$, i.e., until the boundary of the matter distribution is reached. The value of $u = u_b$ at $r = r_b$ determines the value of $e^{\lambda(r_b)}$ at the boundary, and this is joined continuously across the boundary to the exterior solution, making
>
> $$u_b = \frac{r_b}{2}[1 - e^{\lambda(r_b)}] = \frac{r_b}{2}\left[1 - \left(1 - \frac{2m}{r_b}\right)\right] = m. \tag*{[(726)]}$$
>
> Thus the mass of the spherical distribution as measured by a distant observer is given by the value u_b of u at $r = r_b$. (Oppenheimer and Volkoff, *loc. cit.*, p. 376)

To ensure the physical interpretation of the result, several restrictions (such as $p_0 = 0$ and $u_0 = 0$ at r_0) must be imposed. Now, for a Fermi gas of particles with mass μ_0, the equation of state could be written in a parametric form as (see Tolman, 1934, pp. 246–147)

$$\rho = K(\sinh t - t) \tag{727}$$

and

$$p = \tfrac{1}{3}K[\sinh t - 8\sinh(\tfrac{1}{2}t) + 3t], \tag{728}$$

[1016] Schwarzschild's solution (1916a) can be written as $\exp[-\lambda(r)] = 1 + A/r$, and then $u(r) = \frac{1}{2}r\{1 - \exp[-\lambda(r)]\}$.

with the quantities K and t defined as

$$K = 4\mu_0^2 c^5 / 4h^3 \tag{728a}$$

and

$$t = 4 \log\left\{\frac{p_{\max}}{\mu_0 c} + \left[1 + \left(\frac{p_{\max}}{\mu_0 c}\right)^2\right]^{1/2}\right\}, \tag{728b}$$

where $p_{\max}$ denoted the maximum momentum of the Fermi distribution.

The integration of Eqs. (724) and (725) had to be performed numerically for the general case. In the case of very small t, the equation of state became

$$p = K\rho^{5/3}, \tag{729}$$

with $p_{\max}$ being proportional to t. Also, the mass of the star turned out to be in good approximation (for small masses and densities) proportional to $t^{3/2}$. If they plotted the mass m in units of the solar mass against $\tan^{-1} t_0$, Oppenheimer and Volkoff found the following behaviour:

> The striking feature of the curve is that the mass increases with increasing t_0 [the value of t at the centre $r = 0$] until a maximum is reached at about $t_0 = 3$, after which the curve drops until a value roughly $\frac{1}{3}\odot$ is reached for $t_0 = \infty$. In other words, no static solutions at all exist for $m > \frac{3}{4}\odot$, two solutions exist for all m in $\frac{3}{4}\odot > m > \frac{1}{3}\odot$, and one solution exists for all $m < \frac{1}{3}\odot$. (Oppenheimer and Volkoff, 1939, p. 378)

Expressed physically, these results meant: (i) For 'a cold neutron core there are no static solutions, and thus no equilibrium, for core masses greater than $m \approx 0.07\odot$'; (ii) 'Since neutron cores can hardly be stable (with respect to the formation of electrons and nuclei) for masses less than $m \approx 0.1\odot$, and since, even after thermonuclear sources and energy are exhausted, they will not tend to form by collapse of ordinary matter for masses below $1.5\odot$ (Landau limit), it seems unlikely that static neutron cores can play any great part in stellar evolution.' (iii) 'The question of what happens, after energy sources are exhausted, to stars of mass greater than $1.5\odot$ still remains unanswered.' (iv) While for masses between 0.1 and $0.7\odot$ the stability of neutron cores seemed to be established, the final behaviour of massive stars was either not described by the equations of Oppenheimer and Volkoff, or 'the star will continue to contract indefinitely, never reaching equilibrium' (Oppenheimer and Volkoff, *loc. cit.*, pp. 380–381).

In July 1939, the *Physical Review* received the account of Oppenheimer's next investigation, this time composed jointly with Hartland Snyder, which was devoted specifically to finding out about the behaviour of stars with masses greater than $0.7\odot$. They started from the following previous results:

> A star under the circumstances would collapse under the influence of its gravitational field and release energy. This energy could be divided into four parts: (1) kinetic energy of the motion of particles in the star; (2) radiation; (3) potential and kinetic energy of the outer layers of the star which could be blown away by the radiation; (4) rotational energy which could divide the star into two or more parts. (Oppenheimer and Snyder, 1939, p. 455)

Only in those cases where the mass of the original star was sufficiently small, or enough mass was lost to reach the limit of about 0.7 solar masses, a white dwarf (in agreement with Chandrasekhar's pioneering work) would develop. For the more massive stars, Oppenheimer and Snyder carried out the calculation of a slightly simplified model, obtaining the result: 'The total time of collapse for an observer comoving with the stellar matter is finite, and for this idealized case of typical stellar masses, of the order of a day; an external observer sees the star asymptotically shrinking to its gravitational radius.' (Oppenheimer and Snyder, *loc. cit.*) 'Of course, actual stars would collapse more slowly than the example which we studied analytically because of the pressure of matter, of radiation, and of temperature,' they concluded (Oppenheimer and Snyder, *loc. cit.*, p. 459)

The work of Oppenheimer and his collaborators in Berkeley by no means exhausted the interest of quantum theorists in the problems of astrophysics toward the end of the 1930's. For example, Hund returned in his *Tagebuch* entries to the problems of stellar structure, in connection with his lecture courses on '*Aufbau der Materie* (Structure of Matter)' of fall 1939 and winter 1939/40. From October 1939 to May 1940, he made notes dealing with the properties of the normal main-sequence stars, then of variable stars (like the Cepheids), and finally of stars whose nuclear energy had been exhausted. However, in spite of these efforts of the theoretical physicists, it seems that the professional astronomers only reluctantly accepted the results of the quantum physicists seeking to invade their domain; as in the case of Chandrasekhar's ingenious efforts of 1931, it took decades until the impressive consequences of quantum mechanics were considered standard knowledge in astrophysics.

IV.5 High-Energy Physics: Elementary Particles and Nuclear Reactions (1932–1942)

(a) Introduction

In the beginning of the 1930s, Werner Heisenberg and the other experts on quantum mechanics had claimed that the phenomena of nuclear and relativistic physics, i.e., the physics of highest energies, were intimately connected and had to be solved—if at all—together. This was before the discovery of the neutron inaugurated a quick change in the physicists' desire to establish a consistent description of nuclear structure on the basis of the usual laws of nonrelativistic quantum mechanics. No such satisfactory result could be achieved in other fields of phenomena, notably, in cosmic radiation, though they also appeared to be related—like the nuclear ones—to the innermost structure of matter. Thus, at the 'International Conference on Physics' in London in October 1934, Patrick M. S. Blackett of Birkbeck College—in the introduction of his talk on 'The Absorption of Cosmic Rays'—stated:

> One of the main difficulties which stand in the way of a satisfactory interpretation of the phenomena of cosmic radiation lies in our ignorance of the exact mechanism of the absorption of photons and charged particles of very great energy.
>
> In fact, it is only through the study of cosmic rays that we can hope to learn about the properties of very energetic radiations. But since the experimental phenomena of cosmic rays are both complicated and hardly at all under the experimenter's control, it is by no means easy to find their correct interpretation. For to do this implies the analysis of the complex radiation into simpler constituents and then the decision as to the nature and properties of the various radiations.
>
> Unfortunately we cannot get much help in this process from theoretical physics for there seems to be no theory which is certainly valid for particles and photons of very great energy. While it is quite certain that, in the cosmic radiation, we have to deal with particle energies of the order of 10^8 to 10^{11} e.V. (electron volts), it seems nearly equally certain that the only existing theory, that of Dirac, is only valid for energies less than 137 mc^2, that is about 7×10^7 volts. (Blackett, 1935, pp. 199–200)

Blackett, the former research student of Ernest Rutherford's and co-discoverer of pair creation, here expressed clearly the experts' knowledge at that time. His report and the reports of the other cosmic-ray physicists who were assembled at the London conference, such as Pierre Auger and Louis Leprince-Ringuet of France, Gerhard Hoffmann of Germany, Bruno Rossi of Italy, and Carl Anderson, Arthur Compton, Robert Millikan, and Seth Neddermeyer of the United States, emphasized the existence of at least two different components in the extra-terrestrial radiation, a highly absorbable 'soft' and a penetrating 'hard' component, for both of which only very preliminary physical interpretation had been suggested so far. While the soft component, consisting of a group of from a few up to a few hundred positively and negatively charged electrons, seemed to be connected with the known process of electron–positron pair creation, the hard component—consisting mainly of single tracks and particles with low ionizing power—more or less lacked any explanation.

The experimental investigation of cosmic radiation actually constituted the most important source of fundamental knowledge of high-energy phenomena in the 1930s and even beyond.[1017] The theoreticians encountered many difficulties when they wanted to describe the empirical findings, because the cosmic-ray events were rare and contradictory in appearance and hard to interpret in terms of the physical quantities that entered into any mathematical formulation of the observed phenomena. In contrast to that, the nuclear reactions observed in ter-

[1017] For a review, see the papers of Weiner, 1972, Brown and Hoddeson, 1982, Xu and Brown, 1987, and Brown and Rechenberg, 1996 (Chapter 4).

restrial laboratories—although they referred to much lower energies—permitted a more definite analysis, yielding in most cases a reliable description in terms of a quantum-mechanical formalism. Hence, the theory of nuclear structure and nuclear reactions could be developed largely in the 1930s, but the understanding of cosmic-ray processes with elementary particles having much higher energies remained largely selective. Still, some progress in applying the rules of relativistic quantum theory to certain events, especially those involving electron–positron pair creation and mesotrons and their decays, could be reached only by the end of the 1930s, while other grave puzzles had to wait for many more years before they finally got resolved.[1018]

During the two years following the London conference, the theorists—especially Walter Heitler in England and J. Robert Oppenheimer in the United States—succeeded in obtaining a theory of the principal soft-component process, the cascade formation, which provided a confirmation of the quantum-electrodynamical schemes of Heisenberg and Pauli or Fermi, respectively. On the other hand, the increasingly detailed analysis of the hard component resulted not only in the discovery of a new intermediate-mass particle, the 'mesotron' (which had been predicted by Hideki Yukawa's theory of nuclear forces treated in Section IV.3), but also gave rise to a number of new puzzles. In this section, we shall first deal with the progress of quantum electrodynamics in the 1930s, achieved both through the description of certain cosmic-ray phenomena (such as cascades) and the mathematical and physical analysis of the existing formalism. The remaining defects of that particular relativistic quantum field theory, also indicated by specific low-energy observations, evidently required the input of new, more or less revolutionary, concepts and methods to account for the high-energy processes involving only the electromagnetic interaction of matter. The puzzles of the hard component considered next posed even tougher dilemmas for their theoretical understanding: In particular, what was the nature of particles participating in the processes in question, and by what fundamental interactions and wave equations had they to be described? In the second half of the 1930s and beyond, many of the leading quantum physicists employed great skill and imagination in dealing with these matters; although satisfactory answers could not be obtained for any of these fundamental questions, their ideas and suggestions served to lay the foundation of most of the concepts of the future theories of elementary particles.

Already at the 1934 London conference, in his opening remarks, Ernest Rutherford presented a much brighter view of the situation existing then in nuclear physics (Rutherford, 1935). After quickly sketching the history of the field from the discovery of radioactivity until 1933, Rutherford continued:

> The rapidity of advance in the last few years has been in large part due to the great improvement in the technical methods of attack. Largely due to the work of [H.]

[1018] For a review of the modest progress in quantum field theory, which accompanied these cosmic-ray researches until 1947, see Wentzel, 1960.

Geiger, [H.] Greinacher, [C. E.] Wynn-Williams and others, we have now available simple and reliable methods for automatically counting swift particles like α-particles and protons. The sensitive Geiger-Müller tube counters have proved of the utmost value in the study of the cosmic rays and in investigating the production of radioactive bodies by artificial methods, and science owes a great debt of gratitude to C. T. R. Wilson for the invention of that wonderful instrument, the expansion chamber. This has been proved a powerful method for investigating the nature of the cosmic rays and the transformation of elements. In many cases it affords in a sense a final court of appeal by which the validity of our explanations can be judged. (Rutherford, 1935, p. 14)

The experimental methods emphasized here actually played a vital role in examining the nature and particle content of both the soft and the hard components of cosmic radiation, in the first place, the Geiger–Müller counter in coincidence and anti-coincidence circuits and the Wilson cloud chamber operating in the field of strong magnets (for determining the energy and velocity of charged particles). However, they also did so in the study of nuclear reactions and transformations, for which in the early 1930s, several new machines were constructed. As Rutherford summarized in London:

The use in the laboratory of high voltages of the order of a million volts to accelerate [charged nuclear] projectiles has raised many difficult technical as well as financial problems. We owe much to those pioneers like [W. D.] Coolidge, [T. E.] Allibone, [M. A.] Tuve, [C. C.] Lauritsen, [Arno] Brasch and [Fritz] Lange and others who have opened up these new methods of attack. Progress in this direction would have been very difficult if not impossible but for the invention of fast diffusion pumps in which [W.] Gaede was the pioneer. The invention by Van der Graaf [*sic*] of a new type of electrostatic machine for the production of very high voltages may prove of much more importance for the future. We must not omit to mention our appreciation of the skill of Lawrence in developing to a successful issue his method of multiple acceleration which has given us the fastest particles so far generated in the laboratory. (Rutherford, *loc. cit.*)

The new artificial accelerators for charged particles, notably, the electrostatic voltage accumulator of Robert J. Van de Graaff and the cyclotron of Ernest Orlando Lawrence, as well as the voltage multipliers of John Cockcroft and Ernest T. S. Walton (according to a method first suggested by Heinrich Greinacher), have been mentioned already (in Section IV.3). From 1934 onward, every modern laboratory of nuclear physics, especially in the USA, the home of Van de Graaff and Lawrence, but also in Europe, would acquire one of these new particle accelerators to perform nuclear transformations, though the energies thus available amounted only to a few MeV, i.e., much less than observed in cosmic-ray particles. Still, the projectiles produced in terrestrial laboratories exhibited a great advantage, as their nature was known and they could be created in regular (though normally of small intensity) beams of a more or less sharply defined energy. The artificially achieved projectile energies did not suffice to produce new particles; hence, cosmic radiation remained during the subsequent fifteen years the unique source of mesotrons and heavier particles; however, they made it possible to study nuclear processes and the strength and properties of nuclear forces systematically.

Besides the action of fast-charged particles, which could penetrate through the Gamow potentials of nuclei, nuclear transformations could also be initiated by the neutral nuclear particle—the neutron—discovered by James Chadwick in 1932, because it was able to sneak easily through the Coulomb potential of even the heaviest (and most charged) atomic nucleus. When, in spring 1934, several physicists in Rome—under the leadership of Enrico Fermi—began to investigate the neutron-induced nuclear transformations, they soon observed that the low-energy neutrons (i.e., the neutrons slowed by collisions with the light hydrogen nuclei contained in paraffin wax) were especially effective in creating new elements. The joint efforts of chemists and physicists, especially in France and Germany, to analyze the results of neutron–nuclei reactions, in particular, the many new substances and isotopes that were detected, led by late 1938 to the discovery of another type of radioactivity exhibited by the heaviest chemical elements: nuclear fission. This discovery greatly surprised the experts in nuclear theory, who—from their quantum-mechanical approach—had previously excluded such a process; but they quickly managed to generalize their standard liquid-drop model of nuclei to yield also the splitting of uranium nuclei by slow neutrons into two, approximately equally heavy, nuclear fragments. Since in the fission process more neutrons were liberated than absorbed, it opened for the first time the door to exploit nuclear energy on a large technical scale, if a chain reaction could be achieved. World War II, which was started in September 1939 by Adolf Hitler's Germany, strengthened the efforts both to obtain useful nuclear energy, both by constructing a critical reactor—i.e., a device in which the fission process is self-sustained by the chain reaction and produces power continuously—and a super-powerful atomic weapon—i.e., a bomb containing critical amounts of the uranium isotope U235 or the transuranic element plutonium, which were to react in an explosive manner. The technical development of nuclear energy during World War II (and beyond) must therefore also be regarded as an immediate outcome of the combined experimental and theoretical work of the quantum physicists in the 1930's. In addition, they investigated, though only purely theoretically, another source of nuclear energy existing in nature: the process of nuclear fusion, primarily of lighter atomic nuclei, which should occur in the stars and provide the vast quantities of energies radiated away by the celestial bodies in the course of billions of years.

(b) Between Hope and Despair: Progress in Quantum Electrodynamics (1930–1938)

In an overview of the development of quantum electrodynamics (*QED*) given at the 'International Symposium on the History of Particle Physics' at the Fermilab in May 1980, Victor Weisskopf characterized the main contributions to the field in the 1930s under four headings (see Weisskopf, 1983, pp. 68–75):

> The fight against infinities: (I) elimination of vacuum electrons; (II) infinities on the attack; the infinite self-mass; (III) infinities on the attack; the infinite vacuum polarization; (IV) counter attack; renormalization.

Being himself quite an active participant in the enterprize, Weisskopf suggested a more or less steadily proceeding evolution toward the goal of achieving a consistent theory of electrons, photons, and the electromagnetic interaction, especially placing: in (I) the investigations of J. Robert Oppenheimer and Wendell Furry; in (II) his work on the self-mass of the electron, as well as that of Felix Bloch and Arnold Nordsieck on the infrared divergence; in (III) the investigations of Werner Heisenberg, Hans Euler, and himself, as well as those of Robert Oppenheimer's collaborators Robert Serber and Edwin Uehling on the dielectric properties of the vacuum; and in (IV) the first indications of the future renormalization scheme, both in the experimental work of certain spectroscopists and in the theoretical investigations, especially those of Hendrik Kramers. The historical accounts given by Abraham Pais (1986, especially, Chapter 16, entitled 'Battling the Infinite,' pp. 360–392) and Silvan S. Schweber (1994, Section 2.2, pp. 76–129) reveal a more complex and less linear substructure in the story, stressing also the role of several personalities (or schools) and the local occurrences at various places in Europe and America (see Pais, 1986, pp. 364–370, 374–385, 388–391).

Indeed, a closer examination of the physical ideas and theoretical investigations connected in the 1930s with the field of *QED* opens a wide variety of topics, ranging from the description of observed phenomena in high-energy cosmic radiation to the consideration of fundamental theoretical concepts, such as the electron mass or the polarization of the vacuum. Of course, the situation was complicated by the fact that certain topics appeared intermingled, thereby often interrupting the historical sequence and turning the logical sequence upside down. Nevertheless, we shall attempt to assemble in the following the important aspects of the development of *QED*, which may also endow the whole story with some historical order. We shall begin with a discussion of the understanding—up to 1934—of elementary processes in cosmic radiation involving the interaction of light and charged matter; then, we shall continue with the first applications of Paul Dirac's idea of the anti-electron, as treated by Oppenheimer and his associates within the framework of quantum field theory, before turning to the new 'hole' theory of positrons inaugurated by Paul Dirac in late 1933 and early 1934, as well as its extensions expounded by Werner Heisenberg and his collaborators in Leipzig. Throughout this period, Heisenberg maintained close contact with Wolfgang Pauli in spite of their different attitudes toward the central idea of hole theory. In Zurich, Weisskopf, in particular, approached the fundamental problem of the field-theoretical mass of the electron and achieved some progress in the divergence problem (Weisskopf, 1934a, b). On the other hand, Felix Bloch, Heisenberg's former student and collaborator, by then well established in America, showed with Arnold Nordsieck how the so-called 'infrared divergence' problem of *QED* could be resolved (1937). Meanwhile, i.e., by the end of 1936, two theoretical groups—one in California and the other in England—proposed a satisfactory theory (i.e., one accounting for the latest observations in cosmic radiation processes) of the 'soft-component' cascade showers, which demonstrated the up-to-then questioned validity of *QED* for high-energy scattering. However, until 1939, a much slower and more hesitant advance occurred in the deeper-lying problems of the entire *QED*-scheme; even the experimental indications of definite deviations from the standard results in atomic spectroscopy (which had previously substantiated Dirac's equation for the electron) could not yet play a decisive role when just the first indications of the later renormalization procedure encountered other more radical proposals for abandoning the structure of the classical theory underlying a future *QED*.

In February 1932, Werner Heisenberg submitted the first of his many substantial papers on cosmic-ray phenomena in the 1930's, a lengthy investigation entitled '*Theoretische Überlegungen zur Höhenstrahlung* (Theoretical Considerations on Cosmic Radiation)' to *Annalen der Physik* (Heisenberg, 1932a).[1019] As Heisenberg wrote in the introduction, he intended 'to discuss in detail the most important experiments on cosmic radiation from the point of view of the existing theories, and to state at which points the experiments roughly agree with the theoretical expectation, and where such large deviations show up that one has to be prepared for important surprises' (Heisenberg, 1932a, p. 430). He then discussed, in particular, the deceleration of electrons when passing through matter and several typical cosmic-ray phenomena (such as those observed in the absorption curves), and he explained the existing discrepancies between theory (especially, the Klein–Nishina formula) and experiment on account of 'the failure, in principle, of Dirac's radiation theory or the equivalent quantum electrodynamics which might be applied for this purpose'—as had been noticed to be 'already a fact for other reasons' (Heisenberg, *loc. cit.*, p. 452). At about the same time, also other theoreticians in Germany turned to the discussion of the problems of cosmic radiation, among them, Walter Heitler in Göttingen. As Heitler recalled, he began to turn away from his previous principal topics of research in quantum chemistry in 1932, and moved into the field of quantum electrodynamics:

> Of course, quantum electrodynamics then represented the fundamental unsolved problem ... then I thought that high-energy phenomena would give some key to the further development of quantum electrodynamics, and so I started to work out the problem of *Bremsstrahlung* in Göttingen. Well, in my first paper about it I merely estimated the order of magnitude, and then I continued my interests in England ... after I had to leave Germany owing to Hitler's persecution. At that time then Dirac's "holes" theory appeared, and also the discovery of the positive electron.... With the work on *Bremsstrahlung* ... I could see ... that this was practically the same process: *Bremsstrahlung* and the creation of pairs. So I included the electron pairs.... Bethe joined [in] this work; then we could show that there was really perfect agreement between the experiment and the theory, thus proving Dirac's hole theory to be cor-

[1019] A general review of these papers on cosmic radiation has been given by Erich Bagge in his annotation to Group 8 in Werner Heisenberg: *Collected Works*, Vol. *AII* (Bagge, 1989).

rect. As a consequence of this I published a few more papers in Bristol, all concerned with electron pairs, with positive electron, annihilation, and various other processes. (Heitler, AHQP Interview, 19 March 1963, pp. 3–4)

Heitler submitted his first study '*Über die bei sehr schnellen Stößen emittierte Strahlung* (On the Radiation Emitted by Very Fast Collisions)' in early June 1933, still from Göttingen (Heitler, 1933). There, he found that the *Bremsstrahlung* calculated for the collisions of electrons having an energy much bigger than mc^2 (with m denoting the mass of the electron) yielded—in the first approximation—an especially large cross section of the order of magnitude $\frac{1}{137}(e^2/mc^2)^2$, in agreement with the cosmic-radiation data. On the other hand, he noticed that the application of Dirac's theory to describe these processes involved the known difficulties with negative-energy states. In particular, the exact back-coupling 'should be obtained only after the *electron radius* has been properly introduced into the theory ... [which is] the main problem of today's physics,' he remarked, and concluded: 'Whether the results of our theory are correct for normal transitions, can only be derived from a closer comparison with experience.' (Heitler, *loc. cit.*, p. 167)

Independently of Heitler, Fritz Sauter of the *Technische Hochschule* in Berlin treated the same problem, starting out from a nonrelativistic theory of the continuous X-ray spectrum (Sauter, 1933). In the following paper, entitled '*Über die Bremsstrahlung schneller Elektronen* (On the *Bremsstrahlung* of Fast Electrons),' he extended the previous theoretical approach—namely, the first Born approximation using plane waves for the incident electrons—to relativistic electrons and arrived at a detailed expression for the intensity of the *Bremsstrahlung*, J, which passed over—for extremely high primary-electron energies—into the equation

$$J = 4\alpha\left(\frac{e^2 Z}{mc^2}\right)^2 E_0\left(\log\frac{2E_0}{mc^2} - \frac{1}{3}\right), \tag{730}$$

where α denoted the fine structure constant, Z denoted the atomic number of the scattering atom, and E_0 denoted the primary energy. Hence, 'the average energy loss of an electron caused by the emission of radiation increases more strongly than linearly with energy,' he concluded (Sauter, 1934, p. 412). Sauter had pointed out the importance of Eq. (730) for the corresponding cosmic-ray process previously in a letter to *Nature* written with Heitler (Heitler and Sauter, 1933).

Heitler continued to work on the problem (as mentioned in the quotation above) with Hans Bethe, another German emigrant to England, who 'contributed mainly by taking into account the qualitatively important screening effects' (Heitler, AHQP Interview, 19 March 1963, p. 4). In their extended paper 'On the Stopping of Fast Particles and on the Creation of Positive Electrons,' which Paul Dirac communicated to the *Proceedings of the Royal Society* in February 1934, Bethe and Heitler arrived at a complicated expression which replaced Sauter's Eq. (730): In particular, the screening effect noticeably raised the increase of the loss of intensity for large energies above the $E_0 \log E_0$ dependence, a result which had to be correct quantitatively for light scattering nuclei and qualitatively correct (because of the errors involved in the Born approximation) for heavy nuclei (see Bethe and Heitler, 1934, pp. 96–97).

These obviously quite reliable deductions from the standard quantum electrodynamical theory (up to 1934) had now to be compared with the latest high-energy data from cosmic radiation, and a good opportunity for doing so arose at the International Conference on Physics held in London in October 1934. In the session on 'Cosmic Radiation,' in particular, the experts Carl Anderson and Seth Neddermeyer from Caltech indeed presented such results in their talk on 'Fundamental Processes in the Absorption of Cosmic Ray Particles' (Anderson and Neddermeyer, 1935), and they stated finally: 'The new theoretical values for the mean radiative loss in lead (1.77 MeV/cm for 100 MeV electrons and 500 MeV for 300 MeV electrons, the latter value [of] 250 MeV/cm for a 1 cm lead plate if the dependence on the probability of a radiative loss of the energy is taken into account) still seem to be too high to be reconciled with our experimental data, although the latter contain as yet too few cases where accurate measurements are possible, for a satisfactory comparison to be made.' (Anderson and Neddermeyer, *loc. cit.*, p. 181, footnote) In the discussion at the session on 'Cosmic Radiation,' Bethe freely admitted:

> The experiments of Anderson and Neddermeyer on the passage of cosmic-ray electrons through lead are extremely valuable for theoretical physics. They show that a large fraction of the energy loss by electrons in the energy range around 10^8 volts is due to emission of γ-radiation rather than to collisions, but still the relative energy loss seems far smaller than predicted by theory. Thus the quantum theory apparently goes wrong for energies of about 10^8 volts, and it would be of special value for any future quantum electrodynamics to know exactly at which energy the present theory begins to fail, in other words to have much more experimental data on the energy loss of fast electrons (energy 10^7 to 5×10^8 volts) passing through matter. (Bethe, in Bernardini *et al.*, 1935, p. 250)

That is, in the case of the net radiation loss for highest-energy γ-rays, the theory and experiment thus did not seem to agree by the mid-1930's.[1020] On the other hand, stimulated by the discovery of the positron, the theoreticians worked out some conclusions from Dirac's theory of the electron that might eventually help in analyzing certain special effects observed in the scattering of short-wavelength gammas with nuclei both in the laboratory and in cosmic radiation. The first such effect was proposed by Max Delbrück, a student of Lise Meitner's at the *Kaiser*

[1020] Further experiments carried out by various groups in Europe and the USA would confirm the conclusions derived from the data of Anderson and Neddermeyer in general. However, it was also discovered that the observed energy losses were partly connected with particles other than electrons; the existence of 'heavy electron' would help to clarify the situation later on.

Wilhelm-Institut für Chemie in Berlin; in an addendum to the paper of Meitner and H. Kösters (1933) on the topic, he assumed that 'negative electrons' created in pairs by hard γ-rays (emerging from radiative nuclei) would contribute to the coherent scattering of the incident γ-rays in matter in the same way as 'positive electrons' (Delbrück, 1933). Delbrück's note appeared in July 1933. Later that year, in a letter to *Physical Review* dated 26 October and published in the second issue of November, Otto Halpern of New York University also considered 'Scattering Processes Produced by Electrons in Negative Energy States' (Halpern, 1933). He discussed there in particular what he called the 'scattering properties of the "vacuum,"' i.e., light-scattering processes below the 'permanent formation of electron-positron pairs,' or 'in the language of Dirac's theory of radiation' splittings of the incident quantum in processes of the following type:

> An electron in a negative energy state passes by absorption of the incident quantum into a state of positive energy; the electron then returns in several steps under emission of *hν in toto* to its original state. At each step the total momentum is conserved. A scattering process of this type can only *reduce* the frequency. (Halpern, *loc. cit.*, p. 856)

Halpern hoped to explain with the help of this special process of light scattering the observed red-shift of the spectral lines emitted by distant galaxies (rather than using the expanding universe solution of general relativity theory). Although the elastic or nearly elastic scattering of light by light, created by the production and annihilation of electron–positron pairs in intermediate steps, could not be isolated then from other scattering mechanisms, the theoreticians in the 1930s certainly agreed that they played a role in several observed high-energy phenomena. Two years later, Homi Jehangir Bhabha, an Indian research student in Cambridge, England, introduced another elementary quantum electrodynamical scattering mechanism in high-energy physics, namely, the scattering of electrons and positrons (Bhabha, 1935).

While the above developments showed European theoreticians at work, a number of publications also appeared in the United States in which the known quantum-electrodynamical formalism was applied to cosmic-ray and other high-energy phenomena and the results were compared to the available data. The central figure in this enterprize was J. Robert Oppenheimer, who after completing his graduate and postdoctoral training in Europe, took a teaching position in 1929 simultaneously at the University of California in Berkeley and at Caltech in Pasadena. Having become involved, while in Zurich a little earlier, in Heisenberg and Pauli's pioneering collaboration on relativistic quantum field theory (see Section III.6), he had published—upon his return to the United States—a number of papers and notes on the subject (Oppenheimer, 1929, 1930a–c), in which he investigated in particular certain aspects of Dirac's relativistic theory of the electron (1930b, c). Great interest among his colleagues was aroused by his 'Note on the Theory of the Interaction of Field and Matter' (Oppenheimer, 1930a), which demonstrated in detail the observation (contained already in the Heisenberg–Pauli papers) that the electromagnetic self-energy of a charged particle (say, an electron) turned out to be infinite; that is, in the second-order approximation of the Heisenberg–Pauli–Dirac Hamiltonian (to the order e^2), the perturbation-energy integral became quadratically divergent. In 1931, Oppenheimer directed his attention more to nuclear problems, but the discovery of the positron (by Carl Anderson in Pasadena, California) and its confirmation as Dirac's anti-electron (in England) brought him back to a further intense examination of the problems of quantum electrodynamics, which he now undertook with an increasing number of students and collaborators. Following a visit of Niels Bohr to California in spring 1933, Oppenheimer submitted early in June of that year a longer note to *Physical Review*, which he composed with Milton Plesset—then a National Research Council Fellow—'On the Production of Positive Electrons' occurring in the Coulomb field of nuclei.[1021] Oppenheimer and Plesset obtained formulae for the absorption cross sections, which for very high energies of the incident γ-quantum were proportional to Z^2, with Z the atomic number (or positive charge) of the nuclear scatterer, evidently in partial agreement with the observations of Carl Anderson and Seth Neddermeyer (1933), although they were derived on the basis of a somewhat doubtful procedure (Oppenheimer and Plesset, 1933, especially, pp. 54–55). But in fall 1933, Oppenheimer reported less happily to George Uhlenbeck about further results:

> During the summer and since my return [to Berkeley] we have been working on two things.... For one thing we have wanted to look again at the calculations of the absorption coefficient of very hard gamma rays, where our perturbation method appeared so dubious, and the results so definitely in disagreement with experiment. We have found a way of calculating this absorption which for large enough gamma energies appears to be fully justified; and the answer is definite.... The results are even more definitely in disagreement with experiment than those which Plesset and I got; for small Z we just get our old result, whereas for larger Z we get a larger result than before, and increasing more rapidly than Z^2. I think therefore that the methods of the radiation theory give completely wrong results when applied to wavelengths of the order of electron radius. For radiation which is not too hard the theory presumably gives the right answer; and I understand that in Cambridge they are making more careful and laborious calculations just for this case. (Oppenheimer to Uhlenbeck, fall 1930, in Oppenheimer, 1980, pp. 167–168)

Nevertheless, the following notes, written with his student Leo Nedelsky and published between December 1933 and February 1934 on that subject (Oppenheimer and Nedelsky, 1933, 1934a, b), satisfied him, although he admitted (in a letter to his younger brother Frank on 7 January 1934): 'There is no doubt that the theory is quite wrong for cosmic ray energies, but it is a devil of a job to see just exactly what it gives.' (See Oppenheimer, 1980, pp. 171–172)

[1021] See Oppenheimer's letter to Bohr, 14 June 1933, published in Oppenheimer, 1980, pp. 161–162.

Simultaneously with this practical application of the known formalism of *QED*—essentially in the Born approximation, as used also by his European colleagues Bethe, Heitler, and others at that time to deal with the stopping power of fast electrons (and leading eventually also to a breakdown at the highest energies)—to the problem of pair production, Oppenheimer approached a deeper theoretical task, as he announced in his letter to Uhlenbeck in fall 1933, notably, 'the development of a general formalism [of electrons and positrons]' (see Oppenheimer, 1980, p. 168)—a deeper theoretical problem indeed—which he now treated with Wendell Furry, another National Research Council Fellow. He reported about the progress in this ambitious programme to his brother Frank in England:

> The work went well all autumn. I sent Dirac a copy of a long discourse on MNtory [i.e., a kind of inventory of the number of positive (M) and negative (N) electrons] but even since the writing we have come on some new and simplifying things. I do not know whether Dirac liked what we wrote; but if you see him you might warn him that we shall send more presently, in which by extending the group of transformations under which positive and negative [energy] states could be defined, we can greatly shorten some of the proofs, treat the gauge invariance more adequately, and take into account the non-observability of the wave functions in the theory. This extension, while it is not absolutely necessary for making a sensible theory, seems to me very clarifying. It makes the nonobservability of the susceptibility of pairs even more certain. (J. Robert Oppenheimer to Frank Oppenheimer, 7 January 1934, in Oppenheimer, 1980, p. 171)

The entire programme had obviously been stimulated by Niels Bohr's visit in spring 1933 and Oppenheimer's discussions with him, and it resulted directly into a lengthy and—for the young Oppenheimer—unusually 'philosophical' paper entitled 'On the Theory of the Electron and the Positive [Positron].' The authors, Furry and Oppenheimer, summarized its contents in the abstract as:

> In this paper we develop Dirac's suggestion for the interpretation of his theory of the electron (Dirac, 1931c) to give a consistent theory of electrons and positives. In Section 1, we discuss the physical interpretation of the theory, the limits which it imposes on the spatio-temporal description of a system and in particular on the localizability of the electron. In Section 2, we set up the corresponding formalism, including wave functions to describe the state of the electrons and positives in the system, and constructing operators to represent the energy, charge and current density, etc. It is shown that the theory is Lorentz invariant, and just has that invariance under contact transformations which the physical interpretation requires. The electromagnetic interaction of the electrons and positives is formulated, and certain ambiguities which arise here are discussed. In Section 3, it is shown that in all problems to which the Dirac equation is directly applicable it gives the correct energy levels for the electron, and the correct radiative and collision transition probabilities.... In Section 4, we discuss certain problems which have no analogue in the original Dirac theory of the electron, show that a certain part of the energy of an electromagnetic field resides in the electrons and positives, and consider the extent to which, in the present state of theory, this can be detected. (Furry and Oppenheimer, 1934a, p. 245)

Thus, the investigation, which Oppenheimer also presented at the meeting of the American Physical Society at Boston in late December 1933 (Oppenheimer, 1934), aimed at no less than a new, more fundamental formulation of Dirac's theory of the electron, as is confirmed by the following excerpt from the introductory remarks of his first paper with Furry:

> The Dirac theory of the electron ... starts with the postulation of a probability density $W(x)$ that the electron be found near the point x, and thus guarantees the observability of the position of the electron. But it does this only at the expense of admitting the existence of states of negative kinetic energy.... Because of the non-existence in fact of electrons of negative kinetic energy, the postulation of complete localizability of the electron and the existence of the probability density $W(x)$ appears unjustifiable.
>
> With the charge density the situation is completely different. On the Dirac theory, it is true, this charge density is merely proportional to $W(x)$:
>
> $$\rho(x) = eW(x) \qquad [(731)]$$
>
> But for the determination of ρ other experimental procedures are available. For the quantum theory of the electromagnetic field and the careful considerations given by Bohr to the possibilities of observation which it implies [see Bohr and Rosenfeld, 1933] show that, at least as we may abstract from the atomic nature of the measuring instruments, the electric field may be mapped out with any precision we want.... In any theory in which the atomic nature of the measuring apparatus is neglected, this observability of charge density must persist. Since we have seen what grave difficulties inhere in relativistic theory in the definition of particle density, we must be prepared to abandon the simple definition of ρ given by [Eq. (731)]. (Furry and Oppenheimer, *loc. cit.*, p. 247)

These statements sounded like a programme envisaged by Niels Bohr, the old 'pope of quantum theory,' and his eager new 'evangelists' Furry and Oppenheimer rushed to carry it out in complete technical detail. For an adequate replacement of Dirac's theory of the electron, they started from a relativistic wave function $\psi_{N,M}(r,\rho)$, yielding (as in Erwin Schrödinger's original wave mechanics) 'directly the probability $P(r_1 \ldots r_N; \rho_1 \ldots \rho_M)$ of finding in the system [under investigation] N electrons and M positives [i.e., positrons] in the state $r_1 \ldots \rho_M$' (*loc. cit.*, p. 254). They constructed this wave function (in Section 2 of their paper) in a somewhat clumsy way from creation and annihilation operators (Furry and Oppenheimer, *loc. cit.*, pp. 250–252), and derived therefrom expressions for the charge density which could be inserted into the expressions for the electromagnetic interaction terms (Furry and Oppenheimer, *loc. cit.*, pp. 253–254).[1022] The systematic replacement of a hole in the magnetic-energy states by a positive particle, as the foundation of the entire Furry–Oppenheimer scheme, evidently demanded a restriction in applying the usual quantum-mechanical transformation theory. In

[1022] The r and ρ variables included, of course, the spin orientation of the states.

Section 3, Furry and Oppenheimer proved the equivalence of the results following in those cases, in which the old Dirac formulation had succeeded, e.g., in the case of stationary energy states of an electron in an atomic field (but not in the case of Klein's paradox); in doing so, they neglected the mutual interactions of the particles (electrons) which they considered to yield a small contribution. They even expressed some unhappiness about this particular situation by saying that 'it is thus in general not necessary to use the wave function $\psi_{N,M}(r,\rho)$ at all ..., since the wave equations which determine them are in generation intractable' (Furry and Oppenheimer, *loc. cit.*, p. 259). However, in other cases, e.g., when calculating the energy $E^{(0)}$ of the 'nascent' electron–positron pairs, the full new formulation had to be applied and even yielded infinite results, thereby pointing also to a limitation of the Furry–Oppenheimer electron-positive theory, which—as they emphasized—'may be schematically formulated as the failure of such theories when applied to extremely small lengths or intervals of time.' Hence, they emphasized that it is 'at once apparent that the theory in its present form can make no predictions whatsoever about the fields within the critical distance e^2/mc^2 of a charge.' (Furry and Oppenheimer, *loc. cit.*, p. 260)

In the case of pair production, the problem considered earlier in Berkeley on the basis of *QED*, Furry and Oppenheimer now obtained evidence 'that the present theory gives too high probability for high energy pairs,' which they ascribed to the '(classical) model of the point electron which underlies the present theory' (Furry and Oppenheimer, *loc. cit.*). But if one took proper care of the fact that the electrodynamical theory 'would give altogether wrong results for the reaction of the electron to light of wavelength appreciably shorter than the critical length e^2/mc^2 [determined by the classical electron radius],' one might be able to compute the energy $E^{(0)}$ of the ground-state pairs in an electromagnetic field of energy $E_e\left(=\frac{1}{8\pi}dV(\mathbf{E}^2+\mathbf{H}^2)\right)$, and obtain via the equation

$$E^{(0)}/E_e = -\alpha\kappa \tag{732}$$

the polarization effect added by 'nascent pairs.' Furry and Oppenheimer estimated a value of about 2 for the quantity κ, and concluded: 'This result tells us that the work we must do to establish an electrostatic field is about 2 percent less than the energy stored in the electromagnetic field; the difference is supplied by the pairs.' They then showed that the result would not change the electromagnetic theory drastically. In order to retain the standard equation for $E^{(0)}$, one had just to redefine the unit charge, and the difference between the redefined and the 'true' charges would not be observable:

> Because of all the polarizability of the nascent pairs, the dielectric constant of space in which no matter has been introduced differs from that of truly empty space. For fields which are neither too strong nor too rapidly varying the dielectric constant of a vacuum then has the constant value $\sim(1+\kappa\alpha)$. Because it is in practice impossible not to have pairs present, we may redefine all dielectric constants, as is customarily

> done, by taking that of a vacuum to be unity. (Furry and Oppenheimer, *loc. cit.*, p. 261)

The only observable consequence from the theory seemed to consist in a small increase of the effective charge of the proton.[1023]

In a short note, dated 12 February 1934, Furry and Oppenheimer simplified the treatment of gauge invariance in their theory; they emphasized that only the finite results obtained were really gauge and Lorentz invariant, and that the theory failed to give reliable results for very short-wavelength quanta (Furry and Oppenheimer, 1934b). 'At this point came a letter from Pauli,' Oppenheimer wrote in March 1934 to Uhlenbeck and reported:

> He told us that he had set Peierls to calculating the magnetic susceptibility, and that they had found what earlier we had—that it was not independent of gauge.... The search was absolutely sterile, and we are now persuaded, although not beyond conviction, that no classification of states can be found in a gauge invariant definition.... (See Oppenheimer, 1980, p. 175)

Furry and Oppenheimer were 'prepared to believe that the theory can be improved.' 'But,' Oppenheimer continued in his letter to Uhlenbeck, 'we are skeptical, and think that this will not be on the basis of quantum-theoretic field methods,' and added: 'This point should be settled by summer; either Pauli or Dirac will have found the improvement or they will have come with us to share the belief that it does not exist.' (Oppenheimer, *loc. cit.*)

The question of a gauge-invariant formulation of a quantum field theory containing no infinities remained for some time as a desideratum that could not be satisfied. What concerned the Furry and Oppenheimer theory of electrons and protons, nobody pursued it further, not even Oppenheimer and his associates. In looking back, Weisskopf emphasized its main merits by saying:

> It was recognized in 1934 by J. Robert Oppenheimer and Wendell Furry that the creation and destruction operators are more suitable for turning the liability of the negative states into an asset, by interchanging the role of creation and destruction of those operators that act on the negative states. This interchange can be done in a consistent way without any fundamental change of the equations. The consequences are identical to those of the filled-vacuum assumption, but it is not necessary to introduce that disagreeable assumption explicitly. Particles and antiparticles enter symmetrically into the formalism, and the infinite charge density of the vacuum disappears. (Weisskopf, 1983, pp. 68–69)

Hence, the great efforts of Furry and Oppenheimer—though they did not result into a workable theory—brought about a formal improvement by suggesting a

[1023] For a proton, owing to its larger mass M ($\gg m$), the quantum-field theoretical difficulties should arise only for smaller distances or larger accelerations than in the case of the electron; hence, the changes in the electron theory might become visible already in the spectroscopic observations. For a check of this suggestion, see below.

possible way out of the 'hole' assumption, though this was not the path to be pursued immediately.

In the letter to Uhlenbeck referred to above, Oppenheimer also remarked that 'from Dirac we have not had a murmur.' Indeed, Dirac did not take the time to respond to the Furry–Oppenheimer theory while he was himself engaged in a new formulation of the hole theory, which he had begun to introduce in October 1933 with his report on the '*Théorie du positron* (Theory of the Positron)' presented at the seventh Solvay Conference in Brussels (Dirac, 1934b). In it, Dirac established a quantum-mechanical description of the experimentally well-established positrons, at least for phenomena on a scale above the classical electron radius e^2/mc^2, or for energies considerably smaller than $mc^2/(e^2/\hbar c)$, by employing his concept of 'holes;' that is, he represented the positrons by holes in a nearly filled sea of occupied single states of negative energy extending throughout space. He then showed that the *positive-energy* states so defined (as compared to a completely filled 'Dirac sea') indeed behaved like an anti-electron, which could also annihilate with a positive-energy state (defining an electron) into photons, with energy and momentum being conserved.[1024] Moreover, a world of fully occupied negative-energy states would not exhibit any electric fields, the latter being created only by the occupied positive-energy states (i.e., electrons with charge $-e$) and/or holes (i.e., positrons with charge $+e$), following the relation

$$\operatorname{div} \mathbf{E} = 4\pi\rho, \tag{733}$$

where **E** denoted the vector of the electric field and ρ denoted the unified charge density. Dirac commented: 'The new assumption works satisfactorily when we deal with a field-free space, where the distinction between positive and negative energy states is clearly defined,' and added:

> But it has to be made more precise to give unambiguous results in regions with non-zero fields. We have to supply a mathematical rule for specifying which electron distribution produces no field, and a rule for subtracting this distribution from the given one, so as to obtain a finite difference which can be substituted into Eq. [(733)], as in general subtracting two infinite quantities is not a mathematically well-defined operation. (Dirac, *loc. cit.*, p. 207).

While Dirac could not solve this problem in the general case of an arbitrary electromagnetic field, he managed in the case of a weak electrostatic field—by introducing a (nonrelativistically defined) density matrix in the Hartree–Fock approximation—to establish the following result: The charge density emerging from the polarization, as produced by the action of the field on the negative-energy electrons, consisted of two terms; the first, the principal term provided 'a charge density only where the charge density ρ producing the field is non-zero, and

[1024] That is, in the presence of an atomic nucleus, one photon would result in 'free'-space into two photons.

that the induced density cancels a fraction of order 1/137 of this density'; the second term 'is a significant correction only when the density ρ varies rapidly with position and changes appreciably over a distance of order $\hbar/mc$.' (Dirac, *loc. cit.*, p. 212) Hence, in conclusion of his Solvay report, Dirac noted that the conventionally assumed situation was reproduced, but for small effects created by the polarization due to the negative-energy states.

In the following paper, submitted in early February 1934 to the *Proceedings of the Cambridge Philosophical Society* and entitled 'Discussion of the Infinite Distribution of Electrons in the Theory of the Positron,' Dirac developed the idea of the density matrix further (Dirac, 1934d). In particular, he now introduced a 'relativistic density matrix R,' whose elements depended on two times t' and t'' and which might be split into appropriate subterms ($\frac{1}{2}R_F$ and $\frac{1}{2}R_1$, where R_F represented the full distribution with all possible states occupied). 'At least to the accuracy of the Hartree method of approximation,' he obtained the result:

> (i) One can give a precise meaning to a distribution of electrons in which every state is occupied. This distribution may be defined as described by the density matrix $R_F \ldots$, this matrix being completely fixed for any given field.
>
> (ii) One can give a precise meaning to a distribution of electrons in which nearly all (i.e., all but a finite number, or all but a finite number per unit volume) of the negative-energy states are occupied and nearly all of the positive-energy ones are unoccupied. Such a distribution may be defined as one described by a density matrix $R = \frac{1}{2}(R_F + R_1)\ldots$. Our method does not give any precise meaning to which negative-energy states are unoccupied or which positive-energy ones are occupied. It is sufficiently definite, though, to take as the basis of the theory of the positron the assumption that only the distributions described by $R = \frac{1}{2}(R_F + R_1)$ ... occur in nature.
>
> (iii) A distribution R such as occurs in nature according to the above assumption can be divided naturally into two parts
>
> $$R = R_a + R_b, \qquad [(734)]$$
>
> where R_a contains all the singularities and is also completely fixed for any given field, so that any alteration one may make in the distribution of electrons and positrons will correspond to an alteration in R_b but to none in R_a. We get this division into two parts by putting the term containing [the finite] g into R_b and all other terms into R_a. Thus
>
> $$R_b = g/4i\hbar. \qquad [(735)]$$
>
> It is easily seen that R_b is relativistically invariant and gauge invariant, and it may be verified after some calculation that R_b is Hermitean and that the electric density and current density corresponding to it satisfy the [usual] conservation law. It therefore appears reasonable to make the assumption that *the electric and current densities corresponding to* [*the finite*] R_b *are those which are physically present, arising from the distribution of electrons and positrons*. In this way we can remove all the infinities mentioned. (Dirac, *loc. cit.*, pp. 162–163)

Dirac added that further work had to be done to complete his formalism, like including the effect of the exclusion principle; and one had to examine the physical consequences, such as the polarization of the vacuum by an electromagnetic field.

Dirac's new formulation of the 'hole theory' caused quite some stir in the community of quantum physicists and stimulated many further investigations, especially in Leipzig and Zurich (by Heisenberg, Pauli, and their collaborators) but also in Berkeley. In Berkeley, Furry and Oppenheimer published soon—in a June issue of the *Physical Review*—a note 'On the Limitation of the Theory of the Positron,' in which they remarked critically:

> In the further development of Dirac's suggestion one meets, however, a curious difficulty, in that it is apparently impossible to find a consistent definition of the operators for the energy and momentum density of the *epd* (electron-positron distribution). Dirac's density matrix, of course, makes possible a complete formal definition of any operator.... If one carries this through for the energy momentum tensor of the *epd*, one finds in general that its divergence is not given by the Lorentz force with Dirac's expressions for the charge and current. This is because the electromagnetic potentials enter explicitly in the density matrix and lead to the existence of non-Maxwellian forces.... (Furry and Oppenheimer, 1934c, pp. 903–904)

Furry and Oppenheimer continued: 'The simplest way of obviating these difficulties is to modify the density matrix in a way which does *not* depend on the electromagnetic field strengths present: i.e., to subtract from the operator given by the Dirac theory of the electron the expressions for the state of the electron distribution in the absence of external fields, for which all negative states are full.' (Furry and Oppenheimer, *loc. cit.*, p. 904) And they emphasized that 'this procedure leads directly to the theory of the positron as we have developed it [in Furry and Oppenheimer, 1934a].' That is, only their theory would yield a valid description of electron–positron phenomena, as long as questions involving lengths of the order of e^2/mc^2 would not be asked.[1025]

In spite of these strong statements, Oppenheimer did not continue to work on his own fundamental theory of electrons and positrons, but rather turned his attention back to the practical applications of Dirac's new theory to the absorption of high-energy photons as observed in cosmic radiation. The leadership in the theoretical questions of principle shifted again to Europe, where Oppenheimer's colleagues in turn criticized his efforts. Thus, Wolfgang Pauli, in a letter to Werner Heisenberg, dated 21 January 1934, categorically declared: 'A short while ago, Oppenheimer sent me a manuscript, which treated, however, only the old, non-gauge-invariant formulation of the hole theory, and which completely ignored the problems treated by Dirac and ourselves.' (See Pauli, 1985, p. 255) In Leipzig and Zurich, they rushed to achieve the next advances.

After completing their pioneering set of papers on quantum field theory (Hei-

[1025] Such questions would, however, play a role in a theory of the positron, as Furry and Oppenheimer pointed out in their note (1934c).

senberg and Pauli, 1929, 1930), Heisenberg and Pauli had directed their attention to other questions, notably, the problems of nuclear physics.[1026] Only around the middle of 1933, following the experimental substantiation of the existence of the positron, did the rich and rewarding correspondence between Heisenberg and Pauli turn to the new topic of Dirac's hole theory.[1027] Pauli opened the exchange on 16 June 1933, when he wrote: 'I do not believe in the theory of holes (*Löchertheorie*), since I wish to have an asymmetry in the laws of nature between positive and negative electricity,' and then added that Walter Elsasser even suspected the positive electrons to obey the Bose statistics, in contradiction to Dirac's theory, which he [Pauli] liked (see Pauli, 1985, p. 169). But, about a month later, Pauli was 'not disinclined to believe in a kind of reformed hole theory,' stimulated that he now was by the theoretical interpretation, given by Max Delbrück and Rudolf Peierls, of the Meitner–Hupfeld effect as a consequence of pair creation (Pauli to Heisenberg, 14 July 1933, in Pauli, *loc. cit.*, p. 187). In his reply, Heisenberg proposed to make use of holes in the Hamiltonian formalism of quantum electrodynamics for improving upon the divergence problems. 'Therefore I believe strongly in the hole theory, and think that one should in future compute all problems, e.g., the scattering of γ-rays from nuclei, with the scheme [including holes and a certain arrangement of non-commuting factors],' Heisenberg wrote to Pauli on 17 July, though he admitted that the procedure would not remove the infinite self-energy (see Pauli, *loc. cit.*, p. 194). Unlike Heisenberg and Peierls, Pauli remained skeptical about the prospects of the hole theory; still, he suggested (in a letter dated 19 July 1933, to Heisenberg) the exposition of the topic in a report at the seventh Solvay Conference in October of that year, to be given either by Paul Langevin or Paul Dirac himself.[1028] Dirac indeed gave the hole-theory report

[1026] However, in 1930, Heisenberg had also written a paper on the behaviour of fast electrons and investigated in particular the consequences from the assumption of zero mass for the electrons (Heisenberg, 1930b); and in January 1931, he had discussed the problems of energy fluctuation in a radiation field (1931c)—these were still topics related to quantum electrodynamics. (For historical reviews of Heisenberg's work on quantum electrodynamics up to 1936, see Pais, 1989, and Mitter, 1993.) Pauli, on the other hand, published only a few investigations from 1930 to 1934, mainly dealing with quite general problems of the quantum theory of the electron and quantum field theory.

[1027] Pauli first mentioned the hole theory in a letter to Patrick M. S. Blackett, dated 19 April 1933, congratulating him on his successful work with Giuseppe Occhialini on the discovery of the positive electron; he then added: 'Besides I don't believe in Dirac's "holes," even if the positive electron exists.' (See Pauli, 1985, p. 158) In the later letter to Heisenberg, dated 16 June 1933, he even admitted: 'What concerns the theoretical scheme of Dirac's hole theory I have after its exposition [by Dirac in late 1929] developed one myself and presented it in detail in Copenhagen and Leyden.' (Pauli, *loc. cit.*, p. 169) This remark evidently referred to Pauli's lectures of March and April 1930 (Dirac had expounded the idea of 'holes' in fall 1929 and written about it to several colleagues; see Section IV.3 above). Heisenberg also made use of the idea of 'holes' quite early, e.g., in his paper on Pauli's exclusion principle, submitted in June 1931 and dealing with nonrelativistic problems of atomic and solid-state theory (Heisenberg, 1931d); however, he did not mention Dirac there at all.

[1028] Although Pauli admitted that his 'attitude towards the hole theory was not anymore entirely reserved and negative' (see his letter to Heisenberg, 29 September 1933), he raised serious objections, such as the lack of gauge invariance against the formalism (see Pauli, 1985, p. 212). Heisenberg then tried to construct a gauge-invariant hole theory, but Pauli proved that it was actually not so (Pauli to Heisenberg, 9 November 1933, in Pauli, *loc. cit.*, p. 223).

(Dirac, 1934b), and in the following months, he entered into a correspondence with Pauli on the subject. Simultaneously, Pauli and Heisenberg developed a joint programme on quantum electrodynamics, which they had agreed upon in Brussels: Basically, Heisenberg worked out the details between November 1933 and January 1934, which Pauli criticized subsequently.[1029] The common goal of their approach and the doubts to achieve it in a hole theory were expressed clearly by Heisenberg as follows:

> Of course, it would be most satisfactory, if one were able to establish—completely independently of any conception of holes—a theory, in which (I) the charge density comes out finite and (II) the energy-momentum density also remains finite, with the former being positive. This goal cannot be achieved before one is able to fix the value of $e^2/\hbar c$, possibly on the basis of using essentially the neutrino.
>
> With respect to analysing (II) one must only put forward the postulate that, starting from *the known force-free state* (*als bekannt vorauszusetzenden kräftefreien Zustand*) of the hole theory, certain matrix elements ... will now be reinterpreted in terms of pair creation, with the energy remaining positive.
>
> The best to be expected is that according to Dirac the postulate (I) can be just satisfied. However, it must really be doubted whether one should put so much emphasis on that, as long as the self-energy still remains infinite.... Therefore I rather believe that, for an arbitrary value of $e^2/\hbar c$, the "theory of holes" cannot actually be formulated in a unique way. (Heisenberg to Pauli, 30 January 1934, in Pauli, 1985, p. 270)

The failure of Dirac's new hole theory to satisfy their programme and hopes disappointed both friends deeply. 'My feeling of unhappiness was increased immensely when yesterday I received Dirac's manuscript of his investigation that we had been expecting since long,' Pauli wrote to Heisenberg on 6 February 1934, and continued: 'At the moment I am close to a light faintness (*leise Ohnmacht*) from the [inability] to calculate practically anything with his formulae.' (See Pauli, *loc. cit.*, p. 275.) He did not hesitate to call the new paper (Dirac, 1934d), '*Diracs Naturgesetzgebung auf dem Berge Sinai* (Dirac's Commandment of the Law of Nature from Mount Sinai),' about which he was very '*degoutiert* (disgusted)' (Pauli, *loc. cit.*). In his reply to Pauli on 8 February, Heisenberg declared that 'Dirac's theory, which I only know so far from two eruptions of despair from Copenhagen and Zurich to be erudite nonsense' (see Pauli, *loc. cit.*, p. 279). But at the same time, he suggested a different hole scheme; after some criticism and subsequent clarification, Pauli proposed that Heisenberg, Weisskopf—who had become his assistant in fall 1933—and he should compose a 'three-man paper.' In particular, Pauli wrote:

[1029] In January 1934, Pauli also formulated a detailed programme based on the assumption that the particle number could not be determined directly by measurement (addendum, entitled '*Über die quantenelektrodynamische Formulierung der Löchertheorie* (On the Quantum-Electrodynamical Formulation of the Theory of Holes),' in Pauli to Heisenberg, 21 January 1934; see Pauli, 1985, pp. 257–263).

> The paper should contain: The formulation of the general theory (with a special section on the problem of the energy-momentum tensor). Precision of limiting procedures. A section on the fluctuation of the charge density (using the contents of your last letter). A further section on the vacuum polarization in a field changing with time (according to Weisskopf). (Pauli to Heisenberg, 17 February 1934, in Pauli, *loc. cit.*, pp. 293–294)

In particular, he requested: 'Dirac's conceptions should be battled.' To his letter, Pauli also added an outline of the programme for the three-man paper he had proposed—entitled '*Beiträge zur Theorie der Elektronen und Positronen* (Contributions to the Theory of the Electrons and Positrons)' (see Pauli, *loc. cit.*, pp. 294–300)—but the proposed common work of the Zurich–Leipzig team was not quite realized. Instead, a part of the proposed programme found its place in Viktor Weisskopf's publication of his work on the self-energy of the electron (which we shall discuss later, and which was received by *Zeitschrift für Physik* on 13 March 1934: Weisskopf, 1934a), while Heisenberg formulated another part—which he elaborated in critical discussions with Pauli and Weisskopf—in an extended paper of his own (having been encouraged to do so by Pauli himself), entitled '*Bemerkung zur Diracschen Theorie des Positrons* (Remarks on Dirac's Theory of the Positron)' and received by *Zeitschrift für Physik* on 21 June 1934 (Heisenberg, 1934d). In the introduction, he wrote that 'the intention of the present work is to build Dirac's theory of the positron into the formalism for quantum electrodynamics,' and continued:

> In this context, it should be required that the symmetry of nature between the positive and negative charge is expressed from the very beginning in the fundamental equations of the theory; moreover, besides the divergences created by the known difficulties of quantum electrodynamics [*QED*], no further infinities [should] occur in the formalism, i.e., the theory provides an approximate method to deal with the set of problems which could already be treated by the known *QED*. … The present attempt … is closely connected with a paper of Dirac [1934d]. As compared to the latter, the importance of conservation laws for the whole system—radiation–matter—is emphasized, and also the necessity to formulate the fundamental equations in a way going beyond the Hartree-Fock method. (Heisenberg, *loc. cit.*, p. 209)

Heisenberg's paper of June 1934 consisted of two parts. In the first, larger part—entitled 'Visualizable (*anschauliche*) Theory of Matter Waves'—he used Dirac's density matrix and the Hartree–Fock approximation explicitly and showed that Dirac's subtraction procedure in the R-matrix (which exhibited symmetry between the electrons and holes) was indeed compatible with the usual conservation laws. He then noted that the additional term computed by Dirac in the charge density, the 'induced density' created by electron–positron pairs, namely,

$$\rho = -\frac{1}{15\pi}\frac{e^2}{\hbar c}\left(\frac{\hbar}{mc}\right)^2 \Delta\rho_0 \tag{736}$$

(with ρ_0 as the external charge density), 'has no physical meaning, because it cannot be separated from the "external" density and is therefore added automatically to the "external" density'; indeed, this 'vacuum polarization' would give rise 'to a physical problem only for time-dependent external densities' (Heisenberg, *loc. cit.*, p. 222). In the second part—entitled '*Quantentheorie der Wellenfelder* (Quantum Theory of Wave Fields)' (Heisenberg, *loc. cit.*, pp. 224–231)—Heisenberg indeed went beyond Dirac's Hartree–Fock approximation method; he especially introduced *q*-number wave fields and developed both a perturbation method (still along the lines of the Hartree–Fock approximation) and a different iteration procedure, thereby expanding the Hamiltonian up to the fourth order in the electric charge. As Abraham Pais noted later, 'Heisenberg gives for the first time the foundations for the quantum electrodynamics of the full Dirac-Maxwell set of equations in the way we know it today,' and added: 'Furry and Oppenheimer [1934a] had the same idea, but Heisenberg pushed it much further.' (Pais, 1989, p. 101)

With the new theoretical scheme, Heisenberg now calculated the photon self-energy in the second order, arriving at the strange result that the energy diverged even before the limit for the distance $x_\lambda \to 0$ was taken (and giving rise to the usual divergences in *QED*). He quickly commented:

> The fact that only the application of quantum theory leads to divergences that do not occur in the visualizable theory of wave-fields, suggests the assumption that, although this visualizable theory already contains essentially the correct correspondence-like description of the events, still the transition to quantum theory cannot be performed in the primitive manner as has been attempted in the presently available theory. (Heisenberg, 1934d, p. 231)

Here, Heisenberg was misled by a computational error, as Robert Serber pointed out later (Serber, 1936); if the error were avoided, a more standard result for the photon self-energy followed in the second order, namely,

$$W = -\frac{\alpha h\nu}{3\pi}\left[z/r^2 - 2\log\frac{1}{2}Cr + O(r)\right], \tag{737}$$

with z the component of the space vector $\boldsymbol{x}$ in the direction of the electric photon vector, $r = |\boldsymbol{x}|$, and $\log C = 0.577$ (Serber, *loc. cit.*, p. 548). Evidently, the right-hand side of Eq. (737) diverged for $r \to 0$, i.e., the limit to zero spatial distance.[1030]

In the following investigation, presented on 23 July 1934, before the *Sächsische Akademie der Wissenschaften*, Heisenberg treated the problem of charge fluctuations—which he had discussed before in the Heisenberg–Pauli *QED* (1931c)—in Dirac's positron theory (Heisenberg, 1934e). He obtained for the

[1030] Several other, less-critical, mistakes were corrected a little later by Heisenberg himself (1934g).

fluctuation the expressions

$$\overline{(\Delta \bar{e})^2} \sim \begin{cases} e^2 \dfrac{V^{2/3}}{cTb} & \text{for } T \ll \dfrac{\hbar}{mc^2}, \quad (738a) \\[2ex] \dfrac{e^2 V^{2/3} \hbar}{(cT^2) \cdot mc \cdot b} & \text{for } T \gg \dfrac{\hbar}{mc^2}, \quad (738b) \end{cases}$$

and concluded 'that in measuring the charge in a given space-time region [denoted by the volume V and the time T] fluctuations occur which have no analogue in classical theory, arising from matter created by measurement on the surface [whose width was denoted by b] of the spatial region under investigation' (Heisenberg, *loc. cit.*, p. 322).[1031] In this case, extra infinities did not occur, since b could be smeared out properly, but what happened to the polarization effect considered by Furry and Oppenheimer in 1933 if calculated in the new Dirac–Heisenberg positron theory? Two contributions dealing with this question came from California, another two from Leipzig, and a fifth from Viktor Weisskopf, then in Copenhagen.

Robert Serber opened the competition in his paper on 'Linear Modifications in the Maxwell Field Equations,' submitted in April 1935 to *Physical Review*; in particular, he calculated both charge and current densities induced in the vacuum by an electromagnetic field, both static and varying in space and time (Serber, 1935). At the same time, Edwin A. Uehling considered the same effects caused by electrostatic fields varying strongly in space but having limited maximum field strengths; the vacuum polarization thus obtained caused deviations from Coulomb's law, which might give rise to 'departures from the Coulombian scattering law for heavy particles and the displacement in the energy levels for atomic electrons moving in the field of the nucleus' (Uehling, 1935, p. 55). They obtained results in agreement with those derived earlier by Furry and Oppenheimer (1934a).[1032] Then, in Leipzig, Heisenberg's students Hans Euler and Bernhard Kockel picked up the already-mentioned problem of the scattering of light by light—Delbrück, Halpern—which also gave rise, in the Dirac–Heisenberg theory of the positron, to additional polarization effects and certain changes resulting in the Maxwell equations (Euler and Kockel, 1935). In particular, they examined the interaction process creating (from each photon) a virtual electron–positron pair

[1031] In a later 'Note on Charge and Field Fluctuations,' Oppenheimer provided a simple interpretation of the effects Heisenberg had calculated, and remarked: 'The pair-induced fluctuations in the radiation field are in general small of order α compared to those which arise from the corpuscular character of radiation.' (Oppenheimer, 1935b, p. 144)

[1032] In January 1936, while at the Institute for Advanced Study in Princeton, Pauli submitted a paper that he wrote with Morris Erich Rose, in which they simplified the calculation of the additional current density in the Dirac–Heisenberg theory (Pauli and Rose, 1936).

and decaying again into light-quanta (below the energy sufficient to create a real pair), which corresponded to a fourth-order (in the electron's charge) perturbation term H_4,

$$H_4 = +\frac{1}{12\pi^2}\left(\frac{e^2}{\hbar c}\right)^2 \frac{1}{\hbar c} \lim_{r\to 0} \int d\xi \left(\mathbf{A}(\xi), \frac{\mathbf{r}}{r}\right)^4, \tag{739}$$

which had been obtained already by Heisenberg (Heisenberg, 1934d, p. 228).[1033] Then, they expanded H_4—this time in terms of the light-quantum energy, or, more accurately, the dimensionless quantity $h\nu/mc^2$—and found that in zeroth order the result could be formally represented by H', a new Hamiltonian of the electromagnetic field, containing an additional term; i.e.,

$$H' = -\frac{1}{360\pi^2}\frac{\hbar c}{e^2}\frac{1}{E_0^2}\int [(\mathbf{B}^2 - \mathbf{D}^2) + 7(\mathbf{B}\cdot\mathbf{D})^2]dV, \tag{740}$$

with **D** and **B** denoting the vectors for the electrical displacement and the magnetic induction, respectively, and $E_0 = \dfrac{e}{(e^2/mc^2)^2}$ denoting the value of 'the field strength at the rim of the electron' (Euler and Kockel, 1935, p. 246–247). Clearly, H', which Euler and Kockel interpreted 'as *anschaulich* as the interaction energy of light-quanta' implied a 'nonlinear correction to the Maxwell equations of the vacuum,' which 'becomes effective if the field strengths approach the ones "at the rim of the electron"' (Euler and Kockel, *loc. cit.*, p. 247)—though the calculations performed required field strengths definitely below E_0. Still, Euler and Kockel mentioned that the experimental deviations from the classical Maxwell equations due to the light-scattering mechanism were extremely small (and they produced for visible light a cross section of about 10^{-70} cm^2).[1034]

In fall 1935, Heisenberg joined Euler in computing the general higher-order terms correcting the Maxwell equations (properly translated into quantum theory) on the basis of the 'positron theory'; i.e., the terms being induced by static, homogeneous, external electric fields in the absence of (real) electron–positron pairs. In a detailed paper, entitled '*Folgerungen aus der Diracschen Theorie des Positrons* (Consequences from the Dirac Theory of the Positron)' and received by *Zeitschrift für Physik* on 22 December 1935, they obtained rather complex expressions which

[1033] Heisenberg's Eq. (61), however, contained an error of a factor 4, which he corrected (in Heisenberg, 1934g). Heisenberg had already referred to the fact that the H_4-term would describe the scattering of light by light (see Heisenberg, 1934d, p. 228).

[1034] Euler carried out systematic and detailed calculations in his Leipzig doctoral thesis, entitled '*Über die Streuung von Licht an Licht nach der Diracschen Theorie* (On the Scattering of Light by Light According to Dirac's Theory),' the publication of which was dated 21 June 1935, but submitted as a thesis only in late January 1936 (Euler, 1936).

could be written in a condensed form as the effective Lagrangian function $\mathscr{L}$,

$$\mathscr{L} = \frac{1}{2}(\mathbf{E}^2 - \mathbf{B}^2) + \frac{e^2}{\hbar c}\int_0^\infty \exp(-\eta)\cdot\frac{d\eta}{\eta^3}$$

$$\cdot\left\{ i\eta^2(\mathbf{E}\cdot\mathbf{B})\frac{\cos(\frac{\eta}{E_{\text{crit}}}\sqrt{\mathbf{E}^2-\mathbf{B}^2+2i(\mathbf{E}\cdot\mathbf{B})} + c.c.}{\cos(\frac{\eta}{E_{\text{crit}}}\sqrt{\mathbf{E}^2-\mathbf{B}^2+2i(\mathbf{E}\cdot\mathbf{B})} - c.c.} + E_{\text{crit}}^2 + \frac{\eta^3}{3}(\mathbf{B}^2 - \mathbf{E}^2)\right\}, \quad (741)$$

with $E_{\text{crit}} = (m^2c^3/e\hbar) = \frac{1}{137}\frac{e}{(e^2/mc^2)^2}$ giving a critical field strength [Heisenberg and Euler, 1936, p. 728, Eq. (45a)]. In a review of this work, Heinrich Mitter wrote: 'Heisenberg later reported that, in carrying out the laborious calculations, the workers were placed in separate rooms and were not permitted any communication during the calculation; only when everybody had obtained the same result, the contribution was believed to be correct.' He added: 'The result of the paper until today remains one of the few, in which the summation of perturbation theory contributions succeeded [fully].' (Mitter, 1993, p. 117)

The final point in the considerations of the whole subject provided Weisskopf with a kind of review paper, '*Über die Elektrodynamik des Vakuums auf Grund der Quantentheorie des Elektrons* (On the Electrodynamics of the Vacuum based on the Quantum Theory of the Electron),' which he published in late 1936 in Copenhagen (Weisskopf, 1936). He emphasized there in particular that the new quantum-electrodynamical methods of Dirac and Heisenberg led to unambiguous results, provided one assumed the following quantities to be physically meaningless: (i) the energy of the vacuum electrons in the field-free space; (ii) the charge and current density of vacuum electrons in the field-free space; (iii) an electric and magnetic polarizability, constant in space and time and independent of the fields. All of these quantities, he argued, came out to be infinite and had to be subtracted. On the other hand, 'all physically meaningful actions of vacuum electrons ... led to convergent expressions,' and he therefore concluded: 'The hole theory of the positron has given rise to no essential problems in the electron theory, as long as no quantized wave fields are involved.' (Weisskopf, 1936, p. 39)

In spite of Pauli's continuing reservation about Dirac's hole theory, Weisskopf—from Zurich—made a major contribution to the new *QED*, namely, his calculation of the self-energy of the electron, which was completed already in March 1934 and submitted to *Zeitschrift für Physik* as the first separate item of the joint Leipzig-Zurich programme (Weisskopf, 1934a).[1035] For this purpose, Weisskopf made use of a previous method of Heisenberg's radiation theory (Heisenberg, 1931c), which had established a closer connection with classical electrodynamics

[1035] The early submission of this paper as a separate publication of one of the intended authors of the 'three-man collaboration' was advocated by Pauli as an exception, because he did not wish to hinder his assistant's publication, since Weisskopf needed a more permanent position at that time.

and had been tested before in Zurich by Hendrik Casimir (to reproduce the Weisskopf–Wigner results: Casimir, 1933), namely, to use the amplitude of the electromagnetic potentials and their quantum-mechanical commutation relations. Thus, he expressed the self-energy of the electron by a sum of two terms,

$$E_{\mathrm{el}} = E^S + E^D, \tag{742}$$

where E^S denoted the 'electrostatic' self-energy and E^D denoted the 'electrodynamical' self-energy (derived from the vector potential). In the hole theory, E^S and E^D became, respectively,

$$E^S = \frac{1}{2} \iint d\mathbf{r}\, d\mathbf{r}' \frac{[\rho(\mathbf{r}) - \tilde{\rho}(\mathbf{r})][\rho(\mathbf{r}') - \tilde{\rho}(\mathbf{r}')]}{|\mathbf{r} - \mathbf{r}'|} \tag{743a}$$

and (where the vector product is taken within the bracket)

$$E^D = \frac{1}{2} \int \left(j(\mathbf{r}) - \tilde{j}(\mathbf{r}), \mathbf{A}(\mathbf{r}) - \tilde{\mathbf{A}}(\mathbf{r}) \right) d\mathbf{r}, \tag{743b}$$

with $\tilde{\rho}$ and $\tilde{\mathbf{j}}$ (the subtractive) charge and current densities of the electrons in negative states, and $\tilde{\mathbf{A}}$ their vector potential ($\mathbf{j}$, ρ, and $\mathbf{A}$ denoted the corresponding quantities of the positive-energy electrons). Upon inserting the proper expansions in terms of creation and annihilation operators, Weisskopf derived the following expression for the electrostatic part of the electron's self-energy in the state q_0 according to the hole theory,

$$2E^S = \sum_{\substack{+\\r}} \mathbf{A}_{q_0\, r\, r\, q_0}^{++++} - \sum_{\substack{-\\r}} \mathbf{A}_{q_0\, r\, r\, q_0}^{+--+}, \tag{744}$$

with the $\mathbf{A}$'s describing the approximate matrix elements of the Coulomb expression

$$\mathbf{A} = e^2 \int d\mathbf{r}\, d\mathbf{r}' \frac{(\phi^*(\mathbf{r}), \phi(\mathbf{r}))((\phi^*(\mathbf{r}'), \phi(\mathbf{r}'))}{|\mathbf{r} - \mathbf{r}'|} \tag{745}$$

(Equation (744) deviated from the one for the case without negative-energy states by the sign of the second term.) The evaluation of Eq. (744) yielded for the electrostatic term of the electron's self-energy the result

$$E^S = \frac{e^2}{h\sqrt{m^2c^2 + p^2}} (2m^2c^2 + p^2) \int_{k_0}^{\infty} \frac{dk}{k_0} + \text{finite terms.} \tag{746}$$

That is, the expression on the right-hand side diverged logarithmically as compared to the linear divergence of one-electron expression (i.e., without occupied negative-energy states).[1036]

Similarly, Weisskopf evaluated the electrodynamical part

$$E^D = -c_1 \int \frac{dk}{k} - c_2 \int_0^\infty dk - c_3 \int_0^\infty k\, dk + \text{finite terms}, \tag{747}$$

which diverged quadratically. ‘Hence one recognizes that the degree of divergence does not diminish by the occupation of the negative states,’ Weisskopf concluded rashly (Weisskopf, 1934a, p. 39). However, in an addendum to his paper, which was received on 20 July 1934, by *Zeitschrift für Physik*, he remarked: ‘Page 38 of the above paper contains an error of calculation falsifying decisively the result of the electrodynamical self-energy of the electron according to Dirac’s hole theory. I am greatly indebted to Mr. Furry (University of California, Berkeley) for kindly pointing it out to me.’ (Weisskopf, 1934b, p. 817) When corrected, the electrodynamical part also became much weaker, namely, logarithmically divergent; i.e.,

$$E^D = \frac{e^2}{h\sqrt{m^2c^2 + p^2}} \left(m^2c^2 - \frac{4}{3}p^2 \right) \int_0^\infty \frac{dk}{k} + \text{finite terms}. \tag{747$'$}$$

This final reduction of the total divergence of the electron’s self-energy to a logarithmic one appeared to open the possibility to avoid eventually the divergence completely by a suitable limiting procedure, e.g., the λ-limiting process of Gregor Wentzel (see below).

Three years after Weisskopf had achieved this encouraging result, Felix Bloch, then settled at Stanford University in California, wrote a ‘Note on the Radiation Field of the Electron’ jointly with Arnold Nordsieck (a Ph.D. graduate of Oppenheimer’s); in this, Bloch and Nordsieck removed another difficulty of *QED* connected with low-energy radiation (and noticed, e.g., in the k_0-limit of the logarithmic integrals of Weisskopf), which was called ‘infrared divergence’ or ‘infrared catastrophe’ (Bloch and Nordsieck, 1937). As Bloch and Nordsieck stated, this ‘characteristic difficulty … is clearly visible in formulae given by Mott (1931), Sommerfeld (1931), and Bethe and Heitler (1934) for the probability of scattering of an electron in a Coulomb field accompanied by the emission of a single light-quantum,’ in particular:

> If the emitted quantum lies in the frequency range ω to $\omega + d\omega$, this probability is for small frequencies proportional to $d\omega/\omega$ independently of the angle of scattering. Taking these formulae literally and asking for the total probability of scattering with the emission of any light-quantum, one therefore gets by integration over ω a result which diverges logarithmically in the low frequencies. (Bloch and Nordsieck, *loc. cit.*, p. 54)[1037]

[1036] Weisskopf introduced here a lowest momentum k_0 to avoid the divergence of the integral at $k = 0$.

[1037] Although Bethe and Heitler noticed the infrared divergence, they claimed that their screening procedure allowed one to avoid the infinity (see Bethe and Heitler, 1934, p. 96, footnote †).

The infinity thus described was unrelated to the usual 'ultraviolet divergences' of *QED*, and it did not really possess an analogue in the classical theory (although Bloch and Nordsieck noticed an indication there). But, as they wrote (in the introduction), it essentially arose from an inadequate perturbation treatment of *QED* in powers of the electric charge (or $e^2/\hbar c$); and they claimed: 'We shall show how this can be formulated [adequately and free of divergences] in quantum mechanics as the solution in successive approximation of a system of two simultaneous differential equations; of these approximations only the one of the lowest order is here needed and investigated.' (Bloch and Nordsieck, *loc. cit.*, p. 55)

Bloch and Nordsieck indeed discussed the system consisting of the electron plus the electromagnetic field according to their proposal, and they then calculated the transitions in this system due to external forces on the electron by the usual method of small perturbations.[1038] Ultimately, an extra frequency factor ω turned up in the expressions for the scattering, which would remove the logarithmic divergence totally. Though the physicists welcomed the result of this particular calculation as a sign that the infrared 'catastrophe' could be avoided, theorists like Pauli were still not quite happy. At the Galvani Bicentennial Celebration in October 1937, Pauli presented the result of a paper which he had written jointly with Markus Fierz (then his assistant at the *ETH* in Zurich). Pauli and Fierz had attacked the problem somewhat differently by using a finitely extended electron; though the infinity disappeared for all models of the electron, and always finite energy losses (due to the emitted long-wavelength radiation) resulted, they cautioned:

> On the other hand, the dependence of [the cross section] for very small energy losses *E* so critically depends on the extension of the [charged] body in the exact treatment that an immediate application of the result to real electrons cannot be made. Hence we conclude that the problem in question is essentially connected with the still unsolved [divergence] difficulties of quantum electrodynamics. (Pauli and Fierz, 1938, p. 167)

Before proceeding to the next fundamental topic in *QED*, let us first return to an application of the theory, albeit in its preliminary form, to cosmic-ray physics, which especially J. Robert Oppenheimer and his collaborators in California never lost sight of.[1039] For instance, in late 1934, Oppenheimer asked the question: 'Are the formulae for the absorption of high energy radiation valid?', i.e., would they

[1038] The two coupled differential equations mentioned above actually connected the situations of positive- and negative-energy states, and the approximation indicated neglected the negative-energy states. Instead of a perturbation theory in orders of $e^2/\hbar c$, alternative assumptions were used, namely, that $e^2\omega/mc^3$, $\hbar\omega/mc^2$ and $\hbar\omega/c\Delta p$ (with Δp the change in the electron's momentum) were small compared to unity.

[1039] In his historical study on 'Cosmic-Ray Showers, High Energy Physics, and Quantum Field Theories,' David Cassidy claimed the existence of a programmatic difference between 'cosmic-ray physicists,' such as Walter Heitler and J. Robert Oppenheimer, and 'field theorists,' like Paul Dirac, Werner Heisenberg and Wolfgang Pauli (Cassidy, 1981). It seems to us that the state of affairs was much more complex at that time than Cassidy believes; the field theorists Heisenberg and Pauli, in particular, concerned themselves a great deal with problems arising from cosmic-ray observations.

describe the absorption of cosmic-ray electron and gamma rays (Oppenheimer, 1935a)? A little later, he wrote a theoretical note on the production of pairs by high-energy charged particles (Oppenheimer, 1935b). At that time, he referred to the fact that Carl Friedrich von Weizsäcker (1934) and Evan J. Williams (1934) in Europe had previously argued that, if viewed in a suitable coordinate frame of reference, also in high-energy cosmic ray collisions only energies not higher than a few MeV were involved, for which *QED* should provide correct results; however, Oppenheimer maintained that he did not believe that result because experiments (which he had discussed with his colleagues at Caltech in Pasadena) contradicted it. In particular, he wrote:

> Little evidence exists for the validity of the theoretical formulae for pair production by gamma rays of very high energy. The theoretical formulae hold quite well up to 10^7 volts, but beyond there are no definite tests of the formulae. (Oppenheimer, 1935a, p. 46)

Hence, he attempted the following procedure: 'By applying a strict criterion for the validity of classical electron theory, it is possible to derive new formulae for impact and radioactive-energy losses ... which are in far better agreement with experiment than the formulae given by an uncritical application of quantum mechanics to these problems.' (Oppenheimer, *loc. cit.*, p. 44) Thus, he obtained certain damping factors reducing the increase derived from the previous *QED* formulae.

During the following one-and-a-half years, Oppenheimer published only little (just a couple of papers on particular problems of nuclear physics), but afterward he turned to new phenomena observed in cosmic radiation, as the abstract of his talk presented at the Seattle meeting of the American Physical Society, held from 17 to 19 June 1936, indicated. It read:

> The theoretical formulae for ionization and radiation losses of electrons and pair production by photons have, as a consequence that an electron or photon of very high energy will form sprays of electrons, positrons and γ-rays as it passes through matter. For an incident energy of 3×10^9 eV, the maximum of the probable number of electrons and positrons occurs at 2.2 cm Pb, and 45 cm Al; the maximum values attained are 12 and 2.3, respectively. For an incident energy of 10^{12} eV, the maximum occurs at 6 cm Pb, and gives about 2,000 electrons and positrons and a comparable number of photons. The energy distribution observed in the cloud chamber, and the transition and absorption curves both for showers and for bursts, are in good agreement with these calculations. (Oppenheimer, 1936, p. 389)

That is, after the continuously expressed pessimism and great lamenting for years about the failure of the standard *QED* calculations in cosmic-ray phenomena, there now suddenly sneaked in a more optimistic view into Oppenheimer's thinking. Less than half a year later, in fact, on 8 December 1936, the *Physical Review* received an extended paper of Oppenheimer's, written with J. Franklin Carlson and entitled 'On Multiplicative Showers' (Carlson and Oppenheimer, 1937), which confirmed the change of attitude. Almost simultaneously, Nevill F. Mott (from Bristol) communicated the paper on 'The Passage of Fast Electrons and the Theory of Cosmic Showers' by Homi Bhabha and Walter Heitler to the *Proceedings of the Royal Society of London*, where it was received on 11 December and appeared in print (Bhabha and Heitler, 1937) nearly at the same time as the work of Carlson and Oppenheimer. As the American authors wrote in their paper, they had not only seen the letter of their European counterparts (Bhabha and Heitler, 1936) on the subject, but also the manuscript of their paper in the *Proceedings of the Royal Society of London*, and they commented: 'Their result differs from ours primarily because of ionization losses; apart from this the agreement between their values and ours is excellent.' (Carlson and Oppenheimer, 1937, p. 222, footnote 7)

Although the two investigations had been carried out independently, they referred essentially to the same set of data. Bhabha and Heitler emphasized the experimental results and their theoretical implications most clearly in the introduction, where they wrote:

> More recent experiments of Anderson and Neddermeyer [1936] have ... led them to revise their former conclusion, and their new and more accurate experiments show that up to energies of 300 million e-volts (the highest energies measured in their experiments) and probably higher, the experimentally measured energy loss of fast electrons is in agreement with that predicted theoretically. In fact, one may say that at the moment there are no *direct* measurements of energy loss by fast electrons which conclusively prove a breakdown of theory.... Under these circumstances, and in view of the experimental evidence mentioned above, it is reasonable ... to assume the theoretical formulae for energy loss and pair creation to be valid for all energies, however high, and work out the consequences which result from them. (Bhabha and Heitler, 1937, p. 432)

Carlson and Oppenheimer, on the other hand, began by saying:

> In nuclear fields, gamma rays produce pairs, and electrons lose energy by radiation. The formulae which have been deduced from the quantum theory give for the probability of these processes values which, for sufficiently high energies, no longer depend upon the energy of the radiation. Because of this, the secondaries, produced by a photon or electron of very high energy, will be nearly as penetrating as the primary, so that the primary energy will soon be divided over a large number of photons and electrons. It is this development and absorption of showers which we wish to investigate. (Carlson and Oppenheimer, 1937, p. 220)

Bhabha and Heitler reported that the crucial idea involved was first expressed by Lothar Nordheim in 1934; the latter did not derive any theoretical consequences at that time because of the anticipated certain failure of *QED* for very high energies (see Bhabha and Heitler, 1937, p. 434, footnote; also Nordheim, 1935). It was also Nordheim who communicated his results on the topic to Carlson and Oppenheimer (Carlson and Oppenheimer, 1937, p. 222, footnote). In any case, toward the end of 1937, Bhabha and Heitler as well as Carlson and Oppenheimer worked out the details and published their classical papers on the theoretical of 'absorption showers' or 'cascade showers.'

Evidently, the theory of electromagnetically (i.e., via *Bremsstrahlung* and pair creation) produced showers in air and other materials thus described by the 'standard' *QED* provided a great triumph of that theory, though it did not contribute to the solution of its fundamental deficiencies. These appeared only when divergent integrals resulted in the second and higher order perturbation approximations as the consequence of the emission and absorption of virtual photons and pairs. Throughout the period from 1933 to 1940 (and even beyond), the theoreticians attempted to come to grips on this fundamental issue, and some steps were taken toward what was later called 'renormalization theory.'[1040] In a review lecture on 'Paul Dirac: Aspects of His Life and Work,' Abraham Pais noted:

> The first steps towards renormalization go back once again to Dirac. In August 1933 [actually, on the 10th], he had written to Bohr: "Peierls and I have been looking into the question of the charge in the distribution of negative energy electrons produced by a static electric field. We find that this changed distribution causes a partial neutralization of the charge producing this field.... If we neglect the disturbance that the field produces in negative energy electrons with energies less than $-137\ mc^2$, then the neutralization of charge produced by the other negative electrons is small and of the order of 136/137.... The effective charges are what one measures in all low-energy measurements, and the experimentally determined value of e must be the effective charge of an electron, the real value being slightly bigger.... One would expect some small alterations in the Rutherford scattering formula, the Klein-Nishina formula, etc., when energies of the order of mc^2 come into play." (Pais, 1998, pp. 18–19)

Here, Dirac spoke about the difference between the 'real' value of the charge and the 'effective,' measured ones—denoting by 'real' the value of the charge which would exist in empty space undisturbed by any fluctuations of matter and charges created by negative-energy states—which, later on, people would rather name 'bare' and 'dressed' values. The essential point in the early debate on renormalization was that the charges thus calculated by Dirac, Heisenberg, Serber, and others (for the vacuum polarization, see besides Serber, 1936; Dirac 1934b; Heisenberg, 1934d; Uehling, 1935) remained finite. However, the theoreticians in the 1930s felt that much more had to be done in *QED* in order to arrive at a consistent description of natural phenomena. For instance, in lectures presented in late 1935 and early 1936, Pauli said that 'quantum theory—when dealing with systems possessing infinitely many degrees of freedom—causes difficulties to appear'; since 'the theory of holes postulates the existence of an infinite number of electrons,' it 'comes into the same category,' and further remarked:

[1040] The phrase 'renormalization' was perhaps first mentioned in Robert Serber's paper on the positron theory (Serber, 1936, p. 546), where he described Heisenberg's earlier method as 'chosen to renormalize the polarization of vacuum.' But that method alone did not fully characterize the theoretical development of the later renormalization procedure.

> It seems to me that our present methods are not fundamental enough, and there are two possibilities for overcoming the difficulties. The first is to change our concept of space and time in small regions. The second, to change the concept of state for systems with an infinite number of degrees of freedom.... I believe that the development of the theory along the correct lines will then lead to a numerical value of the fine structure constant $\alpha = e^2\hbar c = \frac{1}{137}$, and to an explanation of the fact that arbitrary high masses do not appear concentrated in any given space region in nature. It seems likely that the future theory will be unitary in the sense that the duality of light and matter will disappear. By this I will not claim that we shall necessarily explain one in terms of the other, but perhaps both in terms of some more fundamental concept. (Pauli, 1935–1936, pp. X–XI)

Pauli's arguments and hopes, as expressed here, were based on the ambitious programme, which he had considered together with Heisenberg since 1930 and of which the work on the problems of *QED* represented only a special aspect.[1041] Now, the first possibility sketched above by Pauli did not yield encouraging results—Heisenberg and Pauli discussed, e.g., the introduction of a quantized (lattice) structure in space for certain models of quantum field theory—but the second possibility occupied both of them for some time, though without any real success either. However, a third possibility still existed, not mentioned by Pauli, which became more evident only after the first half of the 1930s were over. As Weisskopf recalled later:

> Already in 1936 the conjecture had been expressed that the infinite contributions of the high-momentum photons were all connected with the infinite self-mass, with the infinite charge Q_0 [of the electron], and with the non-measurable vacuum quantities such as a constant dielectric coefficient of the vacuum. Thus it seemed that a systematic theory could be developed in which these infinities were circumvented. At that time, nobody attempted to formulate such a theory, although it would have been possible then to develop what is known as the method of renormalization. (Weisskopf, 1983, pp. 73–74)

Evidently, this definition of the concept of renormalization deviated a bit from what Dirac and Serber had had in mind earlier, because it explicitly addressed the infinite quantities in *QED*, which had so far been treated by the subtraction formalism. In contrast to what Weisskopf said later, in 1936, the experts in quantum electrodynamical theory were—as outlined above—far from succeeding in the programme of renormalization. One major step, for instance, consisted in formulating a Lorentz- and gauge-invariant *QED* scheme. Heisenberg and his collaborators, as well as Pauli, definitely insisted on the requirement of gauge invariance (see, e.g., Euler, 1936), but in general the quantum-field-theoretical perturbation methods of the 1930's were not fully covariant. On the other hand, Ernst C. G. Stueckelberg 'wrote several papers in which manifestly invariant formulation of field theory was put forward,' Weisskopf recalled and added:

[1041] See the correspondence carried on between Pauli and Heisenberg (in Pauli, 1985); for a brief historical account, we refer to Rechenberg, 1993b, especially, pp. 3–9.

> Unfortunately, his writings and his talks were rather obscure, and it was very difficult to understand them or to make use of his methods. He came frequently to Zurich in the years 1934–36, when I was working with Pauli, but we could not follow his way of presentation. Had Pauli and myself been capable of grasping his ideas, we might well have calculated the Lamb shift and the correction to the magnetic moment of the electron at that time. (Weisskopf, *loc. cit.*, p. 74)

Stueckelberg, who lectured until 1935 at the neighbouring University of Zurich, indeed submitted a paper in September 1934 on what he called a '*Relativistisch invariante Störungstheorie des Diracschen Elektrons* (The Relativistically Invariant Perturbation Theory of the Direct Electron)' to *Annalen der Physik*, in which he investigated in particular the high-energy collision phenomena between electrons and nuclei (Stueckelberg, 1934).[1042] Indeed, it took more than two years until, as Pauli wrote to Heisenberg about this work on 5 February 1937: 'Concerning the formalism of scattering theory, I wish to draw your attention to a paper of Stueckelberg (1934). This paper is not written very well, but the basic idea (which goes back to Wentzel) seems to me reasonable; it consists of establishing relativistic invariance by the fact that one removes space and time totally from the theory, and directly examines the coefficients of the four-dimensional Fourier expansion of the wave function.' (Pauli, 1985, p. 513) In the early years at Geneva, Stueckelberg explored Lorentz-invariant formulations of more general quantum field theories involving electrons, neutrinos and nuclear particles, aiming ultimately at a unified description of all the known elementary particles (Stueckelberg, 1938), which—as Weisskopf noted—could hardly be grasped by his colleagues.[1043]

Gregor Wentzel—Erwin Schrödinger's successor at the University of Zurich, and Stueckelberg's superior there—also became quite active in the fundamental problem of relativistic interactions between elementary particles, notably, in a series of three papers, entitled '*Über die Eigenkräfte der Elementarteilchen* (On the Self-Interactions of the Elementary Particles)' and submitted in fall 1933 to *Zeitschrift für Physik* (Wentzel, 1933b, c; 1934a). He departed from the Dirac–Fock–Podolsky version of the many-time quantum electrodynamics (which we have discussed in Section IV.3), and expanded the Maxwellian field of an elementary particle below its four-dimensional space-time surface into the interior. Wentzel claimed that 'In the interior of the light cone emerging from the particle

[1042] Ernst Carl Gerlach Stueckelberg von Breidenbach und zu Breidenstein was born on 1 February 1905, in Basel, Switzerland. From 1923 to 1926, he studied at the University of Basel and then at the *Technische Hochschule* in Munich, obtaining his doctorate with an experimental thesis on the properties of cathode rays in Basel. He then switched over to theoretical physics, and from 1927 to 1932, he worked on molecular problems (partly with Philip Morse) at Princeton University. Upon his return to Switzerland, he became a *Privatdozent* at the University of Zurich and then, in 1935, Professor of Theoretical Physics at the University of Geneva; beginning in 1942, he also taught courses at the University of Lausanne. He suffered from serious health problems, which caused some interruptions in his duties in Geneva, but he continued to function until his retirement in 1975. He died on 4 September 1984, in Geneva.

[1043] Stueckelberg's contribution to the meson theory of nuclear forces and other items will be mentioned below; for his further work on *QED*, see the Epilogue.

the field behaves quite differently from the outside. Especially for the classical limiting case ($\hbar = 0$), it will be shown that the field strengths assume at the world point of the particle *finite* limiting values if one approaches the origin from a time-like region.' (Wentzel, 1933b, p. 479) As a consequence, Wentzel obtained for the self-force of a point particle an expression exhibiting no electromagnetic inertial force but only a radiation damping. Still, in the quantum-theoretical evaluation of the particle's self-energy, a quadratic divergence remained, at least for empty negative-energy states (i.e., in a non-hole theory)—the divergence disappeared only in the classical limit if external forces were absent (Wentzel, 1933c). In spite of such remaining problems, the hope was raised that Wentzel's so-called 'λ-limiting process' might help to improve the situation in the hole theory, and even improve upon the logarithmic divergence found by Weisskopf (1934a) and corrected by Furry (Weisskopf, 1934b).[1044] Further, the resolution of the infrared divergence, an originally logarithmic divergence, by the methods of Bloch and Nordsieck (1937) or Pauli and Fierz (1938), respectively (see above), encouraged the optimism of the theoreticians to arrive ultimately at a consistent, even finite, *QED*. Another sign of the optimism might have emerged from a completely different approach which Hendrik Kramers took in Leyden, proceeding along paths quite isolated from the rest of quantum field theorists. He indeed promoted essentially the concept of renormalization, as outlined above by Weisskopf.

From the very beginning, Kramers had expressed unhappiness about Paul Dirac's radiation theory of 1927 and his relativistic electron theory of 1928.[1045] In contrast to Dirac, Kramers did not wish to make too abrupt and too radical alterations away from the classical electron theory of Hendrik Lorentz, but rather proposed a cautious step-by-step procedure in order to construct the new *QED* scheme. 'The concepts of Dirac are sufficient for everyday use, for most purposes the photon idea of Einstein is incorporated in an acceptable manner,' Kramers argued in fall 1931 in his inaugural lecture at the Technical University of Delft (where he was appointed extraordinary professor of theoretical physics in addition to his professorship in Utrecht); but, he cautioned: 'The problem of principle—which is the complete synthesis of quantum theory and relativity—remains unsolved and is left untouched.'[1046] Only several years later—in the meanwhile (in 1934), he had moved to Leyden to take up the chair of theoretical physics previously held by Lorentz (and his successor Paul Ehrenfest)—Kramers presented his views deviating from the accepted *QED* more explicitly. Thus, in the preface of an extended account of the foundations of quantum theory and the theory of electrons and radiation (forming Volume 1 of the *Hand- und Jahrbuch der chemischen Physik*), which he completed and signed in August 1937, Kramers emphasized 'in particular the fact that Dirac's radiation theory cannot be considered

[1044] See Weisskopf 1934a, p. 27, footnote 1, together with the correction in Weisskopf, 1934b.

[1045] See Max Dresden's biography of Hendrik Kramers (1987), Chapter 16, for detailed information concerning this matter.

[1046] See Kramers' inaugural lecture of 30 October 1931, quoted according to Dresden, 1987, p. 336.

right off as a quantization of the classical electron theory but is—in contrast to it—only able to deal with the "secular" interaction of radiation and particles' (Kramers, 1938a, p. VI). He elaborated on this point at the Luigi Galvani Bicentennial Celebration—held in October 1937 in Bolognia, Italy—as follows:

> In a recently published work, I have developed the fundamental relations of the quantum theory of interaction between the radiation field and charged particles in a way that is quite different from the usual presentations in the literature ... I have tried to display the theory in such a way that the problem of the structure and finite extension of the particles does not occur explicitly, and that the quantity, which is introduced as "particle mass," is identified from the very beginning with the experimental mass. Notably I depart—for the moment we talk purely in classical terms—from the phenomena where a charged particle moves in an external electromagnetic field and where the emission and reaction of the radiation can be neglected ("quasistationary motion"). This motion is governed by a Hamiltonian which I call $H^{(\mathrm{mat})}$, and in this function the experimental mass occurs: one might even say that by $H^{(\mathrm{mat})}$ the use of the concept of mass is defined. $H^{(\mathrm{mat})}$ depends on the space variables x, y, z (vector $\mathbf{r}$) and time, on the one hand, and on the component of the momentum vector $\mathbf{p}$, on the other. Because of the gauge invariance $\mathbf{p}$ occurs in the combination $\mathbf{p} - \frac{e}{c}\mathbf{A}^{\mathrm{ext}}$, where $\mathbf{A}^{\mathrm{ext}}$ represents the vector potential of the field at the position of the particle, hence a function of x, y, z, t. (Kramers, 1938b, pp. 108–109)

Kramers then explained his deviating interpretation of electrodynamics by writing explicitly the Hamiltonian function for a system of radiation (with the Fourier coefficients a'_λ and b'_λ, and the wave vector $\boldsymbol{\sigma}'_\lambda$) interacting with the charged particles via $H^{(\mathrm{mat})}$; i.e.,

$$H = \frac{1}{8\pi}\sum_\lambda \boldsymbol{\sigma}_\lambda^2 (a'^*_\lambda a'_\lambda + b'^*_\lambda b'_\lambda) + H^{(\mathrm{mat})}. \tag{748}$$

In the usual radiation theory, Kramers said, the difference between the external field (denoted by the primes) and the total field (which is the sum of the external field and the proper field of the particles) was neglected; hence, one replaced the primed components by the nonprimed ones. However, he criticized this procedure, and warned: 'Quite apart from the divergence difficulties [connected with the proper fields] one must criticize Eq. [(748)], because the transverse part of the electromagnetic mass is now counted twice, at least if m and $H^{(\mathrm{mat})}$ should represent the external mass.' (Kramers, *loc. cit.*, pp. 110–111) Hence, in his new theory, Kramers carefully discussed the proper field of the particle and interpreted Eq. (748) by clearly identifying the a'_λ and b'_λ with the external field, and:

> We therefore can interpret h as the sum of the energy of the external field and $H^{(\mathrm{mat})}$, which we ascribe to the particle's quasistationary motion; the latter contains implicitly the energy of the proper field, because the kinetic energy of the potential, expressed with the help of the experimental mass, enters into $H^{(\mathrm{mat})}$. While in the former interpretation of H certain energy terms were doubly counted, one must say

that in the present interpretation a certain part of the total energy is discarded, namely that energy which in the full expression $\frac{1}{8\pi}\int(\mathscr{E}^2 + \mathscr{H}^2)\,dV$ corresponds to the inner products of the scalar field and the proper field. (Kramers, *loc. cit.*, pp. 112–113)

Moreover, in the new interpretation, H automatically embraced the reaction of the external field on the particle (though for secular motions only). The quantization of Eq. (748) then showed: The interactions of the proper fields are included in $H^{(\mathrm{mat})}$ and would 'not appear, as in the usual quantum electrodynamics, as a consequence of the quantized field theory' (Kramers, *loc. cit.*, p. 113).

Kramers displayed details of the treatment outlined above, especially in Sections 89 and 90 of his *Handbuch* article (Kramers, 1938a, pp. 448–464). One should not say that the difference between this new approach to *QED* and the previous one consisted just in fine subtleties. Max Dresden actually pointed out that Kramers took care of at least three points which he had criticized in Dirac's theory, namely:

(1) The occurrence of divergences in Dirac's theory was objectionable to Kramers. He was unhappy and concerned about the divergence of zero-point energy, but he was especially critical of the result (first obtained by Oppenheimer [1930a]) that the Dirac Hamiltonian and the Dirac theory led to an infinite shift of the spectral lines of an atom in a radiation field.
(2) To Kramers ... it was particularly upsetting that the relation between the Dirac theory and the Lorentz electron was very tenuous. A naive application of the Bohr correspondence principle to the Dirac theory does not yield the correct correspondence limit.
(3) Kramers was enormously impressed by Lorentz's discussion of the electromagnetic mass of the electron. He felt that Dirac had not made sufficiently precise distinction between the electromagnetic mass and the experimental mass.... He simply could not accept a theory in which the famous Lorentz radiation term $\frac{2}{3}(e^2/c^3)\,\dddot{\mathbf{x}}$, which classically is responsible for electromagnetic radiation, would not have a simple straightforward quantum-mechanical interpretation. (Dresden, 1987, pp. 339–340)

Although the principles of Kramers's criticism of the standard *QED* and of his own attempts were quite clear—and even shared by some theoreticians, including Pauli—neither he nor anyone else pushed the programme outlined above much further.[1047] The historical development in the following years rather proceeded on the basis of what Kramers called 'the Dirac theory.' Thus, the *Physical Review*

[1047] In general, Kramers did not publish much at that time. In connection with *QED*, we may just refer to an earlier paper on Dirac's hole theory, in which Kramers pointed out 'that a correction must be applied to the energy values of the stationary states of the hydrogen atom, as given by the Dirac theory of 1928' (Kramers, 1937, p. 823); but he did not present here or later the promised calculation of this correction. In another paper, which J. Serpe of the University of Liège published, he made use of Kramers's '*théorie rectifiée*' in order to remove the infinite level shifts derived by Wigner and Weisskopf (1930a, b) in their calculation of the line widths (Serpe, 1940).

carried two articles in the first half of 1939 that pursued this line, one submitted by Victor Weisskopf in April and the other by Sidney M. Dancoff in March. Weisskopf essentially reviewed the status of his old problem of the electron mass and assembled arguments that the higher order (than the second in the fine structure constant) approximations would also not diverge more strongly than logarithmically (Weisskopf, 1939, especially, p. 85). Dancoff, in an extended note 'On Radiative Corrections for Electron Scattering' investigated—stimulated by his teacher Oppenheimer and Felix Bloch—as 'to what extent the inclusion of relativistic effects modifies the conclusions of Pauli and Fierz,' who had treated in their paper on infrared divergence (Fierz and Pauli, 1938), the motion of charged particles in a nonrelativistic approximation (Dancoff, 1939, p. 960). After carrying out a detailed calculation, Dancoff arrived at three types of terms—(A), (B), and (C)—and found: 'For a Dirac electron ... while terms (A) converge, terms (C) contribute a *positive* logarithmic divergence; it is to be remembered that nonrelativistically the divergence was negative, indicating an infinite cross section.' (Dancoff, *loc. cit.*, p. 963) In his historical account of development of *QED*, Silvan S. Schweber described Dancoff's result as follows:

> He thus obtained a divergent result and calculated that in hole theory a *new type* of divergence occurred in the radiation corrections to the elastic scattering of an electron by an external field ... divergences that would later be called "vertex function" divergences. When combined with pieces of self-energy divergences ... these divergences cancel one another. (Schweber, 1994, p. 60)

Dancoff, however, made a mistake in his calculation by omitting the contribution of the Coulomb interaction terms.[1048] 'Why did no one redo Dancoff's calculation at that time?' lamented Schweber, and claimed: 'Had it [been] done so, the difficulties of *QED* might have been resolved much earlier.' (Schweber, 1994, p. 91)

Evidently, the time was not really ripe to make essential progress in renormalizing *QED* already at the end of the 1930's. The quantum theoreticians were concerned with many other problems of high-energy physics at that time: They investigated different field theories—rather than just *QED*—and applied them to particles other than the electron, also those observed in cosmic radiation (see below). Even indications of deviations from the standard result of Dirac's relativistic theory of the electron on the fine structure of the hydrogen and deuterium spectral lines H_α and D_α, studied by William V. Houston (1937) and Robley C. Williams (1938) (see also R. C. Williams and R. C. Gibbs, 1934), and analyzed by Simon Pasternack (1938) did not change the outlook. Thus, Pasternack, then at Caltech, noted that 'these deviations [of the D_α-line, as observed by Williams] are consistent with a perturbation of the 2^2S-level of deuterium,' and that 'an S-level displacement of this magnitude checks quite well with discrepancies observed in the doublet separations of other Balmer lines of hydrogen.' He argued further:

[1048] Actually, Robert Serber reminded him of this omission (see Dancoff, 1939, p. 962, footnote *).

> A displacement of the S-levels would seem to point toward some perturbing interaction between the electron and the nucleus.... An estimate of the magnitude of the interaction can be obtained by superposing on the Coulomb field a simple repulsive potential of height D, extending for a distance r_0 from the nucleus. A first order perturbation treatment raises the energy of the n^2S-level of a hydrogen-like atom by an amount $\frac{4}{3}D\frac{Z^3r_0^3}{n^3a_0^3}$, where a_0 is the Bohr radius. If we assume a displacement of the $2S$-level of deuterium of about 0.3 cm^{-1}, as suggested by Williams' results, we find that ... D would have to be given the extremely high value of about 100 MeV. (Pasternack, 1938, p. 1113)

These observations and further, more accurate results, obtained by new experimental methods later after World War II, would immediately stimulate the first breakthrough to renormalized *QED*.

Also, in the late 1930s, theoretical ideas for removing divergences in different ways surged forward. For example, Dirac—previously reproached by Kramers to have abandoned too much of Lorentz's classical electron theory—constructed a classical theory of radiation, involving an electron of finite size (in the interior of which signals could be transmitted faster than the speed of light), as the correspondence limit of a new *QED* (Dirac, 1938b; see also Pryce, 1938). While this proposal could be related conceptually to that of Gregor Wentzel of 1933, which we have already mentioned, an earlier one of Max Born's changed the classical basis of electrodynamics even more drastically: In particular, he replaced the Maxwell equations by nonlinear field equations (see, e.g., Born and Infeld, 1934a, b; 1935). But in spite of great efforts, all attempts to quantize these equations failed; on the other hand, from the hole theory, such nonlinear terms seemed to follow directly; hence, *QED* did not really have to begin with a corresponding nonlinear classical theory.

(c) New Fields Describing Elementary Particles, Their Properties, and Interactions (1934–1941)

In an address, delivered at the Indian Science Congress on 8 February 1936, Megh Nad Saha discussed 'The Origin of Mass in Neutrons and Protons' (Saha, 1936). He drew attention to the difference between the electromagnetic mass of the electron (due to Lorentz's theory) and the masses of the nuclear constituents, and proposed to look at the neutron (due to an idea of D. S. Kothari) as being 'composed of two equal and oppositely charged free magnetic poles' (Saha, *loc. cit.*, p. 146). He also mentioned other attempts to explain the ratio of the proton mass to that of electron mass, as being 'one of the outstanding fundamental problems of physics,' especially the rather speculative one of Arthur Stanley Eddington, which the latter had discussed for a number of years in the literature. According to Eddington, the charged elementary particles, electron and proton, could be described in a multi-dimensional mathematical space, and their masses resulted from the equation

$$10m'^2 - 136m + 1 = 0, \tag{749}$$

such that the ratio of the two roots assumed the particular value 1847.60, close to the actually observed one (Eddington, 1931, p. 529). In 1936, Eddington generalized his equation to

$$10m^2 - 136mm' + m'^2 = 0, \tag{749'}$$

where m' now denoted the mass of neutral scalar particle, while m remained associated with the electron and proton having half-integer spin [Eddington, 1936, Eq. (12.47)]. Two years later, Herbert Charles Corben, then at Trinity College, Cambridge, pointed out that the scalar object whose mass computed from Eq. (749′) was $m' = 135.9m_e$ might be interpreted as follows: 'If this particle were to combine with an electron or a positron with the emission of a neutrino, it would yield a heavy negative or positive electron obeying Bose statistics and with a mass between 136 and 137 times that of an ordinary electron.' He further remarked: 'This result is so closely in agreement with the *U*-particle theory of Yukawa (1935) and Bhabha (1938a), and others, which is in turn supported by facts so far as present accuracy goes ... [hence] Eddington's theory merits more attention than is usually given to it.' (Corben, 1938, p. 747)

However, Eddington's speculative theory of 1931 and the following years did not win the approval of most of his colleagues, and Max Born in a later lecture just mentioned 'a few coincidences ... which are not true predictions, but expressions of known quantities' (Born, 1943, p. 38). Born, adding that another prediction of the same theory was a value of the fine structure constant and, like others, he mocked: 'Now at that time when Eddington began his work the experimental value of $hc/2\pi e^2$ was near to 136. Later experiments indicated a larger value, and today it is very near 137. Accordingly Eddington adapted his theory by adding [quite arbitrarily without proper motivation] a unit.' (Born, *loc. cit.*) Still, the references occasionally made to Eddington's numerology in the second half of the 1930s (and even afterward) indicate how hard were the problems that the theory then faced and how desperately one was looking everywhere for a solution in the theory of elementary particles.[1049] As we have mentioned earlier, the hope of determining the fine structure constant from quantum field theory had greatly driven Heisenberg and Pauli in their efforts, and this hope was especially stimulated by the appearance of new elementary particles—such as the neutron, the positron, and the neutrino—upon the scene. Thus, on 21 January 1934, Pauli wrote to Heisenberg about a new development in the context of this problem:

[1049] In 1937, even Paul Dirac considered another proposal to explain the 'cosmological constants,' such as the ratio of the electrodynamical to the gravitational force for an electron (about 10^{39}), or the ratio of the mass of the proton to the mass of the universe (about 10^{-78}) by an ingenious hypothesis involving a change of these constants in time during the evolution of the universe (Dirac, 1937a, b; 1938a). This speculation even gave rise to much more serious discussion among the physicists than Eddington's (see, e.g., Kragh, 1991, for a historical review).

> I entirely agree with your conviction that the solution of quantum electrodynamics (the self-energy difficulty) lies along the direction which we discussed in Brussels. A light-quantum must consist of a neutrino and a neutrino hole, like [Louis] de Broglie wants to have it, and the neutrino mass must be zero.... I also believe that the Fermi Hamiltonian for β-decay and the usual quantum electrodynamics must be understood in a unified manner.... I very much wish to stimulate you *to think further about the neutrino and quantum electrodynamics*, for I believe that the solution cannot be [much] farther anymore. (See Pauli, 1985, p. 256)

In a note presented to the *Académie des Sciences* (*Paris*), Louis de Broglie had just proposed a 'neutrino theory of light' (de Broglie, 1934a), which both Werner Heisenberg and Wolfgang Pauli found 'very suggestive' and which supported their own desire to establish a relation between Enrico Fermi's β-decay constant (derived experimentally) and the fine structure constant. Indeed, they devoted—for a time—part of their following exchange of correspondence to that subject. The Heisenberg–Pauli exchange started with a letter from Pauli of 19 January 1934, in which he wrote: 'In the *Comptes rendus* of 8 January 1934 ... there has appeared a rather interesting note of de Broglie, in which he discusses the point of view that the photon is composed of two neutrinos. It seems to me that the main problem is to formulate in a reasonable manner the *interaction terms of neutrinos and electrons* in the Hamiltonian. One cannot grasp *a priori* how the particular neutrino pairs which stick together and build up the photon occur much more easily than any two neutrinos having different directions of momenta and different energies.' (Pauli, 1985, pp. 253–254) Although, in 1934, Louis de Broglie wrote several notes and papers on this idea, in which certain results about the equation of motion and spin of the photon—when composed of neutrinos—were derived (de Broglie, 1934, b, c; de Broglie and Winter, 1934), he did not answer Pauli's question. However, a number of other theoreticians also picked up the idea and worked out certain consequences.[1050]

After Gregor Wentzel at the University of Zurich demonstrated in a paper submitted in October 1934 that 'the fundamental equations of electrodynamics can indeed be derived from a formal scheme, in which the electromagnetic field quantities enter as operators representing the creation and destruction of *pairs of corpuscles*' (Wentzel, 1934b, p. 337), Pascual Jordan (then in Rostock) entered into the discussion by addressing the 'key problem (*Kernfrage*), namely the emergence of *Bose statistics* for the light-quanta if one starts with *Fermi statistics* for the fundamental objects' (Jordan, 1935a, p. 465). This question was not answered by simply putting together two spin-$\frac{1}{2}$ neutrinos, but the result now followed from a one-dimensional model of Jordan. Ralph Kronig from Groningen proceeded along the same lines in a series of investigations submitted to *Physica* from March to August 1935: He established the statistical relationship between the number of

[1050] For details of the neutrino theory of light in the 1930s, see Brown and Rechenberg, 1991a, or Brown and Rechenberg, 1996, Section 4.3.

light-quanta and the number of neutrinos and their energies (Kronig, 1935a), and he further pointed toward a relation with Fermi's β-decay theory by noting 'radiation-free states of the neutrinos field having a finite neutrino density' (Kronig, 1935b; see also, 1935c). Jordan and Kronig, partly in collaboration, continued to pursue the consequences from the neutrino theory of light in the following year, but at the end of 1936, Vladimir Fock of Leningrad criticized their results because of two objections: First, the light-quantum field thus emerging depended quadratically on the neutrino field and could not therefore satisfy any linear differential equation; second, the photon operator constructed by Jordan and Kronig would commute with its conjugate operator (Fock, 1936). However, Ernst C. G. Stueckelberg of Geneva immediately countered the latter statement by referring to the fact that Fock's conclusion did not apply to the actual situation, where infinitely many neutrinos were required to construct one photon (Stueckelberg, 1937a).

The discussion on the neutrino theory of light still occupied physicists for a number of years, notably, after Max Born and S. N. Nagendra Nath in Bangalore, India, and M. H. L. Pryce in England joined the fray. Evidently, the challenge of the scheme lay in the expectation 'that it might be possible to dress quantum electrodynamics in such a form that the present role of light-quanta can now be taken over by particles or pairs of particles which behave in a higher measure according to the manner of ordinary corpuscles (say, similar to Dirac electrons), and that one might thus arrive on a wave-mechanical basis of a new type "unitary" theory of matter and field' (Wentzel, 1934b, p. 337). Indeed, if one took the prevailing concept of nuclear forces as being described by the Fermi-field theory (which we have outlined in Section IV.3), the connection between light and neutrinos suggested the possibility of obtaining eventually a unified quantum field theory of all electromagnetic and nuclear forces. A step toward this goal could be discerned in the efforts of Werner Heisenberg to draw further consequences from the Fermi field theory by seeking to predict the occurrence of a certain phenomenon in cosmic radiation. He addressed them first in a letter to Pauli dated 26 May 1936 (see Pauli, 1985, pp. 445–446). Heisenberg wrote that by taking the familiar quantum-electrodynamical description, the probability for creating n electron–positron pairs in a high-energy process turned out to be smaller by a factor $(e^2/\hbar c)^{n-1}$ than the probability for creating one pair, independently of the energy of the incident object. He continued:

> Entirely different is the situation in Fermi's theory. If one puts there, according to Uhlenbeck-Konopinski, [for the Hamiltonian],
>
> $$\varepsilon = \int \psi^* \alpha_i \frac{\partial}{\partial x_i} \psi + \cdots + \frac{g}{\hbar c} \psi^*_{\text{proton}} \psi_{\text{neutron}} \psi^*_{\text{electron}} \left(\frac{\partial}{\partial x} \psi_{\text{neutrino}} \right) dV, \qquad [(750)]$$
>
> then the constant $g' = g/\hbar c$ has the dimension of cm^3. For large energies, where one can neglect the rest-mass of the particles, this implies: every perturbation method is an expansion in g'/λ^3, with λ denoting the wavelength of the particle involved. Hence

> it follows: for large energies the interaction term becomes decisive; in particular, now the processes in which many particles are emitted simultaneously turn out to be not far less probable than processes in which only one or two particles are emitted; below a wavelength $\lambda = \sqrt[3]{g'}$, therefore, "showers" of particles must be expected. *Hence it seems to me that one can understand the existence of cosmic-ray showers on the basis of the Fermi-field theory.* (Heisenberg to Pauli, 26 May 1936, in Pauli, 1985, pp. 445–446)

Heisenberg addressed here, as he would explain in greater detail in a paper—entitled '*Zur Theorie der "Schauer" in der Höhenstrahlung* (On the Theory of "Showers" in Cosmic Radiation)' and submitted early in June 1936—the cosmic-ray processes exhibiting the creation of a large number of secondary particles, which had been observed since several years, especially by Gerhard Hoffmann and his collaborators (Heisenberg, 1936b, p. 533). Already in 1928, Hoffmann had reported the existence of 'spontaneous bursts' in cosmic radiation when registered at high altitudes (Hoffmann and Lindholm, 1928). This phenomenon—often called '*Hoffmannsche Stöße* (Hoffmann bursts)'—was later experimentally studied by many experts, especially in Germany and the United States, and several, partly conflicting, conclusions about their nature had been suggested. Heisenberg's new explanation of the bursts as the simultaneous production of multiple pairs of neutrinos and electrons, or 'explosive showers' as he called them, received a mixed reaction from his theoretical colleagues. While Bhabha and Heitler, in their paper on cascade showers, described the theory of explosive showers as 'elegant' and said that it might well explain the largest showers observed in cosmic rays (Bhabha and Heitler, 1937, p. 435), Carlson and Oppenheimer claimed that it was 'without cogent experimental foundation' and 'in fact rests on an abusive extension of the theory of the electron-neutrino field' (Carlson and Oppenheimer, 1937, p. 221). At the same time, Heisenberg assembled further experimental evidence in support of his views; in a letter to Pauli, dated 18 December 1936, he reported about a new Hungarian work on shower formation at large depths (Barnóthy and Forró, 1937). 'It is shown,' he wrote, 'that there exists a non-ionizing shower producing radiation of absorption coefficient $\mu = 2.1 \times 10^{-5}$ cm^2 g^{-1} (corresponding to a cross section of about 10^{-28} cm^2),' and said: 'Light-quanta can scarcely have such penetrating power (according to Bethe certainly not); neutrons also surely not, hence Barnóthy and Forró conclude that we are dealing with neutrinos. That seems to me to be quite convincing.' (Heisenberg, in Pauli, 1985, p. 491) When Jørgen Bøggild, in his Copenhagen doctoral thesis, arrived at the conclusion that the 'Hoffmann bursts' nevertheless could be described by the cascade theory (Bøggild, 1937; also Bøggild and Karkov, 1937), Heisenberg argued in a letter to Niels Bohr:

> It seems as though many showers can only originate via cascades; however, it seems certain ... that the explosive type of showers does occur ... a thousand times less frequently than cascades, and that the "bursts" have mainly this origin. (Heisenberg to Bohr, 5 July 1937)

The same conclusion was arrived at by Hans Euler, the theoretical expert on bursts in Leipzig, who analyzed the situation in his *Habilitation* thesis (Euler, 1938b, c) in close cooperation with the experimentalist Gerhard Hoffmann who, meanwhile, had become Peter Deybe's successor in the experimental chair at Leipzig. Euler admitted that a part of the 'bursts' might be ascribed to electromagnetic cascades, but a substantial fraction (dominant both below very thin and very thick absorbers) was clearly 'non-cascade bursts created in an explosive manner' (Euler, 1938c, p. 692). Indeed, exactly the very penetrating, 'hard component' of cosmic radiation appeared to be connected with the explosive creation of many particles, which most probably were neutrinos. However, the understanding of the hard component of cosmic rays changed quickly at that time, and in 1939, Heisenberg would develop a different approach to explosive showers via the vector-meson theory (see below).

Clearly, behind Heisenberg's work on cosmic-radiation showers in 1936, there lay the desire to unify the description of nuclear forces (first via the Fermi-field theory), maybe even with the electromagnetic forces (via de Broglie's neutrino theory of light). An even stronger push into the same direction of a unified theory of all elementary particles was attempted by Ernst Stueckelberg, who expounded his ambitious programme in a short letter to *Nature* on 7 May 1936:

> The hypothesis is put forward that positive electron, neutrino, positive proton and neutron are four different quantum states of one elementary particle. Such an assumption would be trivial unless transitions between the different states occur. It is required that Dirac's equation follows from the theory, and that the conservation law of electric charge holds, so only a small number of transitions are allowed. If in addition we satisfy a certain symmetry condition (corresponding to the conservation law of Jordan's neutrino charge [Jordan, 1936, §2]) the number of possible processes is further reduced. (Stueckelberg, 1936a, p. 1032)

Stueckelberg then wrote a list of transitions (including that of a positive electron into a neutrino, or of a proton into a neutron), which 'occur only if another [transition] takes place in the reverse direction;' hence, the entire process satisfies the symmetry conditions, and he concluded by saying: 'As soon as the neutrino theory of light can be formulated in a satisfactory way, we have a unitary field theory, its variable being a spinor of 16 components.' (Stueckelberg, *loc. cit.*, p. 1032) Stueckelberg published the details of the development of these ideas in the following months (Stueckelberg, 1936b, c). After the discovery of the mesotron, he sent a letter to the *Physical Review*, welcoming the new particle and placing it into the unified field theory (Stueckelberg, 1937b); he then wrote two papers on '*Die Wechselwirkungskräfte in der Elektrodynamik und in der Feldtheorie der Kernkräfte* (The Forces of Interaction in Electrodynamics and in the Field Theory of Nuclear Forces),' where he introduced a

16-component spinor field to describe the structure of matter (Stueckelberg, 1938).[1051]

In contrast to Stueckelberg's far-reaching field-theoretical speculations, the investigations conducted at other places (with the possible exception of some of Heisenberg's) seemed to be more modest and followed conventional paths, though they served the same goal, namely, to understand the nature of high-energy processes and the elementary particles involved. Wolfgang Pauli announced the first such work in a letter to Werner Heisenberg, dated 14 June 1934, as follows:

> My own physics in the meanwhile has turned out to be completely negative (*not* because of my laziness). Still I have hit upon a kind of curiosity about which I would like to tell you. If, instead of Dirac's [equation], one assumes as the basis the old scalar Klein-Gordon relativistic equation, it possesses the following properties:
>
> 1. The charge density
>
> $$\rho = \psi^* \left(\frac{\hbar}{i}\frac{\partial \psi}{\partial t} - e\Phi_0\psi\right) - \left(\frac{\hbar}{i}\frac{\partial \psi^*}{\partial t} + e\Phi_0\psi^*\right)\psi \qquad [(751)]$$
>
> may be both positive and negative.
>
> 2. The energy density
>
> $$\left|\frac{\hbar}{i}\frac{\partial \psi}{\partial t} - e\Phi_0\psi\right|^2 + \sum_{k=1}^{3}\left|\frac{\hbar}{i}\frac{\partial \psi}{\partial t} + e\Phi_k\psi\right|^2 \qquad [(752)]$$
>
> is always ≥ 0, it can never be negative [with Φ_0 and Φ_k denoting the electromagnetic potentials].
>
> This is exactly the opposite situation as in Dirac's theory, and exactly what one wants to have.—Then I could easily show: the application of our old [Pauli-Heisenberg] field quantization formalism to *this* theory leads with *without any further hypothesis* (without the "hole" idea, without limit-aerobatics, without subtraction physics!) to the existence of positrons and to processes of pair-creation with an easily calculable frequency. Furthermore, this works for both Einstein-Bose and Fermi-Dirac statistics. (In the first case one must drop a zero-point energy of matter analogous to the zero-point energy of radiation.)—Now I'll let Weisskopf check whether an (eventually finite) polarization of the vacuum follows or not in the theory. (Pauli, 1985, p. 328)

In the same letter to Heisenberg, Pauli regretted that the 'much more satisfactory scalar-wave theory would not represent reality, as one could not include spin in a relativistic way without running again into the negative-energy states difficulty,' and he concluded by saying, 'Therefore, practically one cannot achieve much with this curiosity,' and added: 'Still I am happy to beat again my old enemy, the Dirac theory of the spinning electron (*aber es hat mich doch gefreut,*

[1051] We shall come back to this work below.

daß ich immer meiner alter Feindin, der Diracschen Theory des Spinelektrons, wieder eins anhängen konnte).' (Pauli, *loc. cit.*, p. 328)[1052] Two weeks later, Pauli sent Heisenberg the manuscript of a joint paper with Weisskopf inviting his criticism. Besides emphasizing the positive points (in the accompanying letter), he also reported about an important 'negative' result, namely:

> When quantizing the scalar wave equation in accordance with the exclusion principle in *the present form*, one *cannot* achieve that simultaneously: 1. relativistic and gauge invariance exist; 2. the energy eigenvalues come out positive (in the quantization according to Bose statistics both are fulfilled). (Pauli to Heisenberg, 28 June 1934, in Pauli, *loc. cit.*, p. 335)

Pauli therefore regretted that the Heisenberg–Pauli quantization scheme for fields was not general enough to admit the two quantum statistics for arbitrary Hamiltonians and wave fields, and he still hoped to succeed in this goal, i.e., to find 'a reasonable change of the quantization rules in field theory' (Pauli, *loc. cit.*). The published paper, which was received by *Helvetica Physica Acta* on 27 July 1934, did not fulfill this hope, however, as the authors admitted: 'For the particles the statistics of symmetrical states (Einstein-Bose statistics) must be assumed.' (Pauli and Weisskopf, 1934, p. 709)[1053] Besides the treatment of the scalar quantum field theory with Bose statistics, the paper contained at the end Weisskopf's calculation of the vacuum polarization, which yielded the result for the density averaged over the directions of the momentum K

$$\bar{\rho}(x) = K\Delta\Phi_0 + \text{finite terms}, \tag{753}$$

[1052] As Weisskopf recalled:

> Note that at the time the method of exchanging the creation and destruction operators (for negative energy states) was not yet in fashion; the hole theory of the filled vacuum was still the accepted way of dealing with positrons. Pauli called our work the "anti-Dirac paper." He considered it a weapon in the fight against the filled vacuum that he never liked. We thought that this theory only served the purpose of a nonrelativistic example of a theory that contained all the advantages of the hole theory without the necessity of filling the vacuum. We had no idea that the world of particles would abound with spin-zero entities a quarter of a century later. That was the reason we published it in the venerable but not widely read *Helvetica Physica Acta*. (Weisskopf, 1983, p. 70)

[1053] As Weisskopf also recalled:

> The work on the quantization of the Klein-Gordon equation led Pauli to the famous relation between spin and statistics. Pauli demonstrated in 1936 the impossibility of quantizing equations of scalar or vector fields that obey anticommutation rules. He showed that such relations would have the consequence that physical operators do not commute at two points that differ by a space-like interval. This would be in contradiction to causality because it would require that measurements interfere with each other when no signal can pass from one to the other (Pauli, 1936). Thus Pauli concluded that particles with integer spin could not obey Fermi statistics. They must be bosons. During the days of the hole theory it was obvious that particles with spin-1/2 could not obey Bose statistics because it would be impossible to "fill" the vacuum. Four years later Pauli proved the necessity of Fermi statistics for half-integer spins, also on the basis of causality arguments [W. Pauli, 1940]. (Weisskopf, 1983, p. 70)

with

$$K = \frac{1}{12\pi^2} \frac{e}{\hbar c} \int \frac{d|k|}{|k|}. \tag{753a}$$

Pauli and Weisskopf commented: 'The induced charge density has the opposite sign as the external density $\rho_0 = -\frac{1}{4\pi}\Delta\Phi_0$, and it is proportional to the latter, with diverging proportionality factor $4\pi K$; hence any external charge would be totally compensated by the induced one. This result completely agrees with Dirac's, as computed in his hole theory; even the factor K of the diverging terms comes out to be the same.' (Pauli and Weisskopf, *loc. cit.*, p. 731)

In spite of having arrived happily at a quantum field theory different from Dirac's to describe the essential features of electrodynamics (and matter theory), Pauli and Weisskopf did not yet know whether any elementary particles existed that could be described by their relativistic scheme.[1054] About two years later, the Romanian-born Alexandre Proca in Paris submitted a paper, entitled '*Sur la théorie ondulatoire des eléctrons positifs et négatifs* (On the Wave Theory of Positive and Negative Electrons)' to *Journal de physique et le radium*, in which he proposed a new relativistic- and gauge-invariant wave equation exhibiting the following properties:

> The wave function does not have more than four components (which form a world vector); it is possible to define a current which satisfies a conservation equation, and a charge which can be positive or negative, of the type that the theory also embraces well the case of positrons and electrons; one can write down a systematic energy-momentum tensor which satisfies a continuity equation and where energy states are always positive; and finally, one can define the magnetic moment of the particles, as well as their spin. (Proca, 1936, p. 347)

The fundamental equation, which Proca presented in his paper, read

$$\Box\Phi_r - k^2\Phi_r = 0, \tag{754}$$

(with $\Box = \frac{\partial}{\partial t^2} - \frac{\partial}{\partial x^2} - \frac{\partial}{\partial y^2} - \frac{\partial}{\partial z^2}$), and the field components Φ_r $(r = 0, 1, 2, 3)$ could be derived from a scalar ψ by taking time and spatial derivatives, the ψ satisfying the Klein–Gordon equation

$$\Box\psi = k^2\psi, \tag{755}$$

where $k = \frac{mc}{\hbar}$ and m denoted the mass of the particle. He was then able to take over essentially the Pauli–Weisskopf quantization procedure, but it must be con-

[1054] They excluded the α-particle as a possible candidate because of its composite structure and the nuclear forces, which 'might lie completely outside the validity of the domain of the present quantum theory.' (Pauli and Weisskopf, 1934, p. 713) sidered as quite remarkable that it took him more than a year to admit that Eq. (754), which would later be called the 'Proca equation' (by Nicholas Kemmer, see below), would not describe electrons and positrons.[1055] By that time, however, the experiments had discovered an object that seemed to be described by Eq. (754), as several theoreticians referring to Hideki Yukawa's theory of nuclear forces claimed.

In 1937, Ettore Majorana, then still working alone at Fermi's institute in Rome, introduced in one of his rare publications, entitled '*Teoria simmetrica dell' elletrone et del protone* (The Symmetrical Theory of Electrons and Protons),' another new quantum field theory (Majorana, 1937). Like Pauli, Weisskopf, and Proca, Majorana sought to avoid a deficiency of Dirac's hole theory, namely, the asymmetry which Dirac had introduced into the treatment of positive and negative electricity (and the associated occurrence of infinite constants due to the negative-energy states, e.g., in the charge density). In order to improve upon this deficiency, Majorana proposed to use a special representation of the Dirac matrices, where all γ_k $(k = 1, 2, 3, 4)$ have the same reality as the components of the four-vector ($\mathbf{r}$, ict). In the new representation, then, the Dirac equation for the free fermion had only real coefficients; hence, all solutions could be split in a real part and an imaginary part, each of which satisfied the equation separately. Now, the real solutions ϕ thus emerging exhibited two properties: first they implied no electric charge and current, since

$$j_k(x) = \bar{\phi}(x)\gamma_k\phi(x) = 0, \quad (k = 1, 2, 3, 4), \tag{756}$$

due to the fact that $\phi^* = \phi$ and $\bar{\phi} = \phi^*\gamma_4$; and second, they satisfied the anti-communication relation

$$[\phi_\rho(x), \phi_\sigma(y)]_+ = 0. \tag{757}$$

Hence, particles associated with the real Majorana field possessed no charge and no magnetic moment; they were identical with their antiparticles, and Majorana thought that he could describe with it possibly the neutron or the neutrino. Wendell Furry argued a little later that neutrons—because of their magnetic moment—could not be Majorana particles while the neutrinos still had a chance; on the other hand, the formalism—Furry actually generalized Majorana's special representation of Dirac's matrices—would 'still show the stigmata associated with the subtractive theories of the positron: the presence of the otiose infinite terms which should be removed by subtraction, and the creation and destruction of pairs of particles' (Furry, 1938, p. 56). Consequently, the Majorana field was not actually considered very seriously in the following period for the description of elementary particles.

[1055] In a paper submitted in December 1937, Proca showed that the spin of the particles obeying Eq. (754) came out as integral multiples of $h/2\pi$ in the nonrelativistic limit (Proca, 1938).

The discovery of the medium-heavy charged particle in cosmic radiation by Seth Neddermeyer and Carl Anderson—see Section IV.3—soon stimulated the interest of theoreticians in all parts of the scientific community. First, J. Robert Oppenheimer and Robert Serber wrote a letter to *Physical Review* on 6 June 1937, in which they referred to the possible interpretation of the particle as Yukawa's heavy or U-quantum of nuclear forces, but they argued that such an interpretation led to many difficulties; hence: 'These considerations [of Yukawa] therefore cannot be regarded as the elements of a correct theory, nor serve as any argument whatever for the existence of the [experimentally observed] particle.' (Oppenheimer and Serber, 1937, p. 1113). Oppenheimer and his collaborators in California indeed refrained for years to adopt and work on meson theory. Second, Ernst Stueckelberg's letter of 6 June 1937, also sent to *Physical Review*, sounded much more positive; he called 'attention to an explanation of the nuclear forces, given as early as 1934 by Yukawa, which predicts particles of that sort,' and then added:

> Independently of Yukawa the writer arrived at the same conclusion.... We describe matter by a 16 component spinor ψ, whose first four components refer to the *electron state*, the second four functions to the *neutrino state*, the third group to the *proton state* and the last four components to the *neutron state* of matter....
>
> The known form of radiation is described by a tensor field A of four components (the vector potential) ... [which] satisfy Poisson's equation, the charge density being expressed by a suitably chosen Dirac matrix $P = e\psi^\dagger \Lambda \psi$ [with Λ a 4×4 matrix, as introduced in Stueckelberg, 1936a]. We generalize Poisson's equation, introducing the fundamental length λ in the form:
>
> $$\left(\Delta - \frac{1}{c^2}\frac{\partial^2}{\partial t^2} - \Sigma\frac{1}{\lambda^2}\right)A = -P. \qquad [(758)]$$
>
> A is now a tensor of more than four components. Σ is a matrix operating on the tensor indices analogously to the way Dirac's matrices act on spin indices of ψ. We assume for simplicity A to have five components. Furthermore let Σ be of such a form that the four first components which represent a four vector satisfy the ordinary Poisson equation ($\Sigma = 0$), while the fifth component (a scalar) satisfies Eq. [(758)] with $\Sigma = 1$. In a nonrelativistic approximation the four-vector part gives the Coulomb potential, while the scalar part [i.e., the fifth component] gives a static interaction term between the particles of the form of $f\,e^2/r\,\exp(r/\lambda)$; f is a numerical factor. A suitable choice of the generalized Dirac matrices Λ gives the electrostatic interaction between charged matter particles plus Heisenberg, Majorana, Wigner and Bartlett interactions between the heavy matter particles and the different interactions between heavy and light matter particles (β-decay, etc.) discussed by the author. (Stueckelberg, 1937b, pp. 41–42)

Thus, Stueckelberg ingeniously included the Yukawa theory of nuclear forces into a general unified theory of electromagnetism and nuclear forces, which he formally sketched here and in later, more detailed publications (Stueckelberg, 1938), without dealing with specific experimental phenomena, as his colleagues would do in the following months.

Hideki Yukawa already took the lead in 1937. He was the fastest to react to the cosmic-radiation results by noticing immediately in the preliminary analysis of the experimentalists (Anderson and Neddermeyer, 1936) the first trace of the *U*-quantum he had predicted, and he gave an account of it in a letter to *Nature* (dated 18 January 1937), and again in a later letter to *Physical Review* (dated 4 October 1937), both of which remained unpublished.[1056] In those letters, he also mentioned the programme of his forthcoming publications in the *Proceedings of the Physico-Mathematical Society of Japan*, namely, the formulation of the *scalar* and *vector* theory of *U*-quanta—or *mesotrons* or *mesons* as they would soon be named. In early fall 1937, Yukawa and Shoichi Sakata—who had come to Osaka already in 1937—developed the scalar theory.[1057] Sakata recalled later about this investigation:

> Just two [actually three] years earlier, Pauli and Weisskopf had studied a "scalar electron theory" ... Therefore this research was purely of formal interest at that time; however, we could literally take over their results in quantizing the *U*-field. Using that formalism, we performed the derivation of the nuclear forces and also calculated some processes by mesons. (Sakata, 1965)

On 10 November 1937, the journal received the completed paper 'On the Interaction of Elementary Particles. II,' marked 'read on 25 September 1937' (at a meeting of the *Physico-Mathematical Society of Japan* in Kyoto), which would appear in print before the end of the year (Yukawa and Sakata, 1937).[1058] After mentioning the pioneering paper (No. 1 of the series: Yukawa, 1935), Yukawa and Sakata drew attention to the recent confirmation of the cosmic-ray particle's discovery (Section 1), and then displayed the details of a scalar description of the *U*-field in terms of the Pauli–Weisskopf formalism (Section 2). In Section 3, they derived the interaction between the nuclear particles—proton and neutron—in terms of the creation and annihilation operators of the *U*-field quanta; in the second-order perturbation calculation, the static potential resulted,

$$V_{pn} = J(r) P_{12}^{H} \beta_1 \beta_2, \tag{759}$$

where P_{12}^{H} denoted the 'Heisenberg exchange operator,' and $J(r)$ the exchange integral (with g the coupling constant and λ the range of the nuclear forces); i.e.,

$$J(r) = g^2 \exp(-\lambda r)/r. \tag{759a}$$

[1056] See above in Section IV.3 and, especially, Rechenberg and Brown, 1990, and Brown and Rechenberg, 1996, Section 6.5.

[1057] Shoichi Sakata, who was born on 18 January 1911, near Hiroshima, studied physics from 1929 to 1933 at Tokyo (with Y. Nishina) and later at the Kyoto Imperial University, attending Yukawa's courses and taking his degree under him. After a year at RIKEN, he joined Yukawa in Osaka and became his principal collaborator in developing the meson theory of nuclear forces; in 1939, he moved (with Yukawa) to Kyoto Imperial University first as instructor; in 1942, he was appointed professor of theoretical physics at Nagoya University, where he remained until his death on 16 October 1970.

[1058] For details of the contents of this paper, see Brown and Rechenberg, 1991b, and Brown and Rechenberg, 1996, Section 7.3.

In particular, they noticed:

> In nonrelativistic approximation, [the Dirac matrices] β_1 and β_2 reduce to 1, so that Eq. [(759)] becomes the same with the result in I except the sign. In order to obtain a result exactly the same as I, we have to change the sign of $\overline{H}_U$ [the Hamiltonian of the U-field], which will obviously lead to serious difficulties of negative energy for the U-field. Whether or not this defect can be removed by introducing non-scalar field will be discussed in III. (Yukawa and Sakata, 1937, pp. 1088–1089)

Even nuclear forces of the Majorana type could be obtained in the scalar scheme from a second-order perturbation calculation by introducing the spin of nuclear particles. However, the ordinary, nonexchange forces between particles (Wigner forces), as well as the forces between particles would appear only in the fourth or higher order, yielding forces of shorter range and smaller magnitude (by a factor of 10) than those computed between the like particles in contradiction to experiment.[1059] In order to account for the last point, Yukawa and Sakata concluded that one would perhaps 'have to introduce neutral heavy [U-] quanta' (Yukawa and Sakata, *loc. cit.*, p. 1090).

The situation for the forces between the nuclear constituents improved considerably when Yukawa and his collaborators submitted the continuation of their paper in part III: The manuscript was marked 'read 25 September 1937 and 22 January 1938,' and the paper appeared in a spring issue of the journal, coauthored by Yukawa, Sakata, and Mitsuo Taketani (1938). Sakata later recalled about its origin as follows:

> Yukawa tried to generalize Maxwell's equations on the electromagnetic field, while I examined the wave equations proposed by Dirac [for particles having arbitrary spins] a year earlier (Dirac, 1936). Taketani joined our group about this time and we included him in our studies. We three developed a theory, today called "vector meson theory," and showed that it agreed with the experimental results. (Sakata, 1965)

Indeed, Yukawa—according to the date of a manuscript in the *Yukawa Hall Archive*—as early as 6 January 1937, considered generalizing Maxwell's equations explored by Proca (1936). He worked on this topic for the entire year and enlisted the association of collaborators (since the first manuscript of paper III, dated November 1937, already included their names as coauthors). In the introduction of the published paper, Yukawa, Sakata, and Taketani referred to papers I and II as having introduced 'a new field of force'—I, using a complex vector field and II, a scalar vector field to describe the U-particles—but stressed that 'neither of them was ample enough for the derivation of complete expressions for the interaction of the heavy [nuclear] particles and their anomalous magnetic moments' (Yukawa, Sakata, and Taketani, 1938, p. 319). The formulation presented in III 'can be considered as a generalization of Maxwell's equations for the electromagnetic field

[1059] The charge independence of nuclear forces will be discussed below.

... described by two four-vectors and two six-vectors, which are complex conjugate to each other respectively,' the three authors continued and noted that their 'system of equations in spinor form reduces to a special case of Dirac's wave equations (1936) for the particle with spin larger than 1/2' and also 'was equivalent to a method of linearization of wave equations for the electron, which had been developed by Proca (1936) as an extension of the scalar theory of Pauli and Weisskopf' (Yukawa, Sakata, and Taketani, *loc. cit.*). The latter reference might well have been particularly stimulated by a letter from Bristol, in which Herbert Fröhlich and Walter Heitler thanked Yukawa (on 5 March 1938) for having especially received a copy of the (unpublished) manuscript of Yukawa, Sakata, and Taketani (i.e., the letter of 4 October 1937, to *Physical Review*) and further commented:

> We quite believe that your theory is correct in principle. We ourselves have considered a great deal about the heavy electron and have formulated a theory of its interaction with the nucleus (together with Kemmer). From the discussion of the *spin dependence* of the proton-neutron force we have arrived at the conviction that the field [of the heavy electron] must be a *vector field*, as you have assumed in your Japanese note. (Fröhlich and Heitler to Yukawa, 5 March 1938, quoted in Brown and Rechenberg, 1996, p. 148)

The three-man Japanese paper thus introduced two vector fields **F** and **G**—analogous to electric and magnetic vectors in electrodynamics—to describe the free U-particle by the equation

$$(\Box - \lambda^2)\begin{pmatrix} \mathbf{F} \\ \mathbf{G} \end{pmatrix} = 0, \tag{760}$$

with $\lambda = m_U c/\hbar$ (m_U being the mass of the U-quantum). The usual linear equations of the Maxwell type would then be obtained by introducing another four-vector of fields $U_0, \mathbf{U}$, similar to the electromagnetic four-potential (§2). In Section 3, Yukawa, Sakata, and Taketani carried out the canonical quantization procedure for the U-field in the vacuum with this U-field, and in Section 4, they considered the interaction between the U-field and the electromagnetic field; in Section 5, they obtained an expression for the anomalous magnetic moments for the neutron and proton owing to the action of the heavy quantum:[1060]

$$\frac{g^2}{\hbar c}\frac{e\hbar}{2m_U c} = \frac{g^2}{\hbar c}\frac{M}{m_U}\mu_n, \tag{761}$$

where μ_n denoted the nuclear magneton and M denoted the mass of the nuclear particle (neutron or proton). Section 6 dealt with the interaction, in general, between the U-field and the heavy nuclear particles, and Section 7 with the forces

[1060] The rough estimate followed from the consideration: 'The fraction of time during which the neutron is splitting up into a proton and a heavy quantum with negative charge virtually, is roughly given by $g^2/\hbar c$.' (Yukawa, Sakata, and Taketani, 1938, p. 329) between protons and neutrons. Yukawa *et al.* obtained, in particular, for the U-field at the point $\mathbf{r}_1$ due to the presence of a heavy particle (spin vector $\boldsymbol{\sigma}^{(2)}$) at the point $\mathbf{r}_2$, the result

$$U(\mathbf{r}_1) = -g_2 \operatorname{curl}\{\boldsymbol{\sigma}^{(2)} Q_2^* \exp(-\lambda r/r)\}, \tag{762a}$$

$$\tilde{U}^+(\mathbf{r}_1) = \frac{g_1}{4\pi\lambda c} \operatorname{grad} \frac{\exp(-\lambda r)}{r} Q_2^*, \tag{762b}$$

with $r = |\mathbf{r}_1 - \mathbf{r}_2|$ and Q_2^* the operator that changes a proton into a neutron ($\tilde{U}^+$ denotes the complex conjugate of the canonical momentum operator associated with the U-field). With $U(\mathbf{r}_1)$ and $U(\mathbf{r}_2)$, the interaction Hamiltonians for two heavy nuclear particles followed, essentially in agreement with the result found earlier by Nicholas Kemmer (1938a, see below). Yukawa *et al.* commented upon the so-derived 'combination of exchange forces of Majorana and Heisenberg types between neutron and proton' by pointing out the specific peculiarity 'that the force thus obtained is not strictly central, so that we can separate S-state, P-state, etc., only in the first approximation' (Yukawa, Sakata, and Taketani, *loc., cit.*, pp. 335–336).

At one point, they could not improve on the previous scalar theory, namely, in the description of forces between like nuclear particles; hence, they emphatically called for 'the introduction of neutral heavy quanta in order to reproduce the approximate equality of the like and unlike-particle forces,' and concluded: 'It is not difficult to consider the field accompanied by the neutral heavy quanta and described by the linear equations similar to those considered above,' and announced a detailed discussion of this topic in the next paper (Yukawa, Sakata, and Taketani, *loc. cit.*, pp. 336–337). On the other hand, they still pursued in the present paper III the calculation of the estimates for the lifetime of U-quanta, obtaining about 5×10^{-7} sec. with a mass $m_U = 100 m_e$ (§8).[1061]

At several places in their paper, Yukawa, Sakata, and Taketani cited recent notes of some colleagues in Great Britain, notably, the German immigrants Herbert Fröhlich, Walter Heitler, and Nicholas Kemmer and the Indian research scholar Homi Jehangir Bhabha. Kemmer had been Pauli's assistant in summer 1936 (succeeding Viktor Weisskopf) and worked on problems of electrodynamics and nuclear forces (e.g., Kemmer, 1935; 1937a, b).[1062] In a paper on the 'Field Theory of Nuclear Interaction,' submitted to *Physical Review* in July 1937, he

[1061] For that purpose, Yukawa *et al.* assumed that the U-quanta coupled to the light nuclear particles (e^+, e^-, neutrinos) in a manner similar to the heavy ones, but for smaller coupling constants.

[1062] Nikolaus (or Nicholas, as he called himself later) Kemmer was born on 7 December 1911, in St. Petersburg, the son of an engineer of German descent. Since 1916, he grew up in London, came to Germany in 1921 and studied in Göttingen and Zurich, obtaining his doctorate with Gregor Wentzel in 1935. In October 1936, he went with a Beit Scientific Research Fellowship to Imperial College, London, and took up British citizenship in 1942. During World War II, he worked in Cambridge on problems of nuclear fission, and then on the Montreal reactor team of John Cockcroft. He returned as lecturer to Cambridge in 1946 and succeeded Max Born in 1953 to the Tait Professorship of Natural Philosophy in Edinburgh. He died on 21 October 1998, in Edinburgh. (See Brown and Rechenberg, 1999) discussed the description of equal nuclear forces between the like and unlike constituents of nuclei, as required by the results of American experiments (which we shall discuss below); he still made use of the then-standard Fermi-field theory and concluded in particular:

> The equality of forces ... is exactly accounted for by introducing interaction terms involving electron pairs or neutrino pairs. The interaction may be stated very simply with the aid of an isotopic spin variable for light as well as for heavy particles. The ratio of force constants obtainable for the theory of mass defects may be accounted for in detail by a suitable choice of the light particle field. However, it is difficult to explain any law involving more than one potential function $J(r)$. (Kemmer, 1937c, p. 906)

This charge-independent formulation of nuclear forces would play a decisive role also in the later work on the Yukawa theory.

After the existence of a 'heavy electron' was confirmed in the second half of the year 1937, Kemmer found it 'certainly suggestive that a Yukawa particle with a mass of the observed order of magnitude ($100m_{\mathrm{el}}$) does indeed give nuclear forces of the correct range'—and thus wrote in a letter to *Nature*, entitled 'Nature of the Nuclear Field' and dated 8 December 1937. Kemmer proposed there a new theoretical scheme to describe the Yukawa particle (assuming erroneously that Yukawa had used a scalar wave equation in 1935), which would be able to explain the observed ^1S and ^3S states of the deuteron. 'It has been found that a more satisfactory theory can be obtained if one admits a *vector* wave function for the new particle, such as was used by Proca [1936] in a different connexion,' he argued and continued:

> Proca's equations can be quantized on lines analogous to the Pauli-Weisskopf method [1934] for the scalar wave equation, and the resulting neutron-proton potential can easily be determined. Using the most general combination of possible interactions of the Yukawa and Proca type, the potential is found to be
>
> $$V(r) = \{A + B(\sigma_N \sigma_P) + Ck^{-2}(\sigma_N \operatorname{grad})(\sigma_P \operatorname{grad})\} \cdot \frac{\exp(-kr)}{r}, \qquad [(763)]$$
>
> where k is $2\pi c/h$ times the rest mass of the particle, σ_N and σ_P the spin operators of neutron and proton respectively and r the distance between these particles. (Kemmer, 1938a, p. 117)

The independent constants A, B, and C could be fitted to the experimental data, obtaining the values $C = 0, A : B = 3 : 5$ (see Heisenberg, 1937, p. 749). The potential should further include the 'isotopic spin' factor $\tau_N \tau_P$, 'which can be accounted for the field theory along the same line as in the case of the Fermi field (Kemmer, 1937c), that is, by assuming that the new particle also has a charged and an uncharged state,' Kemmer wrote and concluded: 'In any case, it is possible to give a field theory in which the magnitude and range of the nuclear forces, as well as their dependence on spin and charge, are all accounted equally well.' (Kemmer, 1938a, p. 117).

Just below Kemmer's letter—in the same issue (of 15 January 1938) of *Nature*—another letter, bearing the date 13 December 1937, and entitled 'Nuclear Forces, Heavy Electrons and β-decay,' made its appearance (Bhabha, 1938a). It began with the words:

> We have generalized a theory [put forward by Yukawa (1935) showing that nuclear forces can be explained by assuming the existence of new particles of mass about two hundred times that of an electron. Our theory is relativistically invariant, and in its present form gives results which we believe are of actual significance for cosmic ray and nuclear phenomena. (Bhabha, *loc. cit.*, p. 117)

Bhabha, who had coauthored with Heitler the paper on cascade showers (1937), then became occupied with nuclear forces in his further study of cascades created by protons and neutrons (Bhabha, 1937).[1063] Then, Heitler drew his attention to Yukawa's theory and he began to take interest in it.[1064] The vector-field theory which he developed to describe the *U*-particles essentially coincided with the schemes given independently by Nicholas Kemmer and Yukawa *et al.* In his letter to *Nature*, however, Bhabha emphasized several consequences in particular from the theory of phenomena observable in cosmic radiation, especially:

> A positive *U*-particle at rest may disintegrate spontaneously into a positive electron and a neutrino. This disintegration being spontaneous, the *U*-particle may be described as a "clock," and hence it follows merely from considerations of relativity that the time of disintegration is larger when the particle is in motion. (Bhabha, 1938a, p. 118)

By this theoretical conclusion, he said, the fact emphasized at that time especially by Patrick Blackett could be easily explained: Below 2×10^8 eV, most cosmic-ray particles are electrons, and above this energy, lie the heavy electrons (i.e., relatively long-lived *U*-particles).

On 13 January 1938, Wolfgang Pauli reported to Victor Weisskopf in America: 'Last week Bhabha and Kemmer were here, and we had (with the important participation of Wentzel) a kind of theoretical conference on cosmic rays,' and added: 'These two gentlemen and also Heitler soon intend to flood *Nature* and the *Pro-*

[1063] Homi Jehangir Bhabha was born on 30 October 1909, in Bombay, India, a nephew of the founder of the wealthy Tata industrial dynasty. In 1927, he went to England for higher studies; he joined the Gonville and Caius College in Cambridge, where he obtained his doctorate in 1934. He obtained various fellowships to travel widely in Europe (e.g., to Pauli in Zurich and Fermi in Rome). In 1940, he accepted a Readership at the Indian Institute of Science in Bangalore, and in 1942, he was promoted to a Professorship; at the Institute in Bangalore, he established the Cosmic Ray Research Unit. Three years later, he became Director of the newly established Tata Institute of Fundamental Research, the research establishment for future Indian scientists and mathematicians, where he had a chequered career. He was killed in an air crash on Mt. Blanc on 24 January 1966.

[1064] See Bhabha, 1938a, p. 118, footnote 5, and Bhabha, 1938b, p. 504, footnote (*).

ceedings of the Royal Society with their intellectual outputs, which deal with the so called "Yukawa theory" of nuclear forces.' (Pauli, 1985, p. 548) Walter Heitler, then in Bristol, had become interested there in the question of the anomalous magnetic moment of the proton. In September 1937, he had attended a meeting in Copenhagen, where the 'heavy electron' was discussed, and he and Herbert Fröhlich (with whom he was collaborating at that time) thought of applying Yukawa's concept to attack the problem of proton's magnetic moment. Already on 24 November 1937, they submitted a letter to *Nature*, dealing with the "Magnetic Moments of the Proton and the Neutron,' which appeared in the first issue of January 1938 of that journal (Fröhlich and Heitler, 1938). Although Fröhlich and Heitler did not cite Yukawa's theory explicitly, they made use of ideas derived from it; so they assumed a virtual emission of 'heavy electrons,' which transform a neutron into a proton and *vice versa*. Provided the (unspecified) interaction could induce a spin flip, as well as a change of charge, there would be a contribution to the magnetic moment of protons and neutrons arising from the orbital angular momentum of the heavy electron. If α denoted the fraction of time that the nuclear particle spends dissociated (i.e., the proton appears as a neutron and a virtual positive heavy electron), then the magnetic moments of proton and neutron assumed the (total) values (in units of Bohr magnetons, with M and m representing the proton—or neutron—and electron masses)

$$\mu_P = 1 - \alpha + \alpha M/m, \tag{764}$$

$$\mu_N = -\alpha + \alpha M/m. \tag{764a}$$

Fröhlich and Heitler happily concluded their note: 'Inserting the observed values $\mu_P = 2.6$, $\mu_N = -1.75$, we obtain $M/m = 22$ or $m = 80$ electron masses.'[1065]

A few weeks later, Heitler met Kemmer in London, and the Bristol team joined efforts with Kemmer to write a detailed study 'On the Nuclear Forces and the Magnetic Moments of the Neutron and the Proton,' which was received by the *Proceedings of the Royal Society of London* on 1 February 1938, and published in its issue of 4 May (Fröhlich, Heitler, and Kemmer, 1938). Kemmer, in particular, pointed out to his colleagues that their original approach violated parity, as they had worked with a scalar instead of a vector-field theory; he taught them how to use the latter systematically.[1066] A little later, on 9 February, the series of papers in the *Proceedings of the Royal Society of London* was continued by the reception of Kemmer's work on the 'Quantum Theory of Einstein-Bose Particles and Nuclear Interaction' (Kemmer, 1938b), and toward the end of February, Bhabha's

[1065] Since 1933, Otto Stern and his collaborators (see Estermann and Stern, 1933) had found the proton's magnetic moment to deviate strongly from the one predicted by assuming the Dirac equation for the proton; the same was found (indirectly) for the neutron (see Kellogg *et al.*, 1936, and Estermann *et al.*, 1937).

[1066] See Kemmer's reminiscences for his collaboration with Fröhlich and Heitler (Kemmer, 1965).

extensive work 'On the Theory of Heavy Electrons and Nuclear Forces' (Bhabha, 1938b) was received. Then, in the beginning of March, Heitler had his investigation on 'Showers Produced by Penetrating Cosmic Radiation' ready for submission (Heitler, 1938). Still, these efforts did not exhaust the immense productivity of the theoreticians in Great Britain during the first months of that year, as Kemmer published a little later another article, entitled 'The Charge Dependence of Nuclear Forces,' in the *Proceedings of the Cambridge Philosophical Society* (Kemmer, 1938c).

In their papers in the *Proceedings of the Royal Society*, Bhabha and Kemmer worked out in full the ideas indicated in their previous letters to *Nature*. Thus, Kemmer, departing from Dirac's general spinor equation (Dirac, 1936), wrote the most general interaction Lagrangians for the system of nuclear particles and *U*-vector fields and derived from them expressions for the neutron–proton exchange potentials (i.e., the static limit of the forces in question), especially

$$V^a(r) = -\frac{c\lambda}{4\pi} g_a^2 Y(r), \tag{765a}$$

$$V^b(r) = +\frac{c\lambda}{4\pi}\{g_b^2 + f_b^2[(\boldsymbol{\sigma}_{\mathrm{N}} \cdot \boldsymbol{\sigma}_{\mathrm{P}}) - (\boldsymbol{\sigma}_{\mathrm{N}} \cdot \mathrm{grad})(\boldsymbol{\sigma}_{\mathrm{P}} \cdot \mathrm{grad})]\} Y(r), \tag{765b}$$

$$\begin{aligned} V^c(r) = -\frac{c\lambda}{4\pi}\{g_c^2(\boldsymbol{\sigma}_{\mathrm{N}} \cdot \mathrm{grad})(\boldsymbol{\sigma}_{\mathrm{P}} \cdot \mathrm{grad}) + f_c^2[(\boldsymbol{\sigma}_{\mathrm{N}} \cdot \boldsymbol{\sigma}_{\mathrm{P}}) \\ - (\boldsymbol{\sigma}_{\mathrm{N}} \cdot \mathrm{grad})(\boldsymbol{\sigma}_{\mathrm{P}} \cdot \mathrm{grad})]\} Y(r), \end{aligned} \tag{765c}$$

$$V^d(r) = +\frac{c\lambda}{4\pi} g_d^2(\boldsymbol{\sigma}_{\mathrm{N}} \cdot \mathrm{grad})(\boldsymbol{\sigma}_{\mathrm{P}} \cdot \mathrm{grad}) Y(r), \tag{765d}$$

with $Y(r) = \exp(-\lambda r)/r$. He then concluded that only the case (765b) 'agrees with experience'; hence, he announced: 'The detailed discussion of this fact is the subject of the paper of Fröhlich and others' (Kemmer, 1938b, p. 147). In the three-man work, the authors (Fröhlich, Heitler, and Kemmer) indeed adopted the solution (765b) to fit the spin-triplet ground state and the spin-singlet 'virtual' scattering state of the deuteron, and obtained the result:

$$\left.\begin{aligned} {}^3S: V_{\mathrm{NP}} &= -Y(r)(g^2 + 2f^2/3), \\ {}^1S: V_{\mathrm{NP}} &= -Y(r)(2f^2 - g^2), \end{aligned}\right\} \tag{766}$$

which, with $f \approx g$, accounted 'in a reasonable way for the nuclear forces, including the right spin dependence' (Fröhlich, Heitler, and Kemmer, 1938, p. 166). They also noticed: 'In the scalar theory (Yukawa and Sakata, 1937) it turns out, for instance, that the ^3S-state is always repulsive and the ^1S-state attractive with the same absolute value, which is contrary to experiment.'[1067]

Bhabha, on the other hand, concerned himself less with the details of the nuclear potential. He began his paper in the *Proceedings of the Royal Society* with an introduction to the decay property of the 'heavy electrons,' and then developed in detail the vector-field formulation closely following Proca's theory and working out the necessary quantization procedures. After a more formal section on the forces of interaction between protons and neutrons, he finally discussed the relativistic scattering of neutrons and protons, and of U-particles by neutrons or protons, as occurred in cosmic radiation (Bhabha, 1938b). Moreover, Heitler, in his paper, attempted within the framework of the vector-field theoretical formulation 'a qualitative explanation of a number of cosmic-ray facts connected with the penetrating radiation' (Heitler, 1938, p. 529), for example, the neutron capture of a positive heavy electron, the multiple-production of heavy electrons from the collision of a heavy electron with a nucleus, or the creation of heavy electrons.

While these robust activities were going on in Great Britain on the vector-version of Yukawa's theory, the Japanese theoreticians did not remain idle. On 28 May 1938, the fourth paper of Yukawa's series was read at a meeting of the Physico-Mathematical Society of Japan, now involving—besides Yukawa, Sakata, and Taketani—Minoru Kobayashi (another Osaka student) as an author. The new investigation first displayed a theory of the neutral Yukawa particle, sometimes called the 'neutretto' (§2), then treated in greater detail the deuteron problem (§3), the β-decay theory in general (§5), and the annihilation (§6), creation (§7), and absorption of U-quanta (§8); finally (in §9), the spin and magnetic moment of the vector object were discussed (Yukawa, Sakata, Kobayashi, and Taketani, 1938). With this four-man paper 'On the Interaction of Elementary Particles. IV,' Yukawa *et al.* completed the pioneering work begun in Osaka in fall 1934, though the authors (and their students) would continue to contribute in subsequent years a series of further theoretical investigations on special problems in this fundamental field of high-energy nuclear forces. For instance, the β-decay lifetime of the Yukawa particle received great attention after 1938, when the first experimental estimates became available. In particular, Hans Euler and Werner Heisenberg (in Leipzig) developed methods to analyze the absorption data of cosmic radiation in different media—larger absorption of the intensity was obtained in air as compared to denser water of the same 'effective thickness!'—and derived a decay time of the 'hard component' particle of $\tau = 2.7 \times 10^{-6}$ s (Euler and Heisenberg, 1938, p. 42).[1068] When Heisenberg wrote about the result to Yukawa (in a letter dated 16 June 1938), the latter replied promptly on 15 July:[1069] 'It is a pity that there

[1067] Fröhlich *et al.* (1938) also emphasized that in the scalar theory the magnetic moments of proton and neutron turned out to be zero.

[1068] The authors assumed, as did most experts at the time, that the penetrating 'hard' component consisted of heavy electrons or Yukawa particles.

[1069] See the Yukawa correspondence preserved in the *Yukawa Hall Archival Library*.

was an error of a factor 2 in our calculations [i.e., Yukawa, Sakata, and Taketani, 1938, p. 339], so that the lifetime for the heavy electron with mass $m_U = 100m_e$ becomes 0.25×10^{-6} s, which makes the situation a little worse.'[1070] The discrepancy of a factor of at least 10 in the lifetime between theory and experiment remained, though Yukawa expressed—in a letter to Proca on 12 December 1938—the hope that 'the matter will be settled, when the cloud chamber photograph showing the ejection of the fast electron with the predicted energy from the end (in the gas) of the track of the mesotron will happen to be obtained,' referring here to a French observation which Proca had reported to him. Yet, neither the final analysis of this observation, nor of further ones carried out by Bruno Rossi and Franco Rasetti in the USA between 1939 and 1941 improved the theoretical situation. However, the very idea of the decaying cosmic-ray particle, as first treated in the literature by Bhabha, together with the first determination of its decay time (by Euler and Heisenberg) led immediately to an enormous progress in cosmic-ray physics, and Heisenberg—in a lecture delivered in Hamburg on 1 December 1938—stressed optimistically:

> Actually, we can then understand the entire experimental material [on the "hard" component]—for the time being, rather qualitatively. We may therefore rightly expect that we now—with the discoveries of recent years, notably that of the positron and finally also of the mesotron with its finite lifetime—have gained the key to a complete understanding of the nature of cosmic rays. (Heisenberg, 1939a, p. 42)

During the year 1939, the theoretical work on the entire field (involving nuclear forces and cosmic-ray phenomena) increased immensely (see Brown and Rechenberg, 1996, Chapter 9). The Japanese and European physicists were finally joined in their efforts by their colleagues in the United States, and a lively scientific exchange between Europe, Asia, and America, went on in that period just before the outbreak of the European (in September 1939) and World War (two years later). At that time, even more complex field theories were proposed for the 'heavy electrons,' 'mesotrons,' or 'mesons,' e.g., the mixed vector-pseudoscalar theory of Christian Møller and Léon Rosenfeld (1939, 1940) in Copenhagen. Besides quantitative discrepancies between theory and experiment—which extended beyond the lifetime problem, say, in the description of cross sections of reactions involving mesotrons—the fundamental divergence difficulties, known already from quantum electrodynamics (and the Fermi-field theory) emerged clearly from these investigations. In view of the new vector-meson theory, one is reminded of the letter which Pauli wrote to Weisskopf on 13 January 1938 (cited earlier), in which he stressed:

> Notably ... the self-energies and magnetic moments of the particles also become infinite—more strongly, by the way, than in quantum electrodynamics. (Heitler has made computations on the magnetic moment of the proton and neutron in such

[1070] For details of the story of mesotron's decay-time between 1937 and 1941, see Brown and Rechenberg, 1996, Chapter 8.

> theories, which partly rest on omissions, partly on wild cutoff manipulations.) Hence we have, of course, arrived again where we always got stuck since 1930, namely at the infinities of quantized field theories. (Pauli, 1985, p. 549)

And on 10 May 1938, he remarked to Heisenberg: 'Meanwhile I have read Yukawa's paper [III] and am satisfied with it; so far the theory does not diverge (as in the case of the magnetic moment of the neutron).' (Pauli to Heisenberg, 1985, p. 573) Similarly, Heitler, the pioneer of the vector-field theory—whom Pauli criticized here—was aware of the fact that the new scheme provided only 'a qualitative explanation of a number of cosmic ray facts connected with the penetrating radiation,' because: 'For higher energies the theory leads to serious mathematical difficulties (diverging self-energy, diverging nuclear forces of higher order, etc.' (Heitler, 1938, p. 529)

Heisenberg responded to such statements with two papers in 1938, one '*Über die in der Theorie der Elementarteilchen auftretende universelle Länge* (On the Universal Length Entering into the Theory of Elementary Particles)'—submitted in January for the issue of *Annalen der Physik* commemorating Max Planck's 80th birthday (Heisenberg, 1938a)—and the other entitled '*Über die Grenzen der Anwendbarkeit der bisherigen Quantentheorie* (On the Limitation of the Applicability of the Present Quantum Theory)'—received on 24 June by *Zeitschrift für Physik* (Heisenberg, 1938b). The main argument put forward by Heisenberg was the following: In the field theories of nuclear forces (as in quantum field theory in general), a universal length $r_0 = e^2/mc^2 = 2.81 \times 10^{-13}$ cm—the classical electron radius, which agreed closely with the Compton wavelength of the Yukawa or cosmic-ray particle—played a fundamental role. In their correspondence, Pauli criticized certain parts of the first paper as being 'sloppy,' but agreed with the improved presentation in the second paper. When in spring 1939, Heisenberg was asked to prepare a report on 'general problems, limitations of the present theory, and the concepts of elementary particle' (see Heisenberg to Pauli, 20 April 1939, in Pauli; 1985, p. 629), he wrote—after deliberating on the contents—to his critical friend:

> I have found out that a considerable part deals with questions which you know better than I do. Hence I want to ask you whether you have got the time and interest in taking over this part. (Heisenberg to Pauli, 23 April 1939, in Pauli, *loc. cit.*, p. 634)

He then sketched in his letter a programme consisting of three parts: 1. General Properties of Elementary Particles; 2. The Specific, Empirical Forms of Interactions and Their Consequences; 3. The Limitations of the Present Theory. 'You see from this programme that I would like to leave Section 1 to you, of which you understand much more, and which I just would have to copy laboriously [anyway] from you and Fierz,' he concluded (Heisenberg to Pauli, 23 April 1939, in Pauli, *loc. cit.*). Pauli agreed to the proposal and prepared his part, while Heisenberg was freed to concentrate on his specialty.

Actually, for Section 2 of his above-mentioned programme, he separated quantum field theories into two classes:

(1) The interaction term in the Hamiltonian contains, besides the wave functions involved, only a dimensionless numerical factor Z. Then this numerical factor Z must be $\ll 1$, so that in the present stage of the theory the introduction of this interaction can be connected at all with any clear physical meaning.
(2) The interaction term contains, besides the wave function, a constant of the dimension of the power of a length. In this case the interaction may be considered as a small interaction only if particles of small energy are involved.... For high-energy particles nothing can be derived at the moment from the interaction expression, because in that case the problem of interaction cannot be separated from the problem of the mass of the particles, hence the quantum-theoretical methods fail at present. (See Heisenberg, 1984a, p. 347)

Among the known quantum field theories, quantum electrodynamics had to be considered as a typical Class-1 theory (at least if not too high energy and momentum transfers were involved). On the other hand, Heisenberg emphasized:

> The most interesting example for an interaction of the second class is provided by Yukawa's theory of the mesotron. If one wants to explain the forms of interactions between the nuclear constituents, as derived from experience, with the help of Yukawa's theory, then one must introduce this field ... as a vector field (hence assume spin$-1\hbar$ mesotrons), and one has to admit in the interaction between the Yukawa field with nuclear constituents) terms containing a dimension of length (in contrast to Maxwell's theory). (Heisenberg, *loc. cit.*, p. 351)

Such Class-2 theories possessed, as Heisenberg explained in a slightly simplified vector-field theory—which Homi Bhabha had proposed in a letter to *Nature* on 17 December 1938 (Bhabha, 1939)—several peculiar properties, namely, in particular, that 'the interaction is a small perturbation only if particles of small energy are involved,' and 'in the Yukawa theory [the root of the numerical factor] l is of the order of magnitude $\frac{1}{\kappa} = \frac{\hbar}{\mu_{\text{meson}}} \sim 2 \times 10^{13}$ cm' (Heisenberg's report intended for the 1939 Solvay Conference; Heisenberg, 1984a, p. 352). Now, for the description of (static) nuclear forces the condition of small energy might be satisfied, Heisenberg wrote, but for large energies the interaction term (involving the factor l^2) would clearly dominate the Hamiltonian and then 'the usual quantum-theoretical treatment fails' (Heisenberg, *loc. cit.*, p. 353). As an immediate consequence, then followed the breakdown of any perturbation-theoretical treatment for energies determined by k_0 (i.e., the energy, up to factors h and c) $> 1/l$, and this fact would show up in cosmic rays by the occurrence of explosive multiparticle production, Heisenberg concluded.[1071] The debate on the role of a fundamental length l in

[1071] Heisenberg discussed the formation of explosive showers in the vector-meson theory, or in Bhabha's simplified model, respectively, in his paper '*Zur Theorie der explosionsartigen Schauer in der kosmischen Strahlung. II* (On the Theory of Explosion-Like Showers in Cosmic Radiation II),' which he had submitted in early May 1939 to *Zeitschrift für Physik* (Heisenberg, 1939b).

meson theory and the description of cosmic-ray showers was carried on in a quite attentive manner at the 'Symposium on Cosmic Rays' in Chicago in June 1939, in which Heisenberg participated and where he gave a talk (Heisenberg, 1939c); this debate would be resumed again and again in later years.[1072]

In Part 3 of his intended Solvay report, entitled (in English translation) 'The Limitations of the Present Theory,' Heisenberg largely followed the items discussed in his second paper of 1938. He proposed to consider the two regions for high-energy processes: first, those in which the energy and momentum changes could be considered small, or in relativistic formulation,

$$|(p_I - p_{II})^2 - (p_I^0 - p_{II}^0)^2| \ll \left(\frac{\hbar}{r_0}\right)^2; \tag{767}$$

here, the available quantum-mechanical formalism should apply. In the second region, determined by the relativistically invariant condition

$$|(p_I - p_{II})^2 - (p_I^0 - p_{II}^0)^2| \gg \left(\frac{\hbar}{r_0}\right)^2, \tag{767a}$$

the quantum-mechanical description broke down—and the theoreticians occasionally tried to work with a 'cutoff'-prescription; in this case, certain phenomena—such as explosive multiparticle production—should occur. Heisenberg warned, however, not to interpret too naively the concept of the fundamental length, as he wrote:

> According to Eq. [(767a)] one might, at first sight, guess that it makes no sense at all to talk about lengths which are small compared to r_0. Such a conclusion would certainly not be justified; because, even if in processes of the type [(767a)] totally new phenomena show up, it is always possible in principle to determine, say, wavelengths that are small compared to r_0 with diffraction phenomena, without at all involving processes of the type [(767a)]. It constitutes a different question whether one can determine the position of a particle more accurately than to the order r_0. Whether this is possible can only be decided once the new phenomena are exactly known which occur in the region given by [(767a)]. (Heisenberg, 1984a, p. 355)

Finally, Heisenberg sketched a few suggestions about the concepts that might be used for describing the new processes for the second region.

In contrast to Parts 2 and 3 of Heisenberg's intended Solvay report, which focused on the most problematic, unsolved questions of elementary particle theory at the end of the 1930s, Pauli—in Part 1, dealing essentially with the properties of free particles—presented some firm results obtained essentially during the previous

[1072] For a historical account of explosive showers in cosmic rays, see Cassidy, 1981, and Brown and Rechenberg, 1991a.

two years by himself and his main collaborator and assistant Markus Fierz, on the one hand, and Frederick Joseph Belinfante of Leyden, on the other.[1073] Already when formulating previously the 'anti-Dirac' theory, Pauli had found—contrary to expectation—that the scalar fields could not be consistently quantized according to the anti-commutation rules (Pauli and Weisskopf, 1934, see above). After his assistant Kemmer left in 1936 for England, Pauli gained the collaboration as assistant for several years (until 1940, when he went to Princeton for the duration of World War II) of Markus Fierz, a very able and devoted helper in analyzing and investigating systematically the available quantum field theories. Fierz, who had worked for his doctorate under Gregor Wentzel, which he obtained in early 1936 with a thesis on the β-decay of artificially produced proton–neutron transitions, began his work with Pauli by studying all possible invariant forms of the matrix elements in β-decay according to Fermi's theory (without derivatives of the Konopinski–Uhlenbeck type) and their consequences (Fierz, 1937). He then assisted Pauli in work on the infrared divergence, which the latter presented in October 1937 at the Galvani Bicentennial Celebration in Bologna (Pauli and Fierz, 1938). At the Delémont meeting of the Swiss Physical Society in spring 1938, Fierz spoke on some of his results ['*Über die relativistische Theorie für Teilchen mit ganzzahligem Spin sowie deren Quantisierung* (On the Relativistic Theory for Particles with Integral Spin and Its Quantization),' Fierz, 1938], and in September of that year, he submitted his *Habilitation* thesis to *ETH*, entitled '*Über die relativistische Theorie kräftefreier Teilchen mit beliebigem Spin* (On the Relativistic Theory of Free Particles with Arbitrary Spin),' which was published in the following January issue of *Helvetica Physica Acta* (Fierz, 1939). Then, at the Brugg meeting of the Swiss Physical Society in May 1939, Pauli and Fierz presented a short report ['*Über relativistische Feldgleichungen von Teilchen mit beliebigen Spin in elektromagnetischen Felde* (On the Relativistic Field Equations of Particles of Arbitrary Spin in an Electromagnetic Field),' Pauli and Fierz, 1939], and they communicated a more detailed paper with essentially the same title to the *Proceedings of the Royal Society* (Fierz and Pauli, 1939). We shall now turn to the contents of these investigations dealing with the spin and statistics connections of relativistic quantum fields.

Markus Fierz began in spring 1938 by investigating the case of a relativistic tensor field of degree f, satisfying conditions like a continuity equation and a second-order wave equation. When applying the rules of quantum dynamics, Fierz could associate with the fields particles of integral spin $f(h/2\pi)$ and mass; he further established relativistically invariant commutation relations by generalizing those of spin-zero particles (in the Pauli–Weisskopf scheme) and showed that the particles obeyed Bose statics (Fierz, 1938). In his *Habilitation* thesis, Fierz then treated again, in the interaction-free case, also the generalized spinor fields of

[1073] Actually, Pauli split his report into two chapters, of which the first was devoted to general considerations and the second to the discussion of special cases (see the programme reproduced in Pauli, 1985, p. 664).

Dirac (1936) associated with particles of half-integral spin; by using the spinor calculus of the Leipzig mathematician Bartel Van der Waerden (1932), he arrived at the results:

> Particles with integral spin must always satisfy Bose statistics and particles with half-integral spin Fermi statistics. Force-free wave fields having spin ≤ 1 are already distinguished by the singular fact that their charge density and energy density are uniquely defined, gauge-invariant quantities; for higher spins only the total charge and total energy satisfy these requirements. (Fierz, 1939, p. 3)

In a letter to Paul Dirac, dated 11 November 1938, Pauli informed him that 'Fierz has a long paper in press, where he can show that no difficulties arise by the quantization of these equations, so long as no interaction between the particles (or with other particles' electromagnetic field) is taken into account,' and continued: 'Recently, however, we investigated more clearly the question of this interaction and came to quite different results.' (Pauli, 1985, p. 607). He then mentioned three difficulties that might occur in the case of higher spins: (i) Dirac's substitution $p_\mu - (e/c)A_\mu$ for p_μ [i.e., the four-momentum of the free particle p_μ, and the same momentum in the presence of an electromagnetic field having the four-potential A_μ] did not apply for particles with spin > 1; (ii) the equations for higher-spin particles would at least describe two types of particles which could make transitions into each other; (iii) for higher spins, at least one type of particles assumed negative energies, hence '*no elementary particle* (at least with non-vanishing rest mass) *with a spin greater than 1 can exist*,' Pauli concluded (Pauli, *loc. cit.*).

While Pauli and Fierz tried to cope with the extra, negative-energy particles—they especially formulated conditions to suppress these objects (Pauli and Fierz, 1939; Fierz and Pauli, 1939)—Belinfante, a student of Hendrik Kramers's, entered into the fray by applying certain new mathematical methods and physical concepts in the theory of elementary particles; in particular, he introduced—instead of the well-known tensor and spin calculus—a different scheme of mathematical quantities, which he called 'undors' and which could describe both integral-spin and half-integral spin fields (Belinfante, 1939a). The undors were related to Dirac spinors—denoting essentially the outer products of the latter—and they (the Dirac spinors) just became 'undors of the first rank.' Belinfante then wrote the different existing relativistic wave equations, such as that for vector mesons, in terms of his undor-formalism, and he noticed in particular that, e.g., the second-rank undor might be decomposed into a scalar and a pseudoscalar, a vector and an axial-vector, and a symmetrical tensor, thus unifying the possible descriptions of wave fields with spin up to $2(h/2\pi)$ (Belinfante, 1939b, c). This formalism *per se* added little to the already known results of the different theories, which had been proposed at the time to describe Yukawa's U-quanta, but it supported the consideration of the new physical concept of charge conjugation in particle physics. Thus, in particular, Belinfante wrote:

> To one description of the Dirac particles, mesons, neutrettos [i.e., neutral mesons as demanded especially by Yukawa and Kemmer in 1938] and the electromagnetic field by undor wave functions[, etc.,] there is an equivalent *charge-conjugated description*.... [which] suggests a kind of symmetry between the two ways of describing physical situations. By way of hypothesis one might assume that such a symmetry is a *fundamental property of nature*. We shall call this property charge conjugation. (Belinfante, 1939b, pp. 881–882)

Belinfante then made use of the new symmetry concept to determine physically meaningful quantities in particle physics and stated:

> We shall show here that the postulate of charge invariance implies directly that photons and neutrettos *must* be neutral, that Dirac electrons *must* obey Fermi statistics and that mesons *must* obey Einstein-Bose statistics. The interesting fact is that this statistical behaviour of particles and quanta follows much more directly from the postulate of charge invariance than from postulates concerning the positive character of the total energy of free particles or quanta. (Belinfante, *loc. cit.*, p. 882)

In the following investigation 'On the Statistical Behaviour of Known and Unknown Elementary Particles' (submitted in December 1939 and published in the March 1940 issue of *Physica*, the article having been written in English, with an abstract in German), Pauli and Belinfante joined forces. They first stated the three postulates which determined the statistics in the relativistic theories of (free) elementary particles, namely:

> (I) The energy is always positive,
> (II) Observables at different space-time points commute for space-like distances,
> (III) There exist two quivalent descriptions of nature, in which the elementary charges have opposite sign, and in which corresponding field quantities transform in the same way under Lorentz transformations. (Pauli and Belinfante, 1940, p. 177)

Pauli and Belinfante then demonstrated that, in the general case of undors having the same rank, postulate (III)—involving Belinfante's charge symmetry—would not suffice to determine the statistics of the associated particles; however, the postulates (I) or (II), respectively, would always do. On the other hand, in the hitherto considered cases of spin-0, spinor (i.e., spin-$\frac{1}{2}$), and vector fields, the postulate (III) indeed fixed the statistical behaviour, as Belinfante had previously claimed (Pauli and Belinfante, 1940).

Since the planned Solvay Conference of October 1939, for which Pauli (and Heisenberg) had written reports, was cancelled because of the outbreak of the European War in September, Pauli published the results of his contribution in the following years. In the first paper, entitled 'The Connection between Spin and Statistics' and submitted from Princeton (where Pauli had moved in spring 1940) to *Physical Review* in August 1940, he summarized the conclusions derived from the collaboration with Markus Fierz and Frederick Belinfante (Pauli, 1940). Especially from the postulates (I) and (II) he obtained the two results:

> *For integral spin the quantization according to the exclusion principle is not possible....* On the other hand, it is formally possible to quantize the theory for half-integral spins according to Einstein-Bose statistics, *but ... the energy of the system would not be positive*. (Pauli, *loc. cit.*, p. 722)

Pauli published an even more extensive report in the July 1941 issue of *Reviews of Modern Physics* (Pauli, 1941). In Part II of this comprehensive paper (called 'an improved form of an article written for the Solvay Congress, 1939, which has not been published in view of the unfavorable times,' Pauli, *loc. cit.*, p. 203, footnote), Pauli also discussed the interaction of spin-0, spin-$\frac{1}{2}$, and spin-1-particles with an external electric field. Since his work with Fierz on this problem in 1939, several theoreticians in Europe and the United States had become interested in the electromagnetic properties of particles described by different relativistic wave equations, such as the cross sections of some electromagnetic processes involving charged particles of various spins.[1074] In his 1941 paper, Pauli thus summarized the status (achieved before the European War turned into World War II) of that aspect of elementary particle theory, which referred mainly to the consistent description of the properties of free elementary particles and their interaction with the external electromagnetic fields.

As mentioned earlier, toward the end of the 1930s, Paul Dirac had attempted to formulate a new classical basis for a more consistent, i.e., less divergent description of the electron and its behaviour (Dirac, 1938b). Three years later, he addressed the problem of the divergences in the existing quantum field theories from a new, quite different, point of view in his Bakerian lecture delivered on 19 June 1941. Evidently, he said, the modern developments of atomic theory had led so far to 'a satisfactory nonrelativistic quantum mechanics;' hence, it seemed to him obvious to associate the divergence problem in relativistic theory *not with an inappropriate mathematical description* of the physical facts, as was usually done, but he rather suggested:

> In extending the theory to make it relativistic, the developments needed in the mathematical scheme are easily worked out, but the difficulties arise in the interpretation. If one keeps to the same basis of interpretation as in the nonrelativistic theory, one finds that particles have states of negative kinetic energy as well as their usual states of positive energy, and, further, for particles whose spin is an integral number of quanta, there is the added difficulty that states of negative energy can occur with a negative probability. (Dirac, 1942, p. 1)

Evidently, Dirac here repeated some of the same difficulties, which he had criticized since 1926 against the Klein–Gordon equation, and which had guided his

[1074] For a historical account, see Brown and Rechenberg, 1996, Section 10.6.

path to the relativistic spinor equation for the electron. While most problems of the scalar equation had been formally overcome by reinterpreting the density with the indefinite sign as a charge density (which could be associated with particles of opposite sign), in Dirac's electron theory, also the problem of negative energy states showed up: They had to be suppressed by invoking the Dirac sea and the hole theory. However, this theory had not succeeded in solving the divergence problems of *QED* completely, in spite of achieving some moderate success in occasionally reducing the degree of divergence. Now, in 1941, Dirac proposed to forget about these previous limited successes entirely as being unsatisfactory and 'extremely complicated,' and rather suggested the following interpretation of the electron and photon situations, respectively:

> The simple accurate calculations that one *can* make [in the case of the electron theory] apply to a world which is almost saturated with positrons, and it appears to be a better method of interpretation to make the general assumption that transition probabilities obtained for this hypothetical world are the same as in the actual world.
>
> With photons one can get over the negative-energy difficulty by considering the states of positive and negative energy to be associated with the emission and absorption of a photon respectively, instead of, as previously, with the existence of a photon. The simplest way of developing the theory would make it apply to a hypothetical world in which the initial probability of certain states is negative, but transition probabilities calculated for this hypothetical world are found to be always positive, and it is quite reasonable to assume that these transition probabilities are the same as those in the actual world. (Dirac, *loc. cit.*)

For demonstrating how his new interpretation worked, Dirac investigated the situation in quantum electrodynamics involving n photons. If one tried to solve the wave equations involving them, then—in general—divergent integrals over frequencies ν of the form

$$\int_0^{\infty} f(\nu)\,d\nu, \quad \text{with } f(\nu) \sim \nu^n \text{ for large } \nu, \tag{768}$$

arose. Dirac now emphasized that one could 'build up a form of quantum electrodynamics symmetrical between positive and negative energy photon states,' which implied similar equations as the old one but led to integrals of the type

$$\int_{-\infty}^{+\infty} f(\nu)\,d\nu, \tag{768$'$}$$

instead of Eq. (768); hence, 'the divergencies with odd n values all cancel out' (Dirac, *loc. cit.*, p. 13).[1075] Thus, a new form of *QED* emerged, in which the

[1075] Those with even n-values might be avoided by a suitable limiting process in the classical theory (Dirac, 1938b; 1939a).

quantum-theoretical operators corresponding to real dynamical variables in the classical theory were no longer self-adjoint. Instead of being bothered by such hitherto unusual perspectives of his ingenious 'cutting the Gordian knot' of quantum field-theoretical divergences, Dirac rather examined the consequences from 'the new hypothetical world,' which he assumed to yield the same probability coefficients as the real world, finding indeed: 'When applied to elementary examples, it gives the same results as Heisenberg and Pauli's quantum electrodynamics with neglect of the divergent integrals.' (Dirac, *loc. cit.*, p. 17)

The 'new method of field quantization' would attract, as soon as it appeared in spring 1942, especially the attention of Wolfgang Pauli in America. In a report for *Reviews of Modern Physics*, which Pauli called a kind of continuation of his earlier one on the spin-statistics connection in quantum field theory (Pauli, 1941), he pointed out that Dirac 'uses an indefinite metric in the space of quantum states' (Pauli, 1943, p. 175). Although he considered Dirac's procedure as by no means supplying 'a consistent and complete system of relativistic quantum field theory—it even led to obviously wrong conclusions'—he confessed in a letter to Homi Bhabha in India: 'Nevertheless it seems to be very interesting.' (Pauli to Bhabha, 16 March 1943, in Pauli, 1993, p. 179)[1076]

(d) Nuclear Forces and Reactions: Transmutation, Fusion, and Fission of Nuclei (1934–1942)

The discovery of artificial radioactivity by Irène Curie and Frédéric Joliot (when bombarding boron nuclei with α-particles in early 1934)—see Section IV.3—stimulated Enrico Fermi to use neutrons in order to produce similar effects; he thought that even the available weak neutron sources should be effective because the neutral particles are not repelled by the positively charged nuclei. The experiments began in March 1934 and were undertaken by Franco Rasetti, the expert on neutrons in Fermi's institute, but at first they did not yield results. As Emilio Segrè recalled:

> Rasetti then left for vacations in Morocco and Fermi continued the experiments. He had the idea, essential for the success, of replacing polonium-plus-beryllium source with a much stronger radon-plus-beryllium source. Radon could be employed because beta and gamma radiation would not interfere with the observation of a delayed effect. Professor G. C. Trabacchi had a radon plant and gave the material to Fermi.... Radon-plus-beryllium sources were prepared by filling a small glass bulb with beryllium powder, evacuating the air, and replacing the air with radon. The sources decayed with the half-life of radon, 3.82 days. When Fermi had his stronger neutron source, he systematically bombarded the elements in order of increasing

[1076] Dirac's method of the indefinite metric would be widely used in the following decades. In 1972, Heisenberg would write a review article on 'Indefinite Metric in State-Space' for the *Dirac Festschrift* (Heisenberg, 1972).

> atomic number, starting with hydrogen and following with lithium, beryllium, boron, carbon, nitrogen and oxygen, all with negative results. Finally, he was successful in obtaining a few counts on his Geiger-Müller counter when he tried fluorine. (Segrè, 1970, p. 73)

In spite of the still comparatively weak neutron source and the primitive counter available, Fermi submitted a short letter already on 25 March 1934, to *Ricerca Scientifica*, announcing a positive result (Fermi, 1934c), the first of a series on the topic in this journal. Soon afterward, on 10 April, he also sent a letter to *Nature* which appeared under the title 'Radioactivity Induced by Neutron Bombardment' in the issue of 19 May (Fermi, 1934d).

In order to push further the investigations in this new field of research, Fermi asked Edoardo Amaldi and Emilio Segrè to assist him and ordered Rasetti to come back from Morocco: In addition, they obtained the assistance of Oscar d'Agostino, a chemist from Trabacchi's laboratory, who had acquired knowledge of radioactivity during his stay in Paris with Marie Curie. This team from Rome communicated almost weekly letters to *Ricerca Scientifica* and sent preprints of these letters to colleagues abroad. Thus, Ernest Rutherford acknowledged the receipt of one of these preprints on 23 April, and congratulated Fermi on 'your successful escape from the sphere of theoretical physics' (quoted in Segrè, 1970, p. 75). Soon, Fermi submitted another letter to *Nature*, in which he reported further results, especially:

> As a matter of fact, it has been shown that a large number of elements (47 out of 68 examined until now) of any atomic number could be activated, using neutron sources consisting of a small glass tube filled with beryllium powder and radon up to 800 millicuries. This source gives an yield of about one million neutrons per second. (Fermi, 1934e, p. 898)

After explaining the methods of detecting the induced activity, he continued:

> It seemed worth while to direct particular attention to the heavy radioactive elements thorium and uranium, as the general instability of nuclei in this range of atomic weight might give rise to successive transformations. For this reason an investigation of these elements was undertaken by the writer in collaboration with F. Rasetti and O. d'Agostino.
>
> Experiments showed that both elements, previously freed of ordinary impurities, can be strongly activated by neutron bombardment.... A rough survey of thorium activity showed in this element at least [the occurrence of] two periods [of decay].
>
> Better investigated is the case of uranium; the existence of periods of about 10 sec, 40 sec, 13 min, plus at least two more periods from 40 minutes to one day is well established. (Fermi, *loc. cit.*, p. 899)

Though 'the large uncertainty in the decay curves due to the statistical fluctuations makes it very difficult to establish whether these periods represent successive or alternative processes of disintegration,' Fermi concluded from the existence of a

'13 minute-product from most of the heaviest elements' that it 'suggests the possibility that the atomic number of the element might be greater than 92' (Fermi, *loc. cit.*). In fact, he claimed that chemical analysis would support the hypothesis that it might be the new element No. 93, homologous to manganese and rhenium. Hence, the title of Fermi's note (1934e) in *Nature* suggestively announced the 'Possible Production of Elements of Atomic Number Higher Than 92,' and the same conclusion from the result of the uranium bombardment was reported in later publications (e.g., the paper of Fermi, Amaldi, d'Agostino *et al.*, which appeared in the *Proceedings of the Royal Society*, 1934).

The discoveries of Fermi's team in Rome aroused much interest in the scientific community, as can be discerned from the large number of letters received especially by *Nature* on the subject in summer 1934. Thus, the transmutation of light to medium elements was confirmed in the Cavendish Laboratory (Bjerge and Weststedt, 1934a, b), and the first theoretical explanations of the results on the basis of a nuclear model (consisting mainly of α-particles and some extra neutrons and deuterons) were offered (Newman and Walke, 1934a, b; Guében, 1934). On the other hand, in the 15 September issue of the well-reputed German journal *Angewandte Chemie*, Ida Noddack, co-discoverer of the element rhenium, analyzed—especially from the chemical point of view—Fermi's claim to have created the transuranic element No. 93 (Noddack, 1934), and she concluded that Fermi's method of proof was not '*stichhaltig* (conclusive),' since:

> The fact that Fermi not only compares the known immediate neighbour of uranium—protactinium—with his newly created β-radioactive substance, but includes several elements down to lead, shows that he considers the possibility of a sequence of decay processes (with the emission of electrons, protons and helium nuclei), which finally led to the formation of the radioactive elements with half-life of 13 minutes. If he proceeds in this way, one cannot understand why he stops with lead, since the old view that the uninterrupted sequence of radioactive elements stops with lead or better with thallium (No. 81) has been rejected by the above-mentioned experiments of Curie and Joliot. Fermi should have compared his new radio[active] element with *all* know elements. (Noddack, *loc. cit.*, p. 654)

After this general criticism, the chemist Ida Noddack specifically attacked the method of chemical analysis pursued in Fermi's laboratory—using nitric acid and the precipitation with manganese dioxide—because it might absorb some of the substances produced in the reaction; hence, she again concluded: 'The proof that the new radioelement has the atomic number 93 is not completed at all.' (Noddack, *loc. cit.*) Instead of Fermi's proposal, she now suggested a different conclusion:

> One may just as well assume that in this nuclear smashing by neutrons very different "nuclear reactions" occur than have hitherto been observed when protons and α-particles hit atomic nuclei. In the latter mentioned irradiations only those nuclear transformations are observed that imply the emission of electrons, protons and helium nuclei, hence for heavy elements the mass of the irradiated atomic nuclei

> should also change only little, since neighbouring elements would result. It might be conceivable [still] that in the bombardment of heavy nuclei with neutrons these nuclei decay in several larger fractions which, though being isotopes of known elements, are not neighbours of the irradiated elements. (Noddack, *loc. cit.*)

After raising more objections, Ida Noddack pleaded for further investigations before one could consider element No. 93 as having really been found. However, her arguments and warning did not attract much attention at that time; only Otto Hahn and Lise Meitner, who carried out their own experiments on the neutron bombardment of uranium at the *Kaiser Wilhelm-Institut für Chemie* in Berlin, objected (to Noddack's conclusions) in a letter to *Naturwissenschaften* (dated 22 December 1934, and published in early 1935), though they did not mention Noddack's name and arguments in detail (Hahn and Meitner, 1935). They argued, in particular, that their analysis excluded all elements down to mercury, 'hence it becomes very probable that the 13- and 90-minute bodies [i.e., the radioactive products emerging from neutron irradiation] constitute elements beyond 92' (Hahn and Meitner, *loc. cit.*, p. 38). This was the beginning of a story which would occupy the nuclear physicists and nuclear chemists for the next four years, until it resulted in the discovery of a new phenomenon: the fission of the uranium nucleus.[1077]

Before the end of the year 1934, Enrico Fermi and his collaborators already expounded upon a new discovery in the field of nuclear bombardment. In a note submitted to *Ricerca Scientifica*, dated 7 November, Fermi, Amaldi, Bruno Pontecorvo, Rasetti, and Segrè (1934) announced an increase of the radioactivity obtained if a layer of paraffin (a few centimetres thick) was placed between the neutron source and the irradiated substances, and they argued: 'A possible explanation of these facts seems to be the following: neutrons rapidly lose their energy by repeated collisions with hydrogen nuclei. It is plausible that the neutron-proton collision cross section increases for decreasing energy.' (Fermi, Amaldi, Pontecorvo *et al.*, 1934, p. 283) This observation of the effectiveness of slow neutrons for stimulating transmutations played an important role in the later experimental and theoretical investigations.

While the transmutation experiments, which constituted one important ingredient for the theory of atomic nuclei, could be performed with neutrons obtained from the irradiation of beryllium with α-particles from *natural radioactive sources* available everywhere, the other important empirical result in nuclear physics demanded the use of *artificially accelerated protons*, produced by the new devices that had been constructed since 1931, especially in the United States. On 13 December 1935, the *Physical Review* received a detailed paper written by Milton G. White from the Radiation Laboratory in Berkeley, California (headed by Ernest Orlando Lawrence), in which he presented new results from the 'Scattering of High-Energy Protons in Hydrogen' (White, 1936). White reported that he had

[1077] For a chronology of the events and the later developments to be reported below, see Rechenberg, 1988.

analyzed 7340 photographs of the tracks of fast protons obtained with the cyclotron in a Wilson cloud chamber and noticed 'strong anomalies when the energy of the incident proton exceeded 600 kV' as compared with Mott's wave-mechanical treatment (White, *loc. cit.*, p. 309), and he concluded from this evaluation: 'If further data are in substantial agreement with the above observed scattering then the present theoretical ideas about intranuclear forces will have to be seriously modified.' (White, *loc. cit.*, p. 316)[1078] More than half a year later, a team from Washington's Carnegie Institution, consisting of Merle A. Tuve, P. Heydenburg, and Lawrence R. Hafstad, submitted the results of their proton–proton scattering experiments—performed with the Van de Graaff accelerator reaching proton energies up to 1.2 MeV (devised earlier by Tuve, Hafstad, and Otto Dahl), and analyzing the scattered protons with the help of slit systems in an ionization chamber—for publication. In contrast to White who based his conclusions 'on a total of 18 observed particles at high angles with energies over 600 kV,' the experimentalists at the Carnegie Institution registered in their 'final experiments a total of 21,540 particles in the same region [notably, between 600 and 900 keV of the incident proton beam]' (Tuve, Heydenburg, and Hafstad, 1936, p. 807). Thus, as summarized in their abstract, they arrived at the following results:

> At 600 kV the observed numbers at all angles are roughly two-thirds of the values predicted by Mott's formula. The curves for this observed "Mott ratio" *versus* angle change progressively as the voltage is increased and at 900 kV the observations show two-thirds of the Mott value at 15°, 1.4 times Mott at 30°, and 4.0 times Mott at 45°. Measurements of the scattering of protons by deuterium, helium, and air ... have led to the conclusion that the observed anomaly is not due to contamination and must be ascribed to a proton-proton interaction at close distances (less than 5×10^{-13} cm) which involves a marked departure from the ordinary Coulomb forces. (Tuve, Heydenburg, and Hafstad, *loc. cit.*, p. 806)

The deviation of the observed proton–proton scattering data from the values derived from Nevill Mott's well-known scattering formula (see Section III.7) evidently contradicted one of the fundamental assumptions of nuclear theory, namely, that the force between two protons in a nucleus was essentially Coulombian. At the time of the experiments, several possibilities had been discussed, such as whether corrections to the Coulomb potentials arising from the creation of pairs (see Section IV.4) might not give rise to these deviations; moreover, the Fermi field theory, which implied that the proton occasionally be in a neutron-electron state, should also cause deviations—hence, the situation appeared to be quite unclear and complicated.[1079] However, by early 1936, the experts favoured the answer proposed by White and Tuve in their papers; thus, Pauli wrote on 24 February to Gregor Wentzel in Zurich:

[1078] White had already given the initial indications of these anomalies in a letter submitted for publication in March 1935 (White, 1935).

[1079] For instance, White reported another attempt to explain his data: His theoretical colleague Robert Serber introduced a phenomenological potential and obtained for its depth a value of 17.2 MeV (at close distances), which quite contradicted observed mass defects (see White, 1936, p. 316).

> I am just returning [to Princeton] from the New York meeting [of the American Physical Society, 21–22 February 1936] . . . where I also learned much about physics. There were especially Tuve's new experiments on proton-proton scattering, the first which are reliable. In particular, he uses counters instead of the Wilson chamber to detect protons, hence the statistical fluctuation errors (which have rendered everything irregular and uncertain in White's published experiments) are eliminated. The result is: *one needs additional attractive forces between two protons which are of the same order* of magnitude as the forces between protons and neutrons. (Pauli, 1985, p. 441)

We should recall at this point that arguments had been given along that direction in the previous year: Thus, Lloyd A. Young of the Carnegie Institute of Technology had claimed in a letter of May 1935 to *Physical Review* and in a detailed paper submitted in August 1935 'that the empirical data on the binding energies of the heavy nuclei could be explained fairly well by taking all these possible interactions [between nuclear constituents] of the same range and strength' (Young, 1935a; 1935b, especially, p. 913). Then, the careful analysis of Tuve and collaborators decided the question directly, as they stated definitely:

> A complete discussion of the theoretical significance ... may be summarized by the statement that these proton scattering experiments demonstrate the existence of a proton-proton interaction which is violently different from the Coulomb repulsion for distances of separation of the order of 10^{-13} cm. The measurements are qualitatively in agreement, as regards magnitudes, variation with angle, and variation with voltage, with a simple phase shift of the spherically symmetrical de Broglie wave ("S wave") due to the collision or scattering, corresponding to a new force overpowering the Coulomb repulsion, and give a rather accurate measure of the "potential well" which is therefore permissible as representing the interaction. Interestingly enough, this potential well appears to be identical, within the limits of error of both determinations, with the potential well which represents the proton-neutron interaction as derived from the scattering and absorption of slow neutrons. Furthermore, the magnitude of interactions thus determined by the scattering experiments is in very satisfactory agreement with that used successfully for calculations of mass defects of light nuclei. It thus appears that a real beginning has been made toward an accurate and intimate knowledge of forces which bind the "primary particles" into heavier nuclei so important in the structure and energetics of the material universe. (Tuve, Heydenburg, and Hafstad, 1936, pp. 824–825)

The detailed theoretical analysis, to which Tuve *et al.* referred, was provided by Gregory Breit of the University of Wisconsin, Edward U. Condon of Princeton University, and Richard D. Present of Purdue University in a paper prepared in August 1936 for the 'Tercentenary Conference of Arts and Sciences at Harvard University.' By applying the standard theory of scattering in central fields, as a phase-shift analysis in angular momenta $L = 0, \hbar, 2\hbar$ of the experimental data, the theoreticians concluded:

> The experiments of THH [i.e., Tuve, Heydenburg and Hafstad] indicate an interaction potential between protons equivalent to -11.1 MeV in a distance of 2.82×10^{-13} cm acting in addition to a Coulombian repulsion. The potential agrees closely with

that obtained from mass defect calculations which use a neutron-proton interaction depending on spin orientation. Higher phase shifts than those for $L = 0$ are not called for sufficiently definitely to make their existence certain.

The magnitude of the interaction between like particles in ^{1}S states is arrived at here with a relatively high precision. It is compared with the proton-neutron interaction in the corresponding state as derived from the experiments of Fermi and Amaldi. The proton-proton and proton-neutron interactions are found to be equal within the experimental error. This suggests that interactions between heavy [nuclear] particles are equal also in other states. (Breit, Condon, and Present, 1936, p. 845)

Thus, the charge independence of nuclear forces was established as a crucial element of nuclear theory after the mid-1930s.

In the meanwhile, the application of nuclear theory to describe the increasingly available data on the properties of nuclei had progressed steadily, starting from Heisenberg's pioneering work in 1932.[1080] In particular, the liquid-drop model of Gamow, formulated in the language of the proton–neutron constitution of nuclei served as the main tool. At the seventh Solvay Conference in October 1933, Heisenberg had derived an expression for the exchange energy of a nucleus containing n_1 neutrons and n_2 protons, namely,

$$E_{\text{ex}} = \frac{h^2}{2M}\frac{4\pi}{5}\left(\frac{3}{8\pi}\right)^{5/3}(n_1^{5/3} + n_2^{5/3})V^{-2/3} - Vf\left(\frac{n_1}{V}, \frac{n_2}{V}\right), \tag{769}$$

with V denoting the nuclear volume, M denoting the mass of the proton or neutron, and f denoting a function of the neutron and proton densities, and had concluded: 'This shows that the exchange action introduced by Majorana leads, for nuclear matter, to characteristic analogies to those of a liquid.' (Heisenberg, 1934a, pp. 306–307). The total energy could then be written as a sum of the exchange energy, Eq. (769), and the Coulomb energy of the protons in the nucleus (Heisenberg, *loc. cit.*, p. 310),

$$E_C = \left(\frac{3}{5}\right)(n_2 e)^2\left(\frac{3V}{4\pi}\right)^{-1/3}. \tag{770}$$

By evaluating the function $f\left(\frac{n_1}{V}, \frac{n_2}{V}\right)$, Heisenberg had arrived at the energy of the nucleus as

$$\begin{aligned}\frac{\bar{E}}{Mc} &= 0.00347n_2 - 0.0364n_1 + 0.01211\frac{n_1^2}{n_2}\\ &\quad + n_2^{5/3}\left(3.19 - 0.715\frac{n_1}{n_2}\right)\cdot 10^{-4}\ (+0.049),\end{aligned} \tag{771}$$

[1080] For the following part, we refer to the detailed historical study of Stuewer, 1994, and for special aspects to Rechenberg, 1993a.

which fitted Aston's mass defect data reasonably well. In Rome, Gian Carlo Wick then improved upon the new version of the liquid drop model: He pointed to the fact that the binding energy (771) should be reduced somewhat, because the particles at the nuclear surface would be attracted only by half as many particles as those in the interior (Wick, 1934). A year later, Carl Friedrich von Weizsäcker—while working on his *Habilitation* thesis—picked up the problem and, in early July 1935, submitted a paper entitled '*Zur Theorie der Kernmassen* (On the Theory of Nuclear Masses)' to *Zeitschrift für Physik* (von Weizsäcker, 1935a). He started from the following ideas:

> It has now become very probable that protons and neutrons are the only elementary constituents of nuclei. Since the rest energies of these particles are large compared to the binding energy of the nuclei, their movement in nuclei ought to be describable in the first approximation by nonrelativistic quantum mechanics. If the forces between the elementary particles were known, it should be possible in principle to compute the binding energies, i.e., the mass defects, of all atomic nuclei. Since the attempts to determine these forces directly from a theory have not yet led to unique results, we are for the moment directed to use the inverse procedure, namely to derive the nuclear forces from the empirically known mass defects. (von Weizsäcker, *loc. cit.*, p. 431)

In order to arrive at his goal of obtaining a satisfactory theory of nuclear masses, von Weizsäcker selected the following data as the basis: '1. The mass defects of the lightest nuclei ($H_1^2, H_1^3, He_2^3, He_2^4$) increase extremely rapidly with the particle number. 2. The mass defects of heavy nuclei increase about linearly with the particle number. 3. The packing fraction (mass defect per particle) of the lightest nuclei (up to about Fe) are not strictly constant but increase further slowly. 4. The packing fractions of heavier nuclei decrease after being approximately constant [for medium heavy nuclei]. 5. Nuclei with even numbers of protons and neutrons are generally bound somewhat more strongly than those with odd numbers.' (von Weizsäcker, *loc. cit.*, p. 432) In order to fit these facts into a theoretical scheme, von Weizsäcker made use of the Majorana forces and the well-known Thomas–Fermi approximation method for many-particle systems (which Heisenberg had often made use of) and derived a constant particle density for infinitely large nuclei, while for finite ones a 'surface tension' existed, which decreased the binding energy toward the surface of the nucleus (von Weizsäcker, *loc. cit.*, p. 434). In addition, a quantum-theoretical effect which avoided (on account of the uncertainty relation) a discontinuous decrease of the nuclear density at the surface had to be considered, thereby creating a 'kinetic surface tension' which even dominated the normal surface tension (of the classical liquid drop, see von Weizsäcker, *loc. cit.*, p. 435). All the terms added up finally to yield the expression for the total energy:

$$E = [\eta \psi_0^{10/3} - \phi(\psi_0)] \frac{N}{\psi_0^2} + \frac{\zeta \psi_0^2 \lambda}{2} YZ + E_C, \tag{772}$$

where $E_C = \left(\frac{3}{5}\frac{Ze^2}{r}\right)$ denoted the Coulomb energy of Z protons when evenly distributed in a sphere of radius r, ψ_0^2 denoted the constant Majorana density of nuclear particles in the interior of the sphere, and ϕ denoted Heisenberg's function f, Eq. (769)—which could be written as a complicated function of the constants a and b of the Majorana potential $J(r) = a\exp(-br)$ and of ψ_0. The constants η and ζ were given by

$$\eta = \frac{8\pi h^2}{5M}\left(\frac{3}{8\pi}\right)^{5/3} \quad \text{and} \quad \zeta = \frac{h^2}{4\pi^2 M}. \tag{772a}$$

Von Weizsäcker evaluated Eq. (772) by constructing tables; the numbers obtained in them reproduced the dependence of the mass defects on atomic numbers reasonably well, but—as von Weizsäcker assumed, because of the deficiency of the Thomas–Fermi method—'no quantitative conclusions could be derived from the mass defects on the proton-neutron interaction'; hence, he called his theory 'a kind of phenomenological description of nuclear masses': It yielded the result 'that the nuclear energies can be considered at all as the sum of a term proportional to the volume energy, one to the surface energy, and another to the Coulomb energy' (von Weizsäcker, *loc. cit.*, p. 443). Following further physical arguments, he eventually arrived at the 'semi-empirical' formula

$$E(Z,N) = \left[-\sqrt{\alpha^2+\beta^2} + \sqrt{\alpha^2+\beta^2\frac{(Z-N)^2}{(Z+N)^2}}\right]\cdot\left[(Z+N-1) - \gamma(Z+N-1)^{2/3}\right]$$
$$+\frac{3e^2}{r_0(Z+N)^{1/3}}\left(1-\delta\frac{|Z-N|}{Z+N}\right)\left[\frac{Z^2}{5}-\left(\frac{Z}{2}\right)^{4/3}\right]. \tag{773}$$

He determined the constants α, β, γ, δ, and r_0 (i) by fitting with the light-nuclei data, or (ii) by fitting with the values for heavy nuclei. He found both methods to agree to some extent; in particular, they led to a reasonable 'effective radius' parameter r_0.

It should be emphasized that Hans Bethe, in Part A of his review of nuclear physics (published with Robert Fox Bacher in the April 1936 issue of *Reviews of Modern Physics*), used a 'slightly simpler' form of the total energy of atomic nuclei, namely,

$$E = NM_n + ZM_p - \alpha A + \beta(N-Z)^2/A + \gamma A^{2/3} + \frac{3}{5}\left(\frac{e^2}{r_0}\right)Z^2A^{-1/3}, \tag{774}$$

where A, N, and Z denoted the atomic number, the number of neutrons, and the number of protons, respectively, and M_n and M_p denoted the slightly different masses of the nuclear constituents. Evidently, the nuclear radius became $r_0A^{1/3}$, and the (Bethe-Bacher) constants α, β, γ and r_0 were derived from the empirical data ($r_0 = 1.48 \times 10^{-13}$ cm, $\frac{3}{5}\frac{e^2}{r_0} = 0.58$ MeV).[1081] Formulae of the type (773) or (774) were henceforth referred to as the 'Bethe–Weizsäcker formulae.'

During the period between 1935 and 1937, the detailed description of the nuclear constitution attracted the attention of more and more physicists. Besides the 'standard' liquid-drop model in its various forms, the alternative proposal existed, pursued especially by Walter Elsasser in Paris, for calculating the energy levels of a neutron–proton system in a potential hole with infinitely high walls; thus, a shell structure of the nuclear particles could be derived that explained the three 'magic numbers' for the nuclear constituents, i.e., the relative abundance of certain isotopes (Elsasser, 1933; 1934a, b).[1082] On the other hand, Werner Heisenberg—in his last publication on nuclear structure, dealing with '*Die Struktur der leichten Kerne* (The Structure of Light Nuclei)' and submitted to *Zeitschrift für Physik* in July 1935—sought to resolve the discrepancy between the values of the parameters a and b of the Majorana potential gained from the semi-empirical formula of von Weizsäcker and evaluated directly by Eugene Wigner (1933a). He proposed to replace the Thomas–Fermi approximation method, which had been preferred thus far and accounted badly for the data on light nuclei, by a Hartree–Fock method (Heisenberg, 1935b). Heisenberg then described the eigenfunctions of the nuclei by the product of suitable eigenfunctions for the individual protons and neutrons, choosing for them those of the harmonic oscillator; thus, he indeed arrived at a better agreement with the earlier descriptions of the helium nucleus (see, e.g., Feenberg, 1935), but he also concluded:

> A determination of the values of a and b individually (i.e., not just the relation between a and b) from computations of the above type would, however, hardly be possible at all. In order to obtain the values of the constants individually, one would rather have to turn to more detailed features of the nuclear constitution, which depend crucially on the single values of the constant (e.g., the mass defect of the deuteron). Moreover, the question whether—besides the Majorana exchange forces—still other smaller forces (e.g., forces between equal particles) play a role in the constitution of nuclei will only be answered by taking into account such refined features of nuclear structure. (Heisenberg, 1935b, p. 484)

Heisenberg then asked one of his doctoral students to carry out such evaluations in the case of the lightest nuclei (deuteron, triton, and α-particle): Heimo Dolch indeed showed that, by taking slight variation of Eugene Feenberg's potential, i.e.,

$$V(r_{ik}) = a \exp(-b^2 r_{ik}^2) P(ik), \tag{775}$$

[1081] In obtaining Eq. (774), Bethe corrected a computational error in von Weizsäcker's formula (773). (See the Interview of Hans Bethe, with Charles Weiner and Jagdish Mehra, 27–28 October 1966, p. 18.)

[1082] A brief account of the development of the 'shell model' of nuclear structure was given by Bethe, 1979, pp. 15–18.

a fit of the ground states of these nuclei seemed to be possible with *one* set of parameters (Dolch, 1936).

For some years at Heisenberg's institute, a small group dealing with nuclear theory and a detailed description of nuclear energy states continued to be active.[1083] In September 1935, von Weizsäcker also included the results of this Leipzig group in his review talk on '*Die für den Bau der Atomkerne maßgebenen Kräfte* (The Decisive Forces Determining the Structure of Atomic Nuclei)' (Weizsäcker, 1935b).[1084] At the end of his lecture, von Weizsäcker referred to certain 'as yet unpublished calculations of the speaker' which extended the previous theory of nuclear forces:

> To each dependence of the [nuclear] force on the mutual distance of particles, however, there also accompanies a dependence on the spin direction. The exact form of this spin-spin and spin-orbit force depends on the special form of the chosen *Ansatz*; in any case, it will essentially be larger than the influence of the spin's magnetic moment [on the nuclear force]. This result implies that the coupling situation between the different angular-momentum vectors of orbit and spin in the nucleus cannot—as in the theory of atomic shells—be computed from the electric and magnetic forces. Therefore, at the moment, no deductive theory of the nuclear spin is possible. On the other hand, the spin systematics will now obtain an interest for the problem of nuclear forces; perhaps at a later time exactly the empirical organization of nuclear spins may contribute in deciding between the different forms proposed for the theory of β-decay. (von Weizsäcker, *loc. cit.*, p. 785)

In his *Habilitation* thesis, entitled '*Über die Spinabhängigkeit der Kernkräfte* (On the Spin Dependence of Nuclear Forces)' and submitted to *Zeitschrift für Physik* in June 1936, von Weizsäcker then generalized the earlier considerations on nuclear forces, especially by taking into account spins, with the goal of arriving at a relativistic theory (von Weizsäcker, 1936b). As the conceptual basis of the

[1083] With the participation of Siegfried Flügge, who joined Heisenberg's institute in Leipzig in 1935, this group included the Chinese student Wang Foh-san, (occasionally) Hans Euler, Berndt Olof Grönblum from Finland, and Harold Wergeland from Norway, and from Japan Satoshi Watanabe and Sin-itiro Tomonaga. (For details, see Rechenberg, 1993a).

[1084] Carl Friedrich von Weizsäcker was born in Kiel on 28 June 1912, the son of a Navy officer (and later diplomat and politician Ernst von Weizsäcker). He grew up in various German cities and abroad (including The Hague, Basel, and Copenhagen), but completed his *Abitur* in Berlin in 1929; then, he studied physics in Berlin, Göttingen, and Leipzig, obtaining his doctorate (under Heisenberg) in 1933 and his *Habilitation* in 1936. In fall 1936, he joined the new *Kaiser Wilhelm-Institut für Physik* in Berlin, of which Peter Debye was then director, where he also became *Privatdozent* at the University (in 1937). Until fall 1942, he participated in the German nuclear energy project, and then obtained an extraordinary professorship of theoretical physics at the University of Strassbourg. In 1944, he returned to Germany and was interned after the war in England until January 1946. From 1946 to 1957, he directed the theory division at the *Max Planck-Institut für Physik* (the reconstituted *Kaiser Wilhelm-Institut*) in Göttingen; then, he was appointed a professor of philosophy at the University of Hamburg. From 1970 to 1980, he served as director of the *Max Planck-Institut zur Erforschung der Lebensbedingungen der wissenschaftlich-technischen Welt*, which he established in Starnberg near Munich. He retired in 1980, but continued to do research, write books, give lectures, and take part in scientific affairs.

approach, he made use of the well-known (and generally accepted) Fermi field theory, which implied as an elementary process the transformation of a neutron into a proton and the pair of light particles electron plus neutrino; he further assumed that both the light nuclear particles *and* the heavy ones, protons and neutrons, obeyed Dirac equations with the wave functions ψ, ϕ and Ψ, Φ, respectively (and even the hole theory)—a point of view which contrasted with the then held majority opinion. Proceeding along these lines, von Weizsäcker derived—in the nonrelativistic limit for the heavy nuclear particles—four different forms of the interaction term, H_a, H_b, H_c, and H_d, of which the first two represented scalar coupling between the fields and the other two vector coupling. Among the latter, that is,

$$H_c = g\Sigma \int s^{\nu}(\Psi^* \Phi) s_{\nu}(\psi^* \phi)\, d\mathbf{r} + \text{conjugate}, \tag{776}$$

$$H_d = g\Sigma \int s^{\nu}(\Psi^* \psi) s_{\nu}(\psi^* \Phi)\, d\mathbf{r} + \text{conjugate}. \tag{776a}$$

H_c described the original Fermi-*Ansatz*, and H_d described the Heisenberg version of the Majorana force. By summing over the spin indices of the light particles and integrating over their momenta, von Weizsäcker arrived at expressions for the 'relativistic' proton–neutron potential in Pauli's spin approximation, which could be expanded as a sum of terms of zeroth, second, and fourth order in the momenta of the heavy particles proton and neutron. Finally, he derived the existence of the Heisenberg and Majorana forces, and also the additional magnetic moments (as compared to the standard ones obtained from the Dirac equation) of the proton and neutron, having the order of magnitude

$$\mu^c = \frac{4\pi e g^2}{5c^2 h^5 \alpha^3} \quad \text{and} \quad \mu^d = \frac{6\pi e g^2}{5c^2 h^5 \alpha^3}, \tag{777}$$

with e denoting the charge of the exchanged electron or positron and g denoting the coupling constant of Fermi's theory. If $\alpha h/2\pi$ were identified with the nuclear radii (which served as a cutoff in the strongly divergent expressions of the Fermi field theory), the nuclear moments and the exchange forces turned out to be negligible; however, when fitting the parameter α by inserting the empirical magnitude of the exchange forces, the magnetic moments (777) also grew to assume nearly the observed order of magnitude.

A little earlier than von Weizsäcker's *Habilitation* thesis, a monumental review article appeared on 'Nuclear Physics. A. Stationary States of Nuclei' in *Reviews of Modern Physics*. The authors—Hans A. Bethe and Robert F. Bacher—after presenting the essential nuclear data and deriving some qualitative conclusions, displayed in detail the theory of the lightest nuclei (deuteron to α-particle), and outlined the various descriptions of atomic nuclei (statistical and semi-empirical approximations for the heavy nuclei); then, they discussed the Fermi field-theoretical approach to nuclear forces and the description of nuclear moments (Bethe and Bacher, 1936). In short, the account given here—as far as the theoretical aspects were concerned—more or less dealt with the same developments which von Weizsäcker addressed in his two major papers on nuclear theory of 1935 and 1936, thus exhibiting the simultaneity of theoretical interests in nuclear physics on both sides of the Atlantic ocean, as represented by Bethe and von Weizsäcker, respectively.[1085] Soon, however, other physicists joined them. Considerable stimulus was provided by the American experiments mentioned above, which demonstrated the existence of attractive proton–proton forces, having the same (exchange) character and magnitude as Heisenberg's proton–neutron force of 1932. Around 10 August 1936, the *Physical Review* received four papers on the topic: one, experimental, by Tuve, Heydenburg, and Hafstad (1936, already mentioned), and three theoretical ones by Breit, Condon, and Present (1936, also already mentioned), Cassen and Condon (1936), and Breit and Feenberg (1936). While the first theoretical paper of Breit, Condon, and Present contained an analysis of the experimental proton–proton scattering data, yielding the result of approximately equal magnitude of proton–proton and proton–neutron forces, the latter two derived the consequences for the theory of nuclear forces. In particular, Bernard Cassen and Edward Uhler Condon found: 'The various types of exchange forces that are being used in current discussions of nuclear structure may all be simply expressed in terms of a formalism which attributes five coordinates to each "heavy" particle and applies the Pauli exclusion principle to all the particles in the system,' and further: 'The simplest assumption for the interaction law is that which implies equality of proton-proton and proton-neutron forces of corresponding symmetry ... in accord with the empirical knowledge of these interactions at present.' (Cassen and Condon, 1936, p. 846) The fifth coordinate mentioned here was introduced by Heisenberg's description of protons and neutrons by the ρ-matrix, later renamed the τ-matrix. Cassen and Condon developed the τ-matrix formalism in detail and used it to express the different types of nuclear forces considered so far: the ordinary Wigner potential (V) and the exchange potentials of Heisenberg (H), Bartlett (B), and Majorana (M). Thus, they obtained the most general nuclear potential

$$U = V + V_h H + V_b B + V_m M, \tag{778}$$

[1085] Of course, important differences showed up in the respective treatments of the various topics, which could especially be noticed in the mathematical style and the use of experimental data. Thus, von Weizsäcker employed more general formulae and less experimental details, while Bethe (with Bacher) focused on experimental details and considered only the meticulous approximation methods. The same difference was characterized by the contents of von Weizsäcker's book *Die Atomkerne*, which he delivered to the publisher in September 1936 (von Weizsäcker, 1937a), when compared with the three monumental review articles of Bethe (Bethe and Bacher, 1936; Bethe, 1937; Livingston and Bethe, 1937). Hans Bethe placed great emphasis on the reliability of the description of the available data, while von Weizsäcker rather outlined the fundamental ideas and was occasionally criticized for slips in his calculations (see, e.g., Pauli to Heisenberg, 24 November 1936, in Pauli, 1985, p. 479).

expressed by the distance-dependent functions V_h, V_b, and V_m and the spin and τ-spin dependent specific operators H, B, and M. Cassen and Condon then described the deuteron states, the capture of neutrons by protons and the proton–proton scattering by their formalism. Breit and Feenberg proceeded in a similar manner to obtain 'a universal form of interaction for all nuclear particles'; i.e.,

$$V_{ij} = \{(1 - g - g_1 - g_2)P_{ij}^M + gP_{ij}^H + g_1 \cdot 1 + g_2 P_{ij}^S\} J(r_{ij}), \tag{779}$$

where g, g_1, and g_2 were constants, P_{ij}^M and P_{ij}^H were the Majorana and Heisenberg operators, $P_{ij}^S = P_{ij}^M P_{ij}^H$ was the Bartlett operator, and $J(r_{ij})$ was the Yukawa potential (Breit and Feenberg, 1936, p. 850). They could account for the observed binding energies of light and heavy nuclei by assuming for the latter case the inequality

$$1 + g + \frac{6e^2}{5r_0|J(0)|} \geq 5g_1 + 3g_2, \tag{780}$$

with r_0 as the nuclear radius.

The charge-symmetrical nuclear forces (of Cassen and Condon, and Breit and Feenberg) were immediately accepted by the experts in the United States and Europe, especially in Germany.[1086] From a more general theoretical point of view, two relatively senior physicists independently drew consequences from the new symmetry: Eugene Wigner in Wisconsin and Friedrich Hund in Leipzig. Wigner had made his entrance into nuclear theory by investigating the mass defect of helium (Wigner, 1933a), and he also introduced a very short-range potential to describe the scattering of protons and neutrons and to fit the mass defect of Harold Urey's heavy-hydrogen nucleus (Wigner, 1933b). After an interruption of a couple of years, he returned to the problem of nuclear structure in late 1936 and at first discussed the saturation of exchange forces (Wigner, 1936). Subsequently, he analyzed with Eugene Feenberg the empirical binding energies of nuclei from helium to oxygen; in their paper, submitted in October 1936, Wigner and Feenberg attempted to answer the question 'whether or not the difference between proton–proton and neutron–neutron interaction is only the Coulomb force' and concluded: 'One cannot claim with certainty at present that the neutron–neutron interaction is stronger than the proton–proton interaction.' (Feenberg and Wigner, 1937, p. 93 and p. 103) Based on the recent experimental investigations of Merle Tuve and his collaborators and the theoretical evaluation of these proton–proton scattering data by Breit, Condon, and Present, then Wigner derived 'Consequences of the Symmetry of the Nuclear Hamiltonian on the Spectroscopy of Nuclei'—this being the title of a paper submitted in late October 1936 and published in the 15 January issue of *Physical Review*—by extensively invoking Hei-

[1086] In particular, Heisenberg's students Hans Euler and Helmut Volz, like Siegfried Flügge, examined the consequences for light and heavy nuclei in early 1937 (see Rechenberg, 1993a, pp. 40–42).

senberg's variable ρ for protons and neutrons (which, like the others, he rewrote as τ), denoting what 'we shall call the isotopic spin' (Wigner, 1937a, p. 106). That is, he investigated 'the structure of the multiplets of nuclear terms, using as a first approximation a Hamiltonian which does not involve the ordinary spin and corresponds to equal forces between all nuclear constituents, protons and neutrons' (Wigner, *loc. cit.*). Recalling here the fact that in December 1932 Heisenberg had used only those wave functions, which had a constant total τ, for determining the energy states of nuclei (see Heisenberg, 1933, p. 588), we note that now in 1936—*under the influence of new experimental information*—Wigner went far beyond in demanding a new τ-symmetry. 'The multiplets [of nuclear energy states] turn out to have a rather complicated structure, instead of the S of atomic spectroscopy one has S, T, Y,' he wrote, thereby introducing a new symmetry group, in which the z-components of spin, isotopic spin and of a new variable Y played a crucial role. By working out the consequences for the Hamiltonian of nuclei in accordance with the familiar methods of group theory, and finally also taking into account the spin forces, Wigner succeeded in explaining qualitatively the observed ground states of stable nuclei up to about Mo. In a second paper, 'On the Structure of Nuclei beyond Oxygen' which he submitted in March 1937, he derived the relation between 'the kinks in the mass defect curve with the energy differences between isobars, both as obtained from direct measurements and from the shift of the isotopic number to higher values with increasing number of the particles,' from his group-theoretical scheme (Wigner, 1937b, p. 947). In this paper he also referred to a publication of 'Friedrich Hund, *Zeitschrift für Physik* to appear soon' (Wigner, *loc. cit.*, p. 947, footnote 1).

In 1935 and 1936, when dealing with the properties of matter at extreme density and temperature, Friedrich Hund had come upon the problems of nuclear structure. Then, in fall 1936, he took a more detailed look at this question, as may be seen from the note in his *Tagebuch*, dated 14 October: 'Deuteron obtains as the lowest term not a triplet or a singlet but as a group of four terms (like the H_2-molecule without the Pauli principle.' During the following months, he examined the nuclear data from the point of view of the Pauli principle and the Hartree approximation, and then talked about his preliminary results on 9 January 1937, at the *Gauvereinstagung* in Freiburg, and finally composed the paper entitled '*Symmetrieeigenschaften der Kräfte in Atomkerne und Folgen für deren Zustände, insbesondere der Kerne bis zu 16 Teilchen* (Symmetry Properties of the Forces in the Atomic Nuclei and Consequences for Their States, in Particular of Nuclei up to 16 Particles)' (Hund, 1937a).[1087] Like Wigner, Hund profited from a long and thorough acquaintance with symmetry methods in quantum-mechanical problems (see Section III.4) which he now displayed in nuclear theory. As the fundamental

[1087] In a footnote, Hund referred to the work of Feenberg and Wigner (1937) and Wigner (1937a) which had meanwhile appeared in print (and of which he had heard as early as 9 January 1937—see the entry in his *Tagebuch*)—but he added: 'Since the applications are a bit different, also since Wigner's presentation seems to me a rather condensed one, I still wish to publish my investigation.' (Hund, 1937a, p. 102, footnote 1) properties and invariances of nuclear constituents and forces, he noted the following: '1. The nuclei are free in space; 2. All neutrons and protons are equal; 3. The Coulomb forces which distinguish between neutrons and protons can be neglected as compared to nuclear forces; 4. Spin-orbit couplings play a minor role; 5. The forces between the nuclear constituents are essentially of the same order of magnitude; 6. Space-dependent nuclear forces (Majorana forces) dominate; 7. The single nuclear constituents may be regarded as being acted upon by spherically symmetric fields.' (Hund, *loc. cit.*, pp. 202–203) He thus immediately derived several consequences: First, nuclei with even particle numbers possessed integral, while those with odd numbers half-integral angular momenta; second, the nuclear wave functions were antisymmetrical in the variables of space, spin, and ρ-spin of all particles involved, and a permutation of the isospin-coordinate of the constituents would not, in the first approximation, affect the energy terms; third, the nuclear terms were described, similar to those of atoms, by quantum numbers L, S, J, and in addition by the quantum number R of the ρ (or isotopic) spin, such that $2R + 1$ nuclei formed a charge multiplet—e.g., the nucleus ${}^{42}P_{3/2}$ was a member of the multiplet ${}^{2}P$ $(L = 1, S = 1/2, J = 3/2)$ with the isotopic spin-$\frac{3}{2}$ and the charge multiplicity 4; fourth, the restriction to space-dependent forces led to the coincidence of a number of spin- and charge-multiplets. In the special case of nuclei up to 16 particles, Hund showed that the energy values were given by his peculiar 'symmetry characters' (see Section III.4), and he obtained a set of simple rules for the ground state of nuclei—with the detailed splitting depending on the type of forces (Majorana, Wigner, Heisenberg, or Bartlett) to be assumed.

During the year 1937, Hund continued his interest in nuclear structure and investigated specific questions, such as the α-particle model of nuclei or the calculation of nuclear momenta. In September, at the Bad Kreuznach meeting of the German Physical Society, he presented a review talk on '*Theoretische Erforschung der Kernkräfte* (Theoretical Investigation of Nuclear Forces),' in which he summarized the progress achieved since 1936 (and von Weizsäcker's 1935 talk at Stuttgart), especially in the USA and at Leipzig in Germany. He concluded his report by saying:

> The past [few] years have provided us with a more accurate qualitative knowledge of forces between the nuclear particles; notably, they taught us that between the equal particles approximately equal forces act as between the non-equal ones. Thus a better approximation for computing nuclear properties is possible. Two limiting cases of approach, the model of single elementary particles in a spherically-symmetric field of force, and the model of a rather rigid scaffolding of α-particles containing a few surplus elementary particles [Hund referred here to the investigations of W. Wefelmeier, see below], enable us to understand some general properties of nuclear energies, angular momenta and magnetic moments. But this knowledge does not yet extend to individual points. (Hund, 1937b, p. 935)

That is, much had yet to be done in experimental and theoretical studies, but as Hund emphasized: 'The goal is worth the effort, since we are dealing with forces that are quite different from the hitherto known electromagnetic and gravitational forces and represent something new *versus* these two types of forces.' (Hund, *loc. cit.*) The next advance in the nuclear force problem came from two sides: first, the development of Yukawa's theory of nuclear forces, based on the assumption of the existence of new particles (as discussed above); second, the progress in the theoretical description of nuclear reactions to which we shall now turn.

In May 1936, George Gamow signed the preface to the second edition of his earlier monograph—*Constitution of Atomic Nuclei and Radioactivity* (1931)—now entitled *Structure of Atomic Nuclei and Nuclear Transformations*, in which he considered as 'the main aim [to deal with] questions of principle concerning nuclear structure and to understand the different nuclear processes from the point of view of the present quantum theory' (Gamow, 1937, p. viii). Besides the knowledge of nuclear structure, the knowledge of nuclear processes had also undergone great changes since 1931, mainly produced by the increasing experimental studies made with artificially accelerated particles or slow neutrons, and it now seemed to Gamow that 'most of the previous calculations concerning nuclear processes must be abandoned or considerably changed' (Gamow, *loc. cit.*, p. vii). In particular, he referred in this context to a recent note on 'Neutron Capture and Nuclear Constitution,' published by Niels Bohr in the issue of *Nature* of 29 February 1936 (Bohr, 1936a). Bohr had personally informed Gamow earlier about this paper in a letter:

> As you will see from the enclosed article, which will soon appear ... this is a development of thought which I already brought up at the last Copenhagen conference in the autumn of 1934, immediately after Fermi's first experiments on the capture of fast neutrons, and which I have taken up again after the latest wonderful discoveries of slow neutrons ... Kalckar and I are at this moment engaged in working out a detailed formulation of the consequences of the theory. (Bohr to Gamow, 26 February 1936, in Bohr, 1986, p. 20)

The members and visitors present in 1934 at Bohr's Institute in Copenhagen confirmed the great impact of the experiments performed by Enrico Fermi and his collaborators in Rome (see, e.g., Wheeler, 1979, p. 253). Otto Robert Frisch especially recalled an incident in this context:

> I vividly remember the occasion: Bohr repeatedly (more than usually) interrupted a colloquium speaker who tried to report on a paper (by Hans Bethe, I believe) on the interaction of neutrons with nuclei; then, having got up once more, Bohr sat down again, his face suddenly quite dead. We watched him for several seconds, getting anxious; but then he stood up again and said with an apologetic smile, "Now I have understood it all"; and he outlined the compound nucleus idea. (Frisch, 1979a, p. 69)[1088]

[1088] See also Frisch, 1979b, p. 107, for a similar account. See further the historical introduction by Peierls for details of Niels Bohr's work on nuclear theory (Peierls, in Bohr, 1986, especially, pp. 14–41), and Stuewer, 1985.

The origin of the important idea of the compound nucleus, and the subsequent work of Niels Bohr and Fritz Kalckar on it, may indeed be traced back to the response to a previous publication of Hans Bethe, who in turn was among the first theoreticians to react publicly to the experimental findings of the Rome group on the disintegration of nuclei by slow neutrons (Fermi, Amaldi, d'Agostino, Rasetti, and Segrè, 1934, see above). Already in a contribution to the New York meeting of the American Physical Society in February 1935, Bethe talked about an attempt to explain the large cross sections observed (Bethe, 1935a)—which he 'realized at the time of the meeting to be an unsuccessful attempt,' as he stated in the following detailed paper on the 'Theory of Disintegration of Nuclei by Neutrons' (received by *Physical Review* on 26 March 1935; Bethe, 1935b, p. 747, footnote 1). As he noted: 'We want to show in this paper that a straightforward application of wave mechanics leads to cross sections of just the right magnitude,' and he stressed at the same time that 'long distance forces between neutrons and nucleus are not required, it being assumed that the interaction is appreciable only when the neutron is inside the nucleus' (Bethe, *loc. cit.*, p. 748).[1089] In particular, Bethe found:

> The large disintegration cross sections are due to two factors. The first is elementary: the cross section is inversely proportional to the neutron velocity, because a slow neutron stays longer in the nucleus. The second factor is $1/\sin^2 \phi_0$, where ϕ_0 is the phase of the neutron wave function at the nuclear boundary. This resonance factor explains the large differences between the cross sections of different elements. ϕ_0 cannot be predicted theoretically, but reasonable assumptions lead to agreement with experiment. The resonance factor occurs in all phenomena with slow neutrons; therefore large cross sections should always be accompanied by large scattering. (Bethe, *loc. cit.*, p. 747)

He concluded: 'The explanation of the large neutron cross sections on the basis of ordinary wave mechanics makes one confident in the applicability of orthodox quantum theory in nuclear phenomena.' (Bethe, *loc. cit.*)[1090]

Actually, Bethe had concerned himself earlier with the whole problem: In September 1934, he had proposed, in a lecture at Bohr's Institute in Copenhagen, a description of the observed large neutron cross sections in terms of a single-particle theory of nuclear reactions. After more than a year, in a letter to Léon Rosenfeld, Niels Bohr referred to his reaction to this lecture:

> I have taken up an old idea again, which already occurred to me in the discussion with Bethe during the last conference in Copenhagen, namely that the motion of the neutron which penetrates into the nucleus can in no way be described as a one-body

[1089] Bethe—in his paper, 1935b, p. 747, footnote 1—mentioned that Enrico Fermi in Rome, Francis Perrin and Walter Elsasser in Paris, and Guido Beck and L. H. Hossley in Kansas, had thought about similar explanations.

[1090] With the help of these ideas, Bethe investigated in the same paper the following phenomena: elastic scattering of neutrons by nuclei; the capture of neutrons with the emission of particles; and he also compared the theoretical predictions with the few experimental data then available.

> problem in a static potential, but on the contrary the neutron will so-to-speak share its energy with the other nuclear particles, and create an intermediate system with a sufficiently long lifetime so that there remains a large probability of radiative transition, before a neutron or another particle leaves the system as the result of an escape process which has no direct connection with the capture process. This point of view seems not only to explain the neutron capture, but also to solve a large number of other difficulties, with which Gamow has struggled on the basis of his schematic model of the nucleus. (Bohr to Rosenfeld, 8 January 1936, in Peierls, 1986, p. 19)

Now Bethe's approach of March 1935, also dealing with a large number of nuclear processes, may be considered as a response to Bohr's criticism of his earlier one-particle description of the phenomena connected with the bombardment of nuclei by neutrons; however, shortly before Bohr published his alternative ideas, the *Physical Review* received a paper on the 'Capture of Slow Neutrons' by Gregory Breit and Eugene Wigner, also suggesting a theoretical interpretation of the same phenomena (Breit and Wigner, 1936).[1091]

In the introduction of their paper (whose contents they also presented at the American Physical Society meeting in New York in February 1936), Breit and Wigner criticized 'the current theories of the large cross sections of slow neutrons,' such as Bethe's [1935b], because these 'expected large capture of thermal energies'; however, they noted:

> This consequence of the current theories is apparently in contradiction with experiment, there being no evidence of a large scattering in good absorbers. It also follows from current theories with very few exceptions that the capture should vary inversely as the velocity of the slow neutrons. Experiments on selective absorption recently performed indiated that there are absorption bands characteristic of different nuclei and it appears from the experiments of Szilard that these bands have fairly well-defined edges. It has been pointed out by Van Vleck [1935] that it is hard and probably impossible to reconcile the difference in internal phase required by the Bethe-Fermi theory with reasonable pictures of the structure of the nucleus. (Breit and Wigner, 1936, p. 519)

Breit and Wigner therefore replaced the so-called Bethe–Fermi approach to neutron absorption by a resonance mechanism, which Wigner and Michael Polanyi

[1091] We have come across the contributions of Gregory Breit to different quantum-mechanical problems at several places; hence, it is appropriate to introduce him biographically. Breit was born on 14 July 1899, in Nikolajev, Russia, from where he emigrated to the United States in 1915. He began to study at Johns Hopkins University and graduated with a Ph.D. thesis under Joseph S. Ames. Following a postdoctoral year (as a National Research Council Fellow) at the University of Leyden, he was appointed in 1923 as an assistant professor at the University of Minnesota. In 1924, he joined the Carnegie Institution in Washington, D.C., as a mathematical physicist, and in 1929, he obtained a professorship of physics at New York University (after another European excursion in 1928, working with Pauli at the *ETH* in Zurich). From 1934 to 1947, Breit taught at the University of Wisconsin in Madison, afterward (until 1968) at Yale University, and finally at the State University of New York in Buffalo, New York. During World War II, Breit worked on nuclear energy and other war-related projects. He died on 13 September 1981, in Salem, Oregon.

had already used over ten years earlier to describe the inverse Auger effect (Polanyi and Wigner, 1925). For this purpose, 'it will be supposed that there exist quasi-stationary (virtual) energy levels of the system nucleus + neutron which happen to fall in the region of thermal energies as well as somewhat above that region,' Breit and Wigner argued, and continued:

> The incident neutron will be supposed to pass from its incident state into the quasi-stationary level. The excited system formed by the nucleus and the neutron will then jump into a lower level through the emission of γ-radiation or perhaps in some other fashion. The presence of the quasi-stationary level, Q, will also affect scattering because the neutron can be returned to its free condition during the mean life of Q. If the probability of γ-ray emission from Q were negligible there would be in fact strong scattering at the resonance, the scattering cross section being then of the order of the square of the wavelength. (Breit and Wigner, 1936, pp. 519–520)

Consequently, Breit and Wigner developed a systematic theory of damping in quantum mechanics, arriving at the capture and scattering cross section formulae,

$$\sigma_c = \frac{\Lambda^2}{\pi} S \frac{\Gamma_s \Gamma_r}{(\nu - \nu_0)^2 + \Gamma^2} \tag{781a}$$

and

$$\sigma_s = \frac{\Lambda^2}{\pi} S \frac{\Gamma_s^2}{(\nu - \nu_0)^2 + \Gamma^2}, \tag{781b}$$

where Λ and S denoted the de Broglie wavelength of the incident neutron and the statistical factor $2L + 1$ of the nuclear level (with L its angular momentum), respectively, Γ_r and Γ_s denoted the half-value widths for the capture and the scattering ($\Gamma = \Gamma_r + \Gamma_s$), and $h\nu_0$ denoted the (central) energy of the quasi-stationary state. They summarized their conclusions as:

> Interaction with the nucleus is most probable through the S part of the incident wave. The higher the resonance region, the smaller will be the absorption. For a resonance region at 50 volts the cross section at resonance may be as high as 10^{-19} cm^2 and 0.5×10^{-20} cm^2 at thermal energy. The estimated probability of having a nuclear level in the low energy region is sufficiently high to make the explanation reasonable. Temperature effects and absorption of filtered radiation point to the existence of bands which fit the present theory. (Breit and Wigner, *loc. cit.*, p. 519)

Evidently, Bethe accepted Breit and Wigner's theory as the explanation of slow-neutron phenomena in nuclei; in 'An Attempt to Calculate the Number of Energy Levels of a Heavy Nucleus,' submitted in early June 1935 to *Physical Review*, he applied the idea to a model in which he considered the nucleus to be represented by a Fermi gas (Bethe, 1936). In the same paper, he also referred to the note of Niels Bohr in the 29 February issue of *Nature*, which assumed the existence of excited nucleon-neutron states, and quoted experiments verifying the Breit–Wigner resonances ranging from 0.1 volt to about 50 volts (Bethe, *loc. cit.*, p. 332).

Toward the end of 1935, Bohr prepared the way for an initial formulation of his ideas on the 'Neutron Capture and Nuclear Constitution,' first in a communication to the Royal Danish Academy, dated 24 January 1936, then in an extended note to *Nature* (Bohr, 1936a).[1092] Concerning the last stages of his considerations, he informed Heisenberg in a letter, dated 8 February 1936:

> I have worked hard to the last minute finishing the small article on nuclear reactions which I promised you long time ago, but the matter was continuously developing for me, and it gradually became a more comprehensive point of view, which I believe to be of use for the understanding of many different nuclear problems.... The small note, of which I enclose a manuscript, is only an approximate reproduction of a lecture ... You should not worry about my remarks on the constituents of the nucleus, which in this context are of minor importance. This does not imply any lack of understanding of yours and Fermi's great contributions, but only a certain skepticism concerning the details, not at least in the application of the Pauli principle, which the new points of view have introduced. (Bohr to Heisenberg, 8 February 1936; English translation in Peierls, 1986, pp. 19–20)

The result of Bohr's last-minute labour, namely, working out the contents of his address at the Royal Danish Academy in Copenhagen, was indeed the five-page note in *Nature*, which Bohr opened by outlining a few empirical facts that had been obtained on this topic during the previous couple of years, after which he claimed: 'The phenomena of neutron capture ... force us to assume that a collision between a high-speed neutron and a heavy nucleus will in first place result in the formation of a compound system of remarkable stability' (Bohr, 1936a, p. 344). This intermediate 'compound system' would later break up by the ejection of material particles or radiation into the final state; thus, the initial collision process and the eventual break-up had 'to be considered as separate competing processes which have no immediate connexion.' Bohr further concluded: 'In view of the close packing of the particles and nuclei we must be prepared ... for just such energy changes to play a predominant role in typical nuclear reactions' (Bohr, *loc. cit.*). In detail, the initial excess energy of the incident neutron would 'be rapidly divided among all the nuclear particles with the result that for some time afterwards no single particle will possess the kinetic energy to leave the nucleus' (Bohr, *loc. cit.*, p. 345). Bohr then proposed a preliminary—not a detailed one, as he emphasized—picture of such processes, taking into account the old difficulties of having individual particles (especially electrons) within the nucleus. He argued that in the nucleus 'we, from the very beginning, have to do with

[1092] A four-page manuscript, entitled 'The Nuclear Constitution and Neutron Captures,' was included in Volume 9 of *Niels Bohr: Collected Works* (1986, pp. 145–147). Bohr travelled to England in February 1936 and gave lectures on this subject at the University College, London, on 11 February, and in Cambridge during the week from 11 February onward (see Peierls, 1986, p. 21).

the essential collective aspects of the interplay between the constituent particles.' Notably, a 'striking difference in the level schemes for low and high excitations of heavy nuclei' must be concluded, that is:

> In contrast to the usual view, where the excitation is attributed to an elevated quantum state of an individual particle in the nucleus, we must in fact assume that the excitation will correspond to some quantized collective type of motion of all the nuclear particles. On account of the rapid increase of the possibilities of combination of the proper frequencies of such motions for increasing values of the total energy of the nucleus, we should therefore expect that the distance between neighbouring levels would become very much smaller for the high excitation concerned in neutron collisions than in the ordinary γ-ray levels where we probably have to do with the states of collective motions of the most simple type. (Bohr, *loc. cit.*, p. 346)

After discussing the γ-ray emission processes, Bohr turned to the collisions of small-energy neutrons, especially the 'irregularities' or transmutations as observed by Fermi and his collaborators. Here, Bohr said, the large de Broglie wavelengths of the neutron as compared with the nuclear dimensions would contradict 'the simple ideas of paths and collisions' applied so far in physics; on the other hand, the 'compound-system' picture derived for the high-energy collisions offered the necessary explanation also for the low-energy neutron collisions (Bohr, *loc. cit.*, p. 346). Bohr then discussed the selective 'resonance capture' of neutrons observed shortly before as well as other collision processes, before he concluded:

> Even if we could experiment with neutrons and protons of energies of more than a hundred million volts, we should still expect that the excess energy of such particles, when they penetrate into a nucleus of not too small mass, would in first place be divided among the nuclear particles with the result that a liberation of any of these would necessitate a subsequent energy concentration. (Bohr, *loc. cit.*, p. 348)

Consequently, 'in general not one but several charged or uncharged particles will eventually leave the nucleus as a result of the encounter'; and, for particles impinging with energies above 1,000 million electron volts, 'we must even be prepared for the collisions to lead to an explosion of the whole nucleus' (Bohr, *loc. cit.*). At the end of his note, Bohr emphasized that this new 'comprehensive interpretation of characteristic properties of nuclei' allowed 'a division of nuclear reactions into well separated stages to an extent which has no simple parallel in the mechanical behaviour of atoms' (Bohr, *loc. cit.*, p. 348).

This nontechnical outline thus provided a two-type description of nuclear phenomena—i.e., the normal reactions and those involving the 'compound system' picture—which Bohr propagated first by letters to theoretical-physicist friends, and it received great attention. The contents of Bohr's article in *Nature* were discussed quite favourably and in detail, e.g., in George Gamow's book (Gamow, 1937, preface and Chapter XI), and in the following publications of Hans Bethe (1936, 1937). Bohr himself presented his new views in many lectures, beginning on

11 February 1936, in London, then in Cambridge (see the report on 'Neutron Capture and Nuclear Constitution' in *Nature* **137**, issue of 29 February 1936, p. 351) and in Helsinki on 12 August 1936 (see Bohr, 1936b); he continued to do so in spring of the following year in the United States (Bohr, 1937b) and Europe (Bohr, 1937c). In particular, he tried to illustrate the concept and the reactions of the atomic nucleus as a compound system by means of a simple mechanical model consisting of a shallow basin containing a number of billiard balls as follows:

> If the basin was empty, then upon striking a ball from the outside, it would go down on a slope and pass out on the opposite side with its original velocity. But with other balls in the basin, these two would similarly share their energies with others, and so [on] until the original kinetic energy was divided among all the balls. If the basin and the balls are regarded as perfectly smooth and elastic, the collisions would continue until the kinetic energy happens to be again concentrated upon a ball close to the edge. This ball would escape from the basin and the remainder of the balls would be left with insufficient total energy for any of them to climb the slope. (See report in *Nature* **137**, 1936, p. 351)

After other authors had taken up the compound-system description of the nucleus and treated it as a thermodynamical system, with the excitation 'considered as a heating up, due to an elementary absorption process . . . while the initial temperature was equal to zero' (Frenkel, 1936b, pp. 533–534), and drew quantitative consequences on the 'Statistics of Nuclear Reactions' (Weisskopf, 1937), Bohr incorporated their results—again in a pictorial way—into his later lectures. Thus, he explained:

> To begin with, the original nucleus is in its normal state and at temperature zero [Fig. 3 (1)]. After the nucleus has been struck by a neutron with about 10 million volts energy, a compound nucleus is formed with 18 million volts energy, and the temperature is raised from zero to roughly one million volts. The irregular contour of the nucleus symbolizes the oscillations in shape corresponding to the different vibrations excited at the temperature in question [Fig. 3 (2)]. The next figure [Fig. 3 (3)] shows how a neutron escapes from the system and the excitation, and accordingly the temperature is somewhat lowered. In the last stage of the process the remaining part of the energy is emitted in the form of electromagnetic radiation and the temperature drops down to zero. (Bohr, 1937b, p. 163)

It should be mentioned here that Bohr also considered the work of Breit and Wigner on the nuclear resonances (1936), and a subsequent one by Hans Bethe and George Placzek (1937), in order to provide essential confirmation of the views entering into the compound-nucleus model (Bohr, 1937b, p. 163, footnote 3).

In a letter to Heisenberg, Bohr announced that 'the details concerning nuclear reactions and the help which the new understanding provides compared with the earlier one, will be discussed in a more complete paper on which I have been working at the same time with Kalckar' (Bohr to Heisenberg, 8 February 1936). However, it took another year and a half until the paper, entitled 'On the Transmutation of Atomic Nuclei by Impact of Material Particles' by Niels Bohr and

Fritz Kalckar, would be submitted to the *Mathematisk-fysiske Meddelelser* of the Royal Danish Academy of Sciences (Bohr and Kalckar, 1937). This final paper also bore the subtitle, 'I. General Theoretical Remarks,' and contained seven sections, namely: '§1. Basic Ideas; §2. Nuclear Level Distribution; §3. Radioactive Properties of Nuclei; §4. Escape of Neutrons from Excited Nuclei; §5. Slow Neutron Collisions; §6. Release of Charged Particles from Nuclei; §7. Collision between Charged Particles and Nuclei.' (Bohr and Kalckar, *loc. cit.*, p. 3)[1093] Bohr and Kalckar here presented the ideas of Bohr in some detail without, however, going too much into the depth of the theoretical formalism. Thus, in §2, they outlined the arguments, allowing 'a simple comparison between many properties of nuclear matter and the properties of ordinary liquid and solid substances' (Bohr and Kalckar, *loc. cit.*, p. 8) by considering the quantum-theoretical behaviour of N nuclear particles in a volume $N\delta^3$, where $\delta = 3 \times 10^{13}$ cm, having an average kinetic energy $K(= h^2/8\delta^2\mu)$, with μ being the mass of the proton or neutron. An elasticity ε and a surface tension ω could be assigned to this system, and then the corresponding oscillations with frequencies ν_ε and ν_ω could be calculated: The difference of these frequencies was found to vary faster than $N^{-1/3}$ and $N^{-1/2}$, respectively. 'Of course,' as Bohr and Kalckar pointed out, 'more detailed considerations regarding the specific character of the interaction between the individual nuclear particles on the stability as well as the excitation mechanism of nuclei are needed' (Bohr and Kalckar, *loc. cit.*, pp. 11–12). In particular, the Pauli principle had to be invoked, but they also emphasized that 'any attempt of accounting for the spin values by attributing orbital momenta to individual particles seems quite unjustifiable,' because 'any orbital momentum is shared by all constituent particles of the nucleus in a way which resembles that of the rotation of a solid body' (Bohr and Kalckar, *loc. cit.*, p. 12). Hence, if I denoted the moment of inertia, the energy differences between the lowest quantum states were estimated to be

$$\Delta_r E = \frac{h^2}{8\pi I} \approx N^{-5/8} K. \tag{782}$$

In a similar qualitative manner, Bohr and Kalckar derived the radiative properties, especially the probability for radiative transitions and the escape of neutrons from excited nuclei, obtaining for the latter

$$\Gamma_n = N^{2/3} \tau^{-1} \exp\left(-\frac{W}{kT}\right), \tag{783}$$

[1093] In the introduction, Bohr and Kalckar said that they envisaged a three-part paper, with 'the second part planned as a more detailed elaboration of the theory of nuclear collisions on the general lines discussed here while the third part should contain an analysis on such lines of the available experimental evidence about nuclear transformations' (Bohr and Kalckar, 1937, p. 1). However, the interruption of the investigations of Bohr and Kalckar after the completion of Part I in January 1937 owing to Bohr's visit to several American universities in the following months, and the appearance of Hans Bethe's Part B of his major review of the current knowledge of nuclear physics in *Review of Modern Physics* (Bethe, 1937) covering the same material, made them abandon their plan. Thus, Bohr and Kalckar just added in Part I several references to Bethe's paper and the related work published in the course of 1937 before they submitted their paper for publication in November of that year.

where $\tau = \mu\delta^2/h$ denoted the characteristic nuclear time of about 10^{-22} sec, W denoted the work function of the neutron, and T denoted the effective temperature of the excited nucleus. Regarding the collision of nuclei, they referred to the results of Breit and Wigner, Eqs. (781a) and (781b), for the capture and scattering of cross sections; concerning the release of charged particles, they assumed a Gamow-like formula; i.e.,

$$\Gamma_\alpha \approx \tau^{-1} \exp\left(-\frac{4\pi}{h} \int_a^b \sqrt{2m[P(r) - E]}\, dr \right). \tag{784}$$

Moreover, they discussed on a qualitative level the collision between charged particles and nuclei, and—in an 'addendum'—drew critical attention to some more detailed suggestions by their colleagues, notably, Bethe, Landau, Kalckar, Oppenheimer and Serber, and Weisskopf. '[Lev] Landau [1937c] has succeeded ... from very general arguments in deducing a comprehensive formula for the dependence of the probability of nuclear disintegration under release of charged particles on the external repulsion as well as on the density of the level distribution of the nucleus in the energy region concerned,' Bohr and Kalckar noted, and concluded their extensive essay by announcing: 'The closer connection between Landau's treatment and the argumentation given in the text will be discussed in a forthcoming paper by Kalckar.' (Bohr and Kalckar, *loc. cit.*, p. 40)[1094]

When Niels Bohr visited the United States in 1937, he attended the 'Third Conference on Theoretical Physics,' organized by George Gamow, Edward Teller, and Merle Tuve, beginning on 15 February in Washington, D.C. Most of the American experts—from Gregory Breit to L. H. Thomas—including many immigrants, like Hans Bethe and Eugene Wigner—participated in this meeting. Afterward, Bohr travelled with Bethe to attend the meeting of the American Physical Society, which was held on 19 and 20 February 1937, at Duke University in Durham and at the University of North Carolina in Chapel Hill. At Chapel Hill, he met John Archibald Wheeler who taught there.[1095] Stimulated by dis-

[1094] The work of Fritz Kalckar suddenly came to an end, when the not yet quite 28-year-old young man died on 6 January 1938. Bohr continued to investigate his compound-nucleus model further in 1938, now keeping closer contact with Rudolf Peierls and George Placzek—but also with Wolfgang Pauli and Werner Heisenberg who criticized but also applied the theory (see, e.g., Bagge, 1938). For details, we refer to Peierls, 1986, pp. 43–52.

[1095] John Archibald Wheeler, born on 9 July 1911, in Jacksonville, Florida, was educated at Baltimore City College and Johns Hopkins University, receiving his Ph.D. in 1933. As a National Council Research Fellow, he spent the academic year 1934–1935 in Copenhagen, and then he went to the University of North Carolina before joining the faculty of Princeton University in 1938, where he was appointed Joseph Henry Professor (1966–1976). Upon retirement from Princeton, he was invited to join the University of Texas at Austin. Wheeler served as a physics consultant on atomic energy projects from 1939 to 1945, and at Los Alamos (1950–1952) and Princeton (1951–1953), he directed projects connected with thermonuclear weapons. From 1969 to 1976, he was a member of the U. S. General Advisory Committee on Arms Control and Disarmament. [For more biographical information on Wheeler, see Klauder, 1972, especially, pp. 1–14; see also John A. Wheeler (with Kenneth Ford), *Geons, Black Holes & Quantum Foam: A Life in Physics* (W. W. Norton & Co., Inc., New York, 1998).]

cussions with Bohr—he retained brief notes of Bohr's two lectures from that time (see Peierls, 1986, p. 39)—Wheeler became very interested in the liquid-drop model of the nucleus.[1096] Actually, at the North Carolina APS meeting in February, he developed a concept, which could be connected with Bohr's compound nucleus, the 'Resonating Group Structure of the Nucleus' (Wheeler, 1937a). Wheeler defined this idea in the following way:

> By regarding the neutrons and protons in a given nucleus, ^{7}Li for example, as resonating between different possible configurations, such as ^{4}He (normal) + ^{3}H (normal), ^{5}He (excited) + ^{2}H (normal), etc., one obtains a description of nuclear structure in which by far the largest part of the energy of the compound nucleus is already accounted for by the internal binding of the separate groups. Use from the beginning of this saturation property of nuclear binding gives an improved treatment of nuclear collisions and transmutations. (Wheeler, *loc. cit.*, p. 683)

Wheeler applied his method by writing 'the wave function as a sum of properly antisymmetrical parts corresponding to the most important configurations, each part involving a different unknown function, *F*, of the inter-group separations,' and then used a variational principle to determine finally the energy levels, scattering phase shifts, and transmutation probabilities of the nuclei with the condition of a vanishing Fredholm determinant. Wheeler noted that 'this method gives very satisfactory results when applied to the interaction between two alpha-particles and is being employed in the treatment of other collision problems.' (Wheeler, *loc. cit.*) Six months later, he submitted a detailed paper on 'Molecular Viewpoints in Nuclear Structure' (Wheeler, 1937b). In it, he wished—as he declared in the introduction—to add to the 'concepts native to atomic structure' also 'some points of view more closely related to molecular structure,' especially:

> If atomic structure be characterized by a central force dominating the motion of almost independent particles, and nuclear constitution, by those collective types of motion which Bohr and Kalckar [in the not yet published paper of 1937] liken to modes of vibration and rotation of a liquid droplet, then the feature which distinguishes molecular structure from these is its division into more or less well defined groups, between which it is a good approximation to say that inter-"atomic" forces act. It is the usefulness, and limitation, of this concept of group structure that we wish to study in connection with the mechanical description of the atomic nucleus. (Wheeler, *loc. cit.*, p. 1083)

In his paper, Wheeler first examined how the mathematical procedure worked in the case of the H_2^+-molecule and the corresponding three-body nucleus by establishing a connection between the associated wave functions (as obtained from his variational principle). In the molecular situation, the wave function represented a state in which the system resonated between the groupings of atom-ion and ion-

[1096] Wheeler had worked on the production of pairs in Copenhagen and began to study nuclear forces in Chapel Hill (see Wheeler, 1936).

atom; in the nuclear situation, the resonance occurred between the deuteron–neutron state and a state of the nucleus ^{3}H. 'Group theory gives information as to which groupings are the most important in describing a particular state of a nucleus,' Wheeler noted and added:

> The interchange of neutrons between the groups is rapid. It is largely responsible for the intergrouping forces, but also prevents one from attributing any well-defined individuality to the groups except as follows: If the time required for a particle to diffuse between two parts of the nucleus vibrating in opposite phase (in the language of the liquid droplet model) is large in comparison with the period of vibrations, then the particles of the nucleus may be divided into groups which preserve their identity long enough to make possible a simple description of the nuclear motions in terms of the relative displacements of these clusters. (Wheeler, *loc. cit.*)

Wheeler outlined in greater detail his peculiar mathematical formalism in another paper 'On the Mathematical Description of Light Nuclei by the Method of Resonating Group Structure,' which was submitted and published simultaneously with the previous one (Wheeler, 1937c). In particular, Wheeler introduced in it what he called the 'scattering matrix,' i.e., a unitary matrix of coefficients connecting 'the asymptotic behavior of an arbitrary particular solution [of the integral equations] with that of solutions of a standard form'; hence, 'the Fredholm determinant also determines [besides the stable energy values of the nuclear system] all the scattering and disintegration cross sections' (Wheeler, *loc. cit.*, p. 1107).[1097] Wheeler did not pursue this method further, but in the following year, he joined Edward Teller in drawing certain consequences from a study of the rotations in Bohr's droplet model (Teller and Wheeler, 1938b). In particular, they arrived at the conclusion: 'The absence of low lying levels in heavy nuclei definitely indicates ... that if nuclei are to be compared with a phase of matter in macroscopic experiments the correct analog to use is not a crystallite but a droplet of a "quantum liquid" such as the low temperature modification of liquid helium' (Teller and Wheeler, *loc. cit.*, p. 789).

Teller and Wheeler first presented their result at the New York meeting of the American Physical Society (Teller and Wheeler, 1938a). At the same meeting, Katherine Way, Wheeler's first graduate student at Chapel Hill, talked about 'Nuclear Quadrupole and Magnetic Moments'; in the abstract of her talk, she stated:

> Observations of nuclear quadrupole moments, q, reveal a lack of spherical symmetry in the distribution of positive charge, the more frequently found positive q indicating an elongated and the less common negative q a flattened shape. Nuclear models which have been proposed may be classified under (1) single particle model, (2) liquid drop model, and (3) central core plus single particle model. (Way, 1938, p. 685)

[1097] This formulation of nuclear problems contained the germ of a later successful treatment of elementary particles, inaugurated by Werner Heisenberg in 1942 (see the Epilogue). Wheeler, however, would not become involved in this development.

Katherine Way then added that model (1) would account for the magnetic moments of light nuclei, though it could not make predictions about the value of q, while model (2) yielded the right order of magnitude for magnetic moments, however, far too small q; finally, model (3) appeared to be most promising in explaining both properties of nuclei. In the detailed paper, which she submitted in March 1939, Miss Way analyzed in greater detail the liquid-drop model (2). In particular, she considered a uniformly charged spinning drop which, 'if assigned classical properties, will have a magnetic moment on account of the rotation of the charges, and a quadrupole moment if any bulging is caused by spinning' (Way, 1939, p. 964). In carrying out the calculations, she found reasonable values for the magnetic moments of the nuclei (roughly between 1 and 5), but the nuclear spin quantum numbers derived from the rotational energies (according to Teller and Wheeler, 1938b, p. 786) came out to be an order of magnitude higher. Furthermore, by calculating the quadrupole moments from the equilibrium shape of the charged spinning drop (which was approximately assumed to be an ellipsoid of revolution), she concluded: 'From the signs of the different terms it is clear that for any values [of the parameters of the liquid drop], a positive q, or a cigar-like shape, will never be obtained from the drop model if the drop has any angular momentum.... Although the cigar-like shape is stable against distortions into other ellipsoids, it has not been shown whether it is stable against arbitrary distortions.' (Way, 1939, p. 965) This was only a weak formulation of a difficulty, which Wheeler expressed later much more definitely:

> One day [Katherine Way] came in and reported a difficulty. The equations gave no solution in the case of a sufficiently highly charged nucleus turning at a sufficiently great angular velocity. It was clear that one had to do in this case with a kind of instability. It took only 1939 and the discovery of Hahn and Strassmann to recognize the nature of the instability: nuclear fission. Why did we not go to the analysis of the higher order terms in the deformation energy and predict fission in advance of its discovery? It was not any difficulty in mathematics. It was a difficulty in the model. It failed to give the right magnitudes and the right trends for nuclear magnetic moments. (Wheeler, 1979, p. 266)

The serious theoretical considerations on nuclear reactions stimulated also in 1937 a renewed discussion of an old problem: the transmutation of chemical elements and the energy sources of stars. Carl Friedrich von Weizsäcker, since fall 1936 an assistant at the new *Kaiser Wilhelm-Institut für Physik* in Berlin-Dahlem, signed on 22 January 1937, a review article entitled '*Über Elementumwandlung im Innern der Sterne* (On the Transmutation of Elements in Stars)' (von Weizsäcker, 1937b). He started from what he called the '*Aufbauhypothese*' ('hypothesis of constitution' or 'building-up'), which he attributed to Arthur Stanley Eddington (1926) and stated 'that, apart from negligibly small effects, the nuclear transmutations studied theoretically provide the sole origin of the composite inner regions of the stars which initially are composed of pure hydrogen and their unique source of energy' (von Weizsäcker, 1937b, p. 176).[1098] The empirical data obtained on the chemical constitution of stars, especially the frequency of the occurrence of heavier elements, encouraged von Weizsäcker to extend the '*Aufbauhypothese*' as

> The temperature in the interior of stars takes on values allowing the transmutation of the lightest nuclei on the basis of hydrogen. The energy obtained from these transmutations provides the source of the emission of radiation due to the [surface] temperature of the star. The transformations do not directly lead to the origin of heavier nuclei; however, in their course free neutrons are created as byproducts, which build-up a part of the available [lighter] nuclei into the known heavier elements. (von Weizsäcker, *loc. cit.*, p. 178)

He added: 'If this concept is correct, the star acts as an engine which, with the help of the liberated nuclear energies, sustains the necessary external conditions steadily,' and: 'It should be the only possible engine of this kind.' (von Weizsäcker, *loc. cit.*) With this fundamental hypothesis in mind, von Weizsäcker pursued a systematic survey of the various energy-producing reactions of the lightest nuclei and their relevance for the internal constitution of the stars, and then the formation of the heavy nuclei by reactions with neutrons.

In dealing with the nuclear reactions in stars, von Weizsäcker confined himself to consider only thermally induced reactions, initially with charged light particles—namely, proton, deuteron, and triton—having essentially a Maxwell–Boltzmann distribution, such as

$$ {}^{7}_{3}\mathrm{Li} + {}^{1}_{1}\mathrm{H} = {}^{8}_{4}\mathrm{Be} \quad \text{or} \quad {}^{7}_{3}\mathrm{Li} + {}^{1}_{1}\mathrm{H} = 2\,{}^{4}_{2}\mathrm{H}, \tag{785} $$

the reaction on the left providing a formation and the one on the right a destruction process. These processes could be described by the liquid-drop model of Gamow and Bohr, with cross sections depending crucially on the available temperature. For the continuous production of energy the existence of 'reaction chains' was necessary, which not only produced enough energy and higher elements (such as helium from hydrogen) but also supplied the material for the initial reaction. von Weizsäcker discussed a number of possibilities for such reactions and the resulting relative frequencies of higher nuclei in stars, and concluded:

> 1. The temperatures in the interior of stars suffice for starting nuclear reactions, in which from hydrogen higher elements can build up....
> 2. All appreciably frequent reactions begin with hydrogen having thermal energy....
> 3. The exact course of reactions depends on the unknown properties of nuclei having the mass 5 [such as ${}^{5}_{2}\mathrm{Li}$ and ${}^{5}_{2}\mathrm{He}$] ...
> 4. Because of the Gamow factor the reactions occur only in the vicinity of the centre of the star....

[1098] The pioneers in connecting Eddington's original building-up hypothesis with nuclear theory were Fritz Houtermans and R. d'E. Atkinson (1929)—see Section III.7.

5. The neutron production suffices for the build-up of the heavier elements, if their total number is about 100 times below that of helium....

...

7. The state of a star should be determined by its hydrogen content. The stability *versus* pulsation rests on the delay of energy production by intermediate β-decays.

(von Weizsäcker, *loc. cit.*, pp. 190–191)

In spite of these initial successes, the generalized '*Aufbauhypothese* (building-up hypothesis)' revealed serious difficulties, as von Weizsäcker summarized in Part One of his second paper '*Über Elementumwandlung im Innern der Sterne. II* (On the Transmutation of Elements in Stars. II),' submitted one-and-a-half years later (von Weizsäcker, 1938b). Notably, von Weizsäcker said that the coupling of the energy-producing processes with those of the building-up of higher elements seemed to be contradicted by empirical data, and the availability of larger numbers of neutrons (required for the synthesis of heavier nuclei) also could not really be justified theoretically. Hence, especially the origin of all heavy nuclei should not be included in the building-up hypothesis.

Having stated this restriction of the fundamental hypothesis, von Weizsäcker returned in Part Two of his 1938 survey to consider the possible mechanisms of nuclear energy production in stars. Now, he also abandoned his previously preferred model cycle which implied the hypothetical mass-5 nucleus (which had been found in the meanwhile to be an unstable object). Besides a process having the net result $\mathrm{H} + \mathrm{H} = \mathrm{D} + \mathrm{e}^{+}$ and those processes leading to the formation of helium via lithium, as well as several other processes, he focused on a process connected with the carbon nucleus $^{12}\mathrm{C}$, the so-called 'carbon cycle'; i.e.,

$$\begin{gathered} ^{12}\mathrm{C} + \mathrm{H} = {}^{13}\mathrm{N}; \quad {}^{13}\mathrm{N} = {}^{13}\mathrm{C} + \mathrm{e}^{+}; \quad {}^{13}\mathrm{C} + \mathrm{H} = {}^{14}\mathrm{N}; \\ ^{14}\mathrm{N} + \mathrm{H} = {}^{15}\mathrm{O}; \quad {}^{15}\mathrm{O} = {}^{15}\mathrm{N} + \mathrm{e}^{+}; \quad {}^{15}\mathrm{N} + \mathrm{H} = {}^{12}\mathrm{C} + {}^{4}\mathrm{He}. \end{gathered} \tag{786}$$

Von Weizsäcker finally noted that 'the energy source of stars would therefore first consist of a decomposition of elements below carbon, followed by the cycle [(786)], and added: 'If via side reactions the abundance of carbon finally decreases as well, an analogous cycle starting from oxygen would be available.' (von Weizsäcker, *loc. cit.*, p. 639) With the carbon cycle, the known features of stellar evolution did indeed follow, while the origin of the heaviest elements certainly had to be decoupled; it required extreme conditions which von Weizsäcker thought might exist in supermassive stars.

In the discussion of stellar evolution, von Weizsäcker referred to investigations of George Gamow, in particular to the papers submitted between December 1937 and August 1938.[1099] The first one, on 'Nuclear Energy Sources and Stellar Evolution' (Gamow, 1938a), began with a modern reformulation of the theory of

[1099] According to the footnotes and acknowledgement, Gamow supplied von Weizsäcker with hints on not yet published results of his own and those of others. (See von Weizsäcker, 1938b, p. 639 and p. 646.)

stellar constitution by Arthur Eddington (1926). Gamow then analyzed, just as von Weizsäcker had done (whose paper of 1937 he cited), the thermal reactions of the lightest nuclei (helium, lithium, and beryllium), arriving at a reasonable description of the known facts from stellar observations. In his considerations, Gamow also included a shell structure of stars (as demanded earlier by Edward Milne and Subrahmanyan Chandrasekhar—see Section IV.4) obtained when an energy production process ceased to operate in the centre of the star. With such ideas, he investigated more closely with Edward Teller a specific problem involved, namely 'The Rate of Selective Thermonuclear Reactions,' especially the dissociation of the ^{8}Be nucleus (Gamow and Teller, 1938a). Later on, Gamow developed a 'Tentative Theory of Novae' (Gamow, 1938b).

In March 1938, Gamow and Teller organized a conference on stellar energy—as one of the regular annual meetings sponsored jointly by the George Washington University and the Department of Terrestrial Magnetism of the Carnegie Institution (see the previous one on nuclear theory referred to above—bringing together astrophysicists (including Bengt Strömgren) and nuclear theorists, including Hans Bethe from Cornell University. Bethe recalled about this event as follows: 'The conference was concerned with energy production in stars. The direct combination of two protons [Bethe and Critchfield, 1938] had been established as a satisfactory energy source for the sun, but could not explain the enormous increase of luminosity with increasing mass of the star. It was therefore necessary to find nuclear reactions depending more strongly on temperature H + H, pointing to the involvement of heavier nuclei than H. At the same time, it was known that the abundance of heavier nuclei is much smaller than that of hydrogen. The CN cycle fits both requirements: 'It involves the heavier nuclei, C and N, but they are regenerated in the cycle, only the abundant H is consumed.' (Bethe, 1997, p. 355) Bethe arrived at this conclusion as a result of the Washington meeting, which aroused his interest in the problem of the energy generation in stars. Like von Weizsäcker and Gamow, he studied the individual reactions between light nuclei as a source of stellar energy. He further recalled:

> One of the participants [at the conference] was von Weizsäcker, and he reported on some of the attempts which he was making to explain energy production. Nobody at the conference had any question but that the energy production must be due to nuclear reactions. This of course was very different from the original ideas of Eddington several years earlier—ten years earlier or so. Eddington thought to use annihilation of matter to produce the energy. But nuclear reactions were well established by this time and gave a good, large amount of energy, and anybody could calculate for himself that nuclear reactions with abundant elements were sufficient to keep the sun shining from the past life of the universe and of course many billions of years thereafter. So this was more or less unwritten common background. (Bethe, Interview with Charles Weiner and Jagdish Mehra, 27–28 October 1966, p. 49)[1099a]

[1099a] Bethe's recollection about von Weizsäcker's participation in the Washington meeting cannot be substantiated. The same is true with Wolfgang Pauli, who is shown on a photo attributed to the 1938 meeting and reproduced in Wali's *Chandra* (1991, No. 18 after p. 182). Von Weizsäcker clearly got information about the meeting only via Gamow (1938a)—see previous footnote.

Bethe also recalled that von Weizsäcker 'in particular had tried to do two things at the same time—namely, to build up the elements and simultaneously generate energy.' This information was contained in the latter's first paper (von Weizsäcker, 1937b) available since a year and probably discussed at the Washington meeting. 'The main step which I took was to get away from the coupling between building up the elements and generating energy. But that of course came later.'[1099b] Bethe continued:

> Another point which excited everybody at the time was that apparently even without knowing the source of energy, there were internal discrepancies in the calculations of the astrophysicists—namely, in one way they calculated the central temperature of the sun as some 40 million volts and in another way they calculated less than 20 million volts. The mistake which they were making at the time, which was soon afterwards corrected, was that they assumed that most of the material of the sun and other stars was of the same composition as the earth—namely, mostly heavy elements, heavy starting from carbon and going up through iron. And if you assumed that, then you get this discrepancy. Later on they discovered ... that the main constituent is really hydrogen, and with hydrogen, the two determinations of central temperature came into agreement.... Before the summer I [learned] that it was all right to assume hydrogen as a major constituent—if not the main constituent. (Bethe, *loc. cit.*, p. 50)

Soon after the Washington conference, Hans Bethe and C. A. Critchfield collaborated on a paper on 'The Formation of Deuterons by Proton Combination,' in which

> The probability of the astrophysically important reaction $\mathrm{H} + \mathrm{H} = \mathrm{D} + \mathrm{e}^+$ is calculated. For the probability of positron emission, Fermi's theory is used. The penetration of the protons through their mutual potential barrier, and the transition probability of the deuteron state can be calculated exactly, using the known interaction between two protons. The energy evolution due to the reaction is about 2 ergs per gram per second under the conditions prevailing at the center of the sun (density 80, hydrogen content 35 percent by weight, temperature 2×10^7 degrees). This is almost but not quite sufficient to explain the observed average energy evolution of the sun (2 ergs/g sec) because only a small part of the sun has high temperatures and density. The reaction rate depends on the temperature approximately as $T^{3.5}$ for the temperature around 2×10^7 degrees. (Bethe and Critchfield, 1938, p. 248)

The Bethe–Critchfield paper was received by the *Physical Review* on 23 June 1938, and published in its issue of 15 August 1938. They noted that 'the most primitive is the combination of two protons to form a deuteron, with positron emission—$\mathrm{H} + \mathrm{H} = \mathrm{D} + \mathrm{e}^+$—... this reaction must stand in the beginning of any building

[1099b] As we have mentioned, von Weizsäcker dropped the connection between building up elements and energy production in stars in his second paper, submitted in July and published in September 1938 (1938b), where he especially arrived at two conclusions: 1. 'The assumption that all known chemical elements have been built up in the presently existing stars and still will be, is given up.' (von Weizsäcker, *loc. cit.*, p. 645) 2. Concerning the energy producing reaction in stars: 'Most probable is a cycle involving carbon as catalyzer for forming helium.' (von Weizsäcker, *loc. cit.*, p. 646)

up of chemical elements; it has already been discussed in this connection by von Weizsäcker [1937b].' (Bethe and Critchfield, *loc. cit.*, p. 248) Bethe recalled:

> The proton-proton reaction was not my idea and not Critchfield's idea. The two of us wrote the paper and got credit for it, but I think it was really Gamow's idea to look at that. Critchfield was Gamow's student, a Ph.D. student, and I think this paper was probably Critchfield's thesis. So Gamow suggested to Critchfield this topic. Critchfield did it, but he didn't feel certain about his methods and so he sent me the paper for criticism and correction. And I found a few factors of two which I would change one way or other, and I used perhaps a somewhat more powerful way to calculate the wave function of the deuteron. I made minor additions to the paper. I was very much interested in this paper of course in connection with the conference, but I don't remember whether the paper was sent to me before or after the meeting [in Washington]. I have a vague feeling that it was before the meeting.
>
> Now this feeling gets a little stronger because I have the impression that when I went home to think about the conference, I had clearly in mind the proton-proton reaction, and we had been told about the properties of stars so that I could figure out that this reaction was insufficient to give energy for the hotter stars—in particular Sirius A and Cygni Y or something like that. (Bethe, Interview with Charles Weiner and Jagdish Mehra, 27–28 October 1966, p. 51)

Bethe continued:

> Now, I had at my disposal—and this has never been sufficiently credited—a paper by Gamow and Teller on the rate of thermonuclear reactions. This came out I think early in '38. And using this and my knowledge of nuclear reactions from the three [comprehensive review papers which he had written for the *Reviews of Modern Physics*], I set out to discuss the various nuclear reactions that could occur between protons and other nuclei, and I would have gone on—and did go on, in fact—to discussing reactions between alpha-particles. So doing this, I very soon was able to rule out reactions between protons and helium because they don't give any product; the reactions between protons and lithium, beryllium, boron, because they immediately consume these elements, and there isn't very much of any of these elements and [one] could easily show that the energy would last only a few hundred thousand years if these were the elements responsible. And having come to that point, I was almost sure that nothing would work but the proton-proton reaction. But then I looked at the next, which was carbon, and that did give the right order of energy production; and looking at it for about a day, I recognized that there was a cycle which returned to carbon. This must have been sometime during May 1938.... After a few more days I found that this reaction in contrast to the proton-proton [reaction] gave a tremendous dependence on temperature, about the 17th power of the temperature, which was sufficient to explain the difference in energy production between the sun and Cygni, with a very modest ratio of temperatures, which the astrophysicists would allow me. Moreover, somehow I must have known already then, which was in May, that hydrogen was a major constituent, perhaps *the* major constituent; and this process used up only hydrogen, and that was fine and would mean that the fuel supply would last billions of years. The whole work took me about a month from the [Washington] conference. (Bethe, Interview with Charles Weiner and Jagdish Mehra, 27–28 October 1966, p. 52)

In his major paper on 'Energy Production in Stars,' which was received by the *Physical Review* on 7 September 1938, Bethe showed in twelve sections that:

> *the most important source of energy in ordinary stars is the reactions of carbon and nitrogen with protons*. These reactions form a cycle in which the original nucleus is reproduced, *viz*. $C^{12} + H = N^{13}$, $N^{13} = C^{12} + e^+$, $C^{13} + H = N^{14}$, $N^{13} + H = O^{15}$, $O^{15} = N^{13} + e^+$, $N^{13} + H = C^{12} + He^4$. Thus carbon and nitrogen merely serve as catalysts for the combination of four protons (and two electrons) into an α-particle.
>
> The carbon-nitrogen reactions are unique in their cyclical character. For any nuclei lighter than carbon, reaction with protons will lead to the emission of an α-particle so that the original nucleus is permanently destroyed. For all nuclei heavier than fluorine, only radiative capture of the protons occurs, also destroying the original nucleus. Oxygen and fluorine reactions mostly lead back to nitrogen. Besides, these heavier nuclei react much more slowly than C and N and are therefore unimportant for the energy production.
>
> The agreement of the carbon-nitrogen reactions with observational data is excellent. In order to give the correct energy evolution in the sun, the central temperature of the sun would have to be 18.5 million degrees while integration of the Eddington equations gives 19. For the brilliant star Y Cygni the corresponding figures are 30 and 32. This good agreement holds for all bright stars of the main sequence.
>
> For fainter stars, with lower central temperatures, the reaction $H + H = D + e^+$ and the reactions following it, are believed to be responsible for the energy production.
>
> It is shown that *no elements heavier than* He^4 *can be built up in ordinary stars*. This is due to the fact, mentioned above, that all elements up to boron are disintegrated by proton bombardment (α-emission) rather than built up (by radiative capture). The instability of Be^8 reduces the formation of heavier elements still further. The production of neutrons in stars is likewise negligible. The heavier elements found in stars must therefore have existed already when the star was formed.
>
> Finally, the suggested mechanism of energy production is used to draw conclusions about astrophysical problems such as the mass-luminosity relation, the stability against temperature changes, and stellar evolution. (Bethe, 1939, p. 434)

After Hans Bethe sent in his paper on 'Energy Production in Stars' to *Physical Review*, he learned from his doctoral student Robert E. Marshak that the New York Academy of Sciences was offering a prize of $500 (A. Cressy Morrison Prize in 1938) for a paper on the energy production in stars, with the condition that the paper must not have been published previously. 'And $500 was a good deal of money for me at the time, so I asked the *Physical Review* to delay publication and sent in the paper for the prize, which I got, and of which I gave the finder's fee to Marshak [10%]. Then after I got the prize, I asked the *Physical Review* now to publish it; and in order to accelerate the publication I sent in the letter to the editor, which is really an abstract of the paper [quoted above] after I had sent in the paper, and that explains why there are two papers and why they were so much delayed . . . it took six months for publication.' (Bethe, Interview with Weiner and Mehra, 27–28 October 1966, p. 58)

One year after this interview, Hans Bethe was awarded the 1967 Nobel Prize for Physics for his work on the energy production in stars; it was the first time that an astrophysical subject was recognized by the Nobel Committee. In his Nobel lecture on 11 December 1967, Bethe stated the CN-cycle as follows:

$$C^{12} + H = N^{13} + \gamma, \tag{787a}$$

$$N^{13} = C^{13} + e^{+} + \nu, \tag{787b}$$

$$C^{13} + H = N^{14} + \gamma, \tag{787c}$$

$$N^{14} + H = O^{15} + \gamma, \tag{787d}$$

$$O^{15} = N^{15} + e^{+} + \nu, \tag{787e}$$

$$N^{15} + H = C^{12} + He^{4}, \tag{787f}$$

He noted:

> Reactions a, c, and d are radiative captures; the proton is captured by the nucleus and the energy emitted in the form of gamma rays; these are then quickly converted into thermal energy of the gas.... Reactions b and e are simply spontaneous beta decays, with lifetimes of 10 and 2 minutes respectively, negligible in comparison with stellar lifetimes. Reaction f is the most common type of nuclear reaction, with 2 nuclei resulting from the collision....
>
> Reaction f is in a way the most interesting because it closes the cycle: we reproduce C^{12} which we started from. In other words, it is only used as a catalyst: the result of the reaction is a combination of 4 protons and 2 electrons to form one He^4 nucleus. In this process two neutrinos are emitted, taking away about 2 MeV energy together. The rest of the energy, about 25 MeV per cycle, is released usefully to keep the sun warm. (Bethe, 1968a, p. 40)

In a later commentary on his paper on 'Energy Production in Stars' (1939), Bethe remarked:

> The CN cycle is now generally accepted by astrophysicists as the source of energy of the heavier stars in the main sequence. The nuclear reactions involved have been measured by [William A.] Fowler and his collaborators at Caltech, and the resulting energy production agrees well with that calculated in this paper. Further confirmation comes from the observation that a number of heavy stars there are clouds in which N is highly enriched relative to C and O, compared with the average in the galaxy. (Oxygen is transformed, by a slower reaction, into C and N: this is to be expected from the CN cycle since ^{14}N has a longer life than ^{12}C.) In modern computations of stellar evolution, the CN cycle is assumed as a basis. (Bethe, 1997, p. 355)

The substantial progress achieved during 1937 and 1938 in the problem of the energy production and the synthesis of heavy elements in stars contrasted with the slow advance in the specific physical questions of nuclear constitution and reactions. Still, von Weizsäcker, in another review paper published in two issues of *Naturwissenschaften* in April 1938, was able to outline some recent model-based conceptions that were applied more or less successfully to the constitution of atomic nuclei (von Weizsäcker, 1938a). For instance, he drew attention to a geometrical model of the atomic nucleus based on loosely bound α-particles in a regular lattice and a few surplus neutrons—advocated by Wilfried Wefelmeier (a member of the *Kaiser Wilhelm-Institut für Physik*—by means of which the latter had explained the frequency of nuclear isotopes (Wefelmeier, 1937a, b). Besides Ugo Fano of Rome (Fano, 1938), two Japanese visitors at Heisenberg's institute in Leipzig—Sin-itiro Tomonaga (1938) and Satosi Watanabe (1939)—developed further details of the thermodynamical model of nuclear matter. By the way, most of these publications made reference to the Bohr–Kalckar paper, which thus seemed to appear as the canonical basis of the treatment of nuclear structure and reactions, especially since it evidently accounted for most of the observed nuclear properties, not least to the phenomena found in 1934 by Enrico Fermi and his collaborators when they bombarded atomic nuclei with neutrons. On 10 December 1938, Enrico Fermi was addressed in Stockholm by the Chairman of the Nobel Committee for Physics with the words: 'Professor Fermi: The Royal Swedish Academy has awarded you the Nobel Prize for Physics for 1938 for your discovery of new radioactive substances belonging to the entire field of elements and for the discovery ... of the selective powers of slow neutrons.' (H. Pleijel, in *Nobel Lectures: Physics 1921–1940*, *Nobel Foundation*, ed., 1965, especially, p. 413) In his Nobel lecture, Fermi emphasized, among other discoveries:

> We attempted since the spring of 1934 to isolate chemically the carriers of these activities, with the result that the carriers of some of the activities of uranium are neither isotopes of uranium itself, nor of elements lighter than uranium down to the atomic number 86. We concluded that the carrier was one or more elements of atomic number larger than 92; we used to call the elements 93 and 94 at home with the names of Auseneum and Hesperium, respectively. It is known that O. Hahn and L. Meitner have investigated very carefully and extensively the decay products of irradiated uranium, and were able to trace among them elements up to the atomic number 96. (Fermi, 1939, p. 4)

Otto Hahn and Lise Meitner of the *Kaiser Wilhelm-Institut für Chemie*, whose careful investigations Fermi referred to, had begun in fall 1934 to examine the claims of the Rome group and some criticism of the work done there (raised by Aristid von Grosse and Ida Noddack).[1100] The Berlin team soon confirmed the discovery of transuranic elements (Hahn and Meitner, 1935), and indeed claimed in subsequent years, just as Fermi had stated in December 1938, the formation of

[1100] The collaboration between Otto Hahn, the director of the Institute, and Lise Meitner, the head of the Physics Department, was stimulated by the latter, who knew that in this programme the physicists and the chemists had to have a close working relationship. For details of the work of Hahn and Meitner and their collaborator Fritz Straßmann, see Fritz Krafft's biography of Straßmann (1981), especially, pp. 203–337.

several transuranic elements up to the atomic number 96 (Hahn, Meitner, and Straßmann, 1937). On the other hand, Irène Curie and Paul Savitch in Paris had obtained, also from the bombardment of uranium by slow neutrons, a substance of 3.5 h half-life, and suggested that it was a uranium isotope (Curie and Savitch, 1937). Then, Hahn, Meitner, and the young analytical chemist Fritz Straßmann, whom they had drawn into their collaboration since 1935, examined and refuted this claim; they did not publish their result but rather informed their colleagues in Paris by letter, who withdrew their conclusion in another paper submitted on 12 July 1938: Curie and Savitch now stated that their 3.5 h half-life substance exhibited chemical properties similar to lanthanum (Curie and Savitch, 1938, p. 355). Before their paper appeared in print in the September issue of *Journal de physique et le radium*, Lise Meitner had fled from Germany on 13 July 1938 (where she was endangered after the occupation of Austria, being an Austrian of Jewish descent) with the help of Otto Hahn and their Dutch colleague Dirk Coster; she arrived in Sweden via Holland and Denmark, where she found a modest position in Manne Siegbahn's physics institute.[1101] Hahn and Straßmann maintained contact with Meitner in Stockholm, while continuing their experimental programme in Berlin and checking the new claim of Curie and Savitch in Paris. Their first joint paper, submitted on 8 November 1938, analyzed the options proposed by Curie and Savitch to explain the nature of the 3.5 h half-life substance, namely:

> 1. The 3.5 h [half-life] substance has the atomic number 93, and the hitherto found transuranic elements have the atomic numbers 94–97, instead of 93–96.
> 2. The 3.5 h [half-life] substance has the atomic number 94, and the previous transuranic elements have [atomic numbers] 93, 95–97.
> 3. The 3.5 h [half-life] substance is isomeric to one of the know transuranic elements, but possesses a different arrangement of electrons; hence in spite of identical atomic number with a normal transuranic [element], elements of the properties of rare-earth show up. (Hahn and Straßmann, 1938. p. 755)

Hahn and Straßmann refuted all of these three hypotheses and, after repeating the procedures of Curie and Savitch, also obtained a '3.5 h [half-life] substance,' which they studied carefully with the following remarkable result:

> When irradiating uranium with neutrons, probably three isomeric radium isotopes result, which therefore must have originated via two successive α-transmutations via thorium.... Their half-lives are about 25 min, 100 min, and several days.... As far as the 3.5 h [half-life] substance is concerned, we believe that it represents a mixture of substances which we have found in detail (*einzeln nachgewiesen*). The properties assigned by the authors to their 3.5 h [half-life] substance are quite consistent with the properties of such a mixture. Furthermore, the authors point out in their last

[1101] A letter to *Naturwissenschaften*, dated 12 July 1938, and dealing with a new radioactive substance having a lifetime of 60 h (which they identified as another transuranic element), was the last joint publication of Hahn, Meitner, and Straßmann (1938).

publication [Curie, Savitch, and Marques da Silva, 1938] to the fact that the 3.5 h [half-life] substance obviously still contains substances having larger lifetimes, of which neither the lifetime nor the chemical properties were possible to detect. (Hahn and Straßmann, *loc. cit.*, p. 755)

Hahn and Straßmann concluded: 'As a result of the irradiation with neutrons of the single uranium type 238, therefore, *in toto* 16 different artificial types of atoms with atomic numbers 88–90 and 92–96 have been detected. (Hahn and Straßmann, *loc. cit.*, p. 756)

The chemical analysis at Berlin proceeded—with considerable exchange between Hahn and Meitner, including a meeting of both in October in Copenhagen—until a letter was sent to Stockholm containing the news:

It is now 11 p.m.; at 11:30 Straßmann wants to return, then I can go home finally. There is, however, something with the "radium isotopes" which is so strange that, for the moment, we can only tell it to you. The half-lives of the three isotopes have been determined rather accurately; they can be separated from all elements except barium; all reactions are fine [speaking in favour of radium]. Just one is not—if not very strange accidents occur, the fractionation does not work. Our radium isotopes behave like barium.... We increasingly arrive at the terrible conclusion: our radium isotopes do not behave like radium, but like barium. As mentioned, other elements, trans-uranic elements, U, Th, Ac, Pa, Pb, Bi, Po are excluded.... Perhaps you can propose some fantastic explanation. We know ourselves that it [i.e., uranium] ought not break up (*zerspalten*). Now we want to test whether the actinium isotopes [obtained] from [the decaying] "radium" may not behave like lanthanum instead of actinium. All these are very tricky experiments. (Hahn to Meitner, 19 December 1938).

Meitner immediately wrote back: 'Your radium results are very astonishing: a process working with slow neutrons which should lead us to barium,' and suggested some chemical checks, adding: 'The assumption of a far-reaching smashing (*Zerplatzen*) appears to me, at the moment, rather problematic, but we have experienced so many surprises in nuclear physics that we cannot say, it is impossible.' (Meitner to Hahn, 21 December 1938) On the same day, 21 December, Hahn dispatched the next letter to Meitner, telling her that the 'active barium' obtained previously indeed decayed into lanthanum, and added: 'We cannot suppress our results, even if they are perhaps, from the point of view of physics, absurd.' On 22 December 1938, the *Naturwissenschaften* indeed received the manuscript (of which Meitner immediately obtained a copy) '*Über den Nachweis und das Verhalten der bei der Bestrahlung des Uransmittels Neutronen entstehenden Erdalkalimetalle* (On the Proof and the Behaviour of Alkaline-Earth Metals Emerging from the Irradiation of Uranium by Neutrons).' (Hahn and Straßmann, 1939a) This extended note discussed the results obtained in December 1938, and closed with the words:

As chemists we really have to rename, on the basis of the experiments described here briefly, the above scheme, instead of Ra, Ac, Th, the symbols Ba, La, Ce. As "nuclear chemists," who, in a way are close to physicists, we cannot yet make up our

> mind to accept this jump which contradicts all the hitherto obtained experimental results in nuclear physics. A number of strange accidents might still have falsified our results. (Hahn and Straßmann, *loc. cit.*, p. 15)

Hahn and Straßmann's note appeared in the issue of 6 January 1939. Less than a month later, on 28 January 1939, they confirmed the findings of barium isotopes obtained from uranium in another note entitled '*Nachweis der Entstehung aktiver Bariumisotope aus Uran und Thorium durch Neutronenstrahlung; Nachweis weiterer aktiver Bruchstücke bei der Uranspaltung* (Proof of the Formation of Active Barium Isotopes from Uranium and Thorium by Irradiation with Neutrons; Proof of Further Active Fragments from Uranium Fission)' (Hahn and Straßmann, 1939b). At the end of their note, they acknowledged: 'That the numerous new transmutation products could be identified in a relatively short time with—as we believe—considerable accuracy, was only possible because of the experience which we have been able to gather in the earlier systematic experiments on transuranic elements and thorium transformation, carried out with Lise Meitner.' (Hahn and Straßmann, *loc. cit.*, p. 95)

The discovery of 'uranium fission' caught the physicists quite unprepared. Thus, Lise Meitner, the only person whom Hahn and Straßmann had informed before the publication of their note in the *Naturwissenschaften* on 6 January 1939, wrote to Hahn again on 28 December 1938: 'Your Ra-Ba results are exciting. Otto [Robert Frisch] and I have already racked our brains,' and three days later she wrote again:

> We have studied your paper very thoroughly and thought that *perhaps* it is energetically yet possible that such a heavy nucleus breaks up (*zerplatzt*). However, your hypothesis about the formation of Ba and Mo^{87} appears to me impossible because of diverse reasons. (Meitner to Hahn, 1 January 1939, see Krafft, 1981, p. 268)

Again, after three days, Frisch (Meitner's nephew, with whom she had discussed the matter—the Hahn–Straßmann results—during the Christmas vacation) reported from Copenhagen:

> We are very excited by your new results.... By the way, we have thought more closely about the possible (*mutmaßlichen*) physical features of the nuclear processes, and we shall perhaps write a short note. (Frisch to Hahn, 4 January 1939, in Krafft, *loc. cit.*, p. 272)

Frisch added that everybody in Copenhagen, especially Niels Bohr (who promised to keep silent for the moment), would eagerly await further results of Hahn and Straßmann; only immediately after confirmation from Berlin would he send off the announced note.

Independently, Frisch also checked the physical evidence for the 'division of heavy nuclei under neutron bombardment' by physical methods, especially in 'a uranium-lined ionization chamber' (Frisch, 1939). He thus found the ionizing particle tracks of the uranium decay-products to be associated with medium atomic weights, and confirmed 'conclusive physical evidence for the breaking up of the uranium nuclei into parts of comparable size' (Frisch, *loc. cit.*, p. 276). On the same day he signed this note as a letter to *Nature*, on 16 January 1939, he also sent to *Nature* a joint letter by Lise Meitner and himself, entitled: 'Disintegration of Uranium by Neutrons: A New Type of Nuclear Reaction' (Meitner and Frisch, 1939). 'At first sight this result [of Hahn and Straßmann, 1939a] seems very hard to grasp,' the authors stated there and explained:

> The formation of elements below uranium [from irradiation by neutrons] has been considered before, but was always rejected for physical reasons, so long as the chemical evidence was not entirely clear-cut. (Meitner and Frisch, 1939, p. 239)

Then, Meitner and Frisch continued:

> On the basis, however, of present ideas about the behaviour of heavy nuclei (Bohr, 1936a), an entirely different and essentially classical picture of the new disintegration process suggests itself. On account of their close packing and strong energy exchange, the particles in a heavy nucleus would be expected to move in a collective way which has some resemblance to the movement of a liquid drop. If the movement is made sufficiently violent by adding energy, such a drop may divide itself into two smaller drops. (Meitner and Frisch, *loc. cit.*)

Now, Meitner and Frisch had to explain, of course, why Bohr himself in 1936 or Bohr and Kalckar (1937) had not thought about the possible disintegration of the liquid-drop nucleus into two smaller drops (of almost equal size). They argued that at that time the effect of the diminishing of the surface tension due to the electric charge in heavy nuclei, leading to a 'small stability of form,' had not properly been taken into account. If this were included, two drops of nearly equal size could indeed be created by the fission of uranium, since the fragments repel each other and would come out with a kinetic energy of 200 MeV—available from the difference in the packing fraction between uranium and the elements in the middle of the periodic system. The fission process might occur as well with thorium,' they concluded—knowing ahead of publication the later result of Hahn and Straßmann (1939b).[1102]

Niels Bohr, who had failed to foresee the possibility of nuclear fission before—certainly, on account of the missing empirical evidence before December 1938—

[1102] It should be mentioned that Siegfried Flügge and Gottfried von Droste, co-workers at Hahn's institute who learned about the discovery of uranium fission only in January 1939, provided independently the same explanation of the phenomenon in a detailed paper, submitted on 22 January 1939, to *Zeitschrift für physikalische Chemie*, where it appeared in a March issue honouring the 60th birthday of Otto Hahn (Flügge and Droste, 1939). Their theoretical study contained detailed calculations of the energies of the fragments, as dependent on their constitution, and also emphasized the large neutron surplus of the resulting nuclei. From this, one may conclude that the physical interpretation of nuclear fission on the basis of the liquid-drop model of Bohr appeared to be rather obvious, once the phenomenon had been discovered experimentally.

also became active immediately: On 20 January 1939, he submitted a letter to *Nature*, which added 'a few comments on the fission process from the point of view of ideas, developed in the recent years' (Bohr, 1939b, p. 330); in particular, he noticed that 'in the case of disintegration comparable to the division of such a drop into two droplets it is evidently necessary that the quasi-thermal distribution of energy be largely converted into some special mode of vibration of the compound nucleus involving a considerable deformation of the nuclear surface' (Bohr, *loc. cit.*).[1103] Again, already the classical-mechanical treatment of a charged liquid drop did provide an understanding of the unusual disintegration of heavy nuclei, and, further, it explained 'the remarkable stability of heavy nuclei in their normal state or in the states of low excitation' (Bohr, *loc. cit.*).

Bohr concluded his letter to *Nature*, which he had sent from Princeton, New Jersey, by saying: 'The continuation of the experiments on the new type of nuclear disintegrations, and above all the closer examination of the conditions of their occurrence, should certainly yield most valuable information as regards the mechanism of nuclear excitation.' (Bohr, *loc. cit.*) Indeed, on 16 January, he had arrived with Léon Rosenfeld on the *MS Drottingholm* in New York, where John A. Wheeler recieved and took him to Princeton. There Rosenfeld (against the intentions of Bohr) immediately talked about the news from Europe on the nuclear fission, and his words quickly made the round in the United States. On 26 January 1939 Bohr and Enrico Fermi opened the 'Fifth Washington Conference on Theoretical Physics' with a discussion of the Hahn-Straßmann discovery and the interpretation by Meitner and Frisch on the basis of the droplet model. 'The whole matter was quite unexpected to all present,' wrote Richard B. Roberts, R. C. Meyer, and C. R. Hafstad of the Carnegie Institution in a letter to *Physical Review*, dated 4 February, and continued:

> We immediately undertook to look for these extremely energetic particles, and at the conclusion of the Conference on January 28 were privileged to demonstrate them to Professors Bohr and Fermi. It was subsequently learned that the particles had been observed independently by Fowler and Dodson the same day, by Dunning and coworkers at Columbia on January 25, and by Frisch in Copenhagen two weeks earlier. (Roberts, Meyer, and Hafstad, 1939, p. 416)

Indeed, several American physicists from the East and West coasts contributed to the same 15 February issue of *Physical Review*, reporting about the confirmation of nuclear fission (Fowler and Dodson, 1939; Green and Alvarez, 1939; Abelson, 1939); in the subsequent issues of the journal, the number of notes and letters on the topic increased steadily, and they were all rather quickly published.[1104] At the

[1103] For lighter nuclei, however, the deformation would not occur; hence, in agreement with the conclusions of Bohr and Kalckar (1937), only single particles were ejected.

[1104] The work on nuclear fission in the USA proceeded so fast and hectically that Bohr became worried and rushed to pressure Frisch in Copenhagen to publish his experimental results, while he tried, at the same time, to tone down a bit the American enthusiasm. (See Peierls, 1986, pp. 57–64.)

same time, Bohr further developed his own theoretical description in the case of thorium fission in a letter to *Physical Review* dated 7 February (Bohr, 1939c), and he entered into a collaboration with John Wheeler on working out the details of a general theory of fission on the basis of his droplet model. In controversial discussions with Fermi, who thought of a different approach, he upheld the opinion that the slow neutron effect was due to the rare isotope U235 (see Bohr, 1939c).[1105] Bohr and Wheeler presented a report on the 'Mechanism of Nuclear Fission' on 28 April 1939, at the Washington meeting of the American Physical Society; they emphasized, against an opinion stated earlier by Fermi, especially: 'An estimation of the energy required to separate the nuclei of thorium into two or more parts of comparable mass and charge shows conclusively that the fission process cannot be attributed to a quantum-mechanical effect analogous to alpha-particle emission from the ground state of a heavy nucleus but that we have to do with an essentially classical effect arising from the possibility of comparatively large deformations of the excited compound nucleus.' (Bohr and Wheeler, 1939a, p. 1124) Two months later, the *Physical Review* received the fully worked out paper of Bohr and Wheeler, in which they indeed presented a complete theory of nuclear fission (Bohr and Wheeler, 1939b).[1106]

The 25 pages of the Bohr–Wheeler paper were organized into six sections. After an introductory Section I, where the total energy was calculated as released in nuclear fission on the basis of the standard liquid-drop model, they turned in Section II to 'nuclear stability with respect to deformations,' considering now in contrast to Bohr and Kalckar (1937) not only the small deformation of the liquid drop, as described by

$$\delta r(\theta) = \alpha_n P_n(\cos\theta), \tag{788}$$

(with r and θ the radial and angular variables, and $P_n(\cos\theta)$ the spherical polynomial of n-th order), but also large deformations. In the first case, they rediscovered 'the characteristic oscillations of a fluid about the spherical form of stable equilibrium, even when the fluid has a uniform charge.' However, 'if the charge reaches the critical value $(10 \times \text{surface tension} \times \text{volume})^{1/2}$, the spherical form becomes unstable with respect to even infinitesimal deformations of the type $n = 2$,' Bohr and Wheeler continued and added: 'For a slightly smaller charge, a finite deformation will be required to lead to a configuration of unstable equilibrium.' (Bohr and Wheeler, 1939b, p. 430) Moreover, they justified in the same section their classical evaluation of the deformation problem and calculated for the ground state of the heavy nucleus a lifetime of about 10^{22} years. In Section III,

[1105] Thus, George Placzek wrote in a letter to Frisch, dated 2 March 1939: 'Bohr insists on 235.' (See Peierls, 1986, p. 67.)

[1106] Nearly forty years later, Wheeler described the genesis of his work with Bohr; he outlined the main ingredients and ideas that entered into the work, such as 'fission barrier'—and the idea arising from Wheeler's previous studies in variational calculus with Marston Morse—and the capillary oscillations of a liquid due to Lord Rayleigh—as remembered by Bohr (Wheeler, 1979, pp. 274–275).

Bohr and Wheeler treated the cross sections for fission (σ_f), and the emission of radiation (σ_r), obtaining the expressions,

$$\sigma_f = \pi\left(\frac{\lambda}{2\pi}\right)^2 \frac{2J+1}{(2s+1)(2i+1)} \frac{\Gamma_{n'}\Gamma_f}{(E-E_0)^2 + \left(\frac{\Gamma}{2}\right)^2} \tag{789a}$$

and

$$\sigma_r = \pi\left(\frac{\lambda}{2\pi}\right)^2 \frac{2J+1}{(2s+1)(2i+1)} \frac{\Gamma_{n'}\Gamma_r}{(E-E_0)^2 + \left(\frac{\Gamma}{2}\right)^2}, \tag{789b}$$

with the neutron wavelength λ, the rotational quantum numbers l and j of the original and the compound nucleus, respectively, $s = \frac{1}{2}$, Γ_f and Γ_r the corresponding partial widths, Γ the total resonance width, and $\Gamma_{n'}$ a partial width (i.e., the one connected with the breakup of the compound nucleus, leaving the residual nucleus in the ground state and giving the neutrons full energy). These results described the observations, as shown in Section IV. Finally, Bohr and Wheeler discussed the phenomenon of delayed neutrons, as observed by R. B. Roberts, R. C. Meyer, and P. Wang (1939) of the Carnegie Institution in Section V, and in the last Section VI, they discussed the fission processes initiated by deuterons, protons, and photons.[1107]

Though it would be refined further, the theory of Bohr and Wheeler was accepted, and continued to remain ever since, as *the* standard description of the mechanism of nuclear fission. The developments of the next two years concerned the experimental exploration of details of the fission process (e.g., of the delayed neutrons) and of further consequences from the discovery of Hahn and Straßmann. In what was probably the first review article, 'Fission of Heavy Nuclei: A New Type of Nuclear Disintegration,' published in the *Nature* issue of 27 May 1939, Norman Feather of the Cavendish Laboratory expressed at the end the expectations of his colleagues:

> The general result appears to be that, for each process of fission with uranium, at least two neutrons having a mean energy of the order of 10^6 eV, eventually evaporate from the residual fragments. Since neutrons of less than this energy are still capable of producing fission on their own account (probably in ^{235}U ...), the possibility of a cumulative process of exothermic disintegration has to be considered. Clearly, if the probability of removal of neutrons in processes other than those which result in the fission is sufficiently reduced, the latter process must eventually build up in any solid substance containing uranium. Direct experiments on this aspect of matter have not been reported in the scientific literature, but at this stage it may be pointed out that,

[1107] For further addenda, see Bohr, Peierls, and Placzek, 1939, and Bohr and Wheeler, 1939c.

> even in pure uranium, it is well known that a non-fission process takes place, whilst the unlimited generation of energy in the solid material would ultimately increase the energy of the "thermal" neutrons until their efficiency as agents for fission was greatly reduced. Already several attempts have been made to calculate the course of the phenomenon using existing data, but the assumptions upon which they have been based have generally been so severely idealized that no confidence in numerical values is at present likely to result. (Feather, 1939, p. 879)

Norman Feather was a former student of the great Lord Rutherford, who had declared several times before his death in October 1937 that any hope of obtaining by technical means any nuclear energy supply was simply 'moonshine.' However, now only two years later, the prospects appeared to be quite favourable in this respect. Especially, the experimentalists in the Paris laboratory of Frédéric Joliot concerned themselves with determining the number of neutrons liberated in the fission of uranium; after improving their estimates in several attempts, they published in the *Nature* issue of 22 April the rather definite value of 3.5 ± 0.7 neutrons per fission process (von Halban, Joliot, and Kowarski, 1939a).

The French experts also discussed in the scientific literature such problems as the 'control of the chain reaction involved in the fission of the uranium nucleus' (Adler and von Halban, 1939, p. 793), or they dealt with the actual energy gained in the nuclear fission by slow neutrons (von Halban, Joliot, and Kowarski, 1939b). Enrico Fermi and his colleagues Herbert L. Anderson and Leo Szilard of Columbia University contributed another study proceeding even more in the direction indicated by Feather, namely, on 'Neutron Production and Absorption in Uranium' (Anderson, Fermi, and Szilard, 1939). They concluded that 'a chain reaction could be maintained in a system in which neutrons are slowed down without much absorption until they reach thermal energies and are then mostly absorbed by uranium rather than by another element,' but also: 'Whether this holds for a system in which hydrogen is used for slowing down the neutrons, remains an open question.' (Anderson, Fermi, and Szilard, *loc. cit.*, p. 285) Evidently, their preliminary experiment of early summer 1939, using a tank filled with 540 litres of 10-percent $MnSO_4$ solution and 200 kg of uranium oxide in cylinder cans—5 cm in diameter and 60 cm in height—had not decided the question. The choice of the proper material to slow the neutrons without absorbing them (called a 'moderator' in English or '*Bremssubstanz*' in German) would attain a crucial importance in getting the nuclear chain reaction going. In Germany, Siegfried Flügge at Hahn's *Kaiser Wilhelm-Institut für Chemie* dealt with the same problem, though only theoretically, in a general report entitled, '*Kann der Energieinhalt der Atomkerne technisch nutzbar gemacht werden*? (Can the Energy Content of Atomic Nuclei be Used Technically?),' and published in the *Naturwissenschaften* issue of 9 June (Flügge, 1939a). Based on the experimental and theoretical results that had been achieved since December 1938, Flügge obtained rough estimates for the operation of what he called an '*Uranmaschine* (uranium machine),' envisaged to provide 'for the first time the situation, where the technical exploitation of the immense energy amounts stored in atomic nuclei may come into seizable reach even for technical purposes' (Flügge, *loc. cit.*, p. 403). Flügge sketched perhaps the earliest outline of the various aspects—such as the energy of the neutrons from fission, absorption of neutrons by different materials, the cross sections for fission and for scattering of neutrons in uranium and other substances, the diffusion equation determining the growth of neutron number—whose interplay should answer the question of whether nuclear fission could really be used as an energy source on earth, and concluded:

> In sum, we should emphasize again that our present knowledge renders likely the possibility of a "uranium machine" of the sort described above. However, the quantitative material is still connected with the large errors which prevent us from consolidating this possibility into a certainty. Be that as it may, it constitutes an important progress that such possibilities have come into question at all, a progress which, even if the hope is not realized, seems to justify quite well the detailed discussion of this essay. (Flügge, *loc. cit.*, p. 410)

Flügge not only composed this essay for the *Naturwissenschaften*, but also wrote a popular article for the *Deutsche Allgemeine Zeitung*, a widely read German newspaper. In the article, entitled '*Die Ausnutzung der Atomkernegie* (The Exploitation of Nuclear Energy)' and published on 15 August 1939, he envisioned:

> With a single neutron which "initiates the fission" a measurable, even arbitrarily large, amount of uranium will be transformed and thus nuclear energy be liberated. One can rather say exactly how much energy could be gained in this way.... A cubic meter of this [uranium] oxide weighs 4.2 tons and contains 9000 trillion uranium atoms. When transformed ... the total amount of 270,000 trillion metre-kilogram are liberated, enough to shoot out the water content of the Wannsee into the atmosphere. (Flügge, 1939b, p. 2)

As Flügge's report demonstrates, he wanted to draw public attention—at home and abroad—to the existence of this tremendous source of energy; but, by the way, he mentioned nothing about the possibility of a military use of this vast source of energy. Exactly this possibility worried Leo Szilard, a Hungarian-born emigré scientist, who had left Germany in 1933 and—after spending several years in England (where he had thought about the use of nuclear energy, and even taken out a patent on a hypothetical device producing a nuclear chain reaction)—settled in the United States. Already on 2 February 1939, he wrote a letter from New York to Frédéric Joliot in Paris, stating:

> When [Otto] Hahn's paper reached this country a fortnight ago, a few of us got interested in the question whether neutrons are liberated in the disintegration of uranium. Obviously, if more than one neutron was liberated, a sort of chain reaction would be possible. In certain circumstances, this might then lead to the construction of bombs which would be extremely dangerous in the hands of certain governments. (Szilard to Joliot, 2 February 1939)

Clearly, Szilard thought especially of Hitler's *Third Reich* government, and he appealed to the discretion of his colleagues in America, England, and France 'to prevent a leakage of these ideas into the newspapers.' Even more, Szilard wanted to prepare the Western scientists with an appeal not to publish at all any results on matters related to the acquisition of nuclear energy, for as Fermi recalled many years later:

> So he [Szilard] proceeded to startle physicists by proposing to them that given the circumstances of the period, ... given the danger that atomic energy and possibly atomic weapons could become the chief tool for the Nazis to enslave the world, it was the duty of the physicists to depart from what had been the tradition of publishing significant results as soon as the *Physical Review*, or other scientific journals might turn them out, and that instead one had to go easy, keep back some results until it was clear whether these results were practically dangerous or potentially helpful to our side. (Fermi, 1955, p. 14)

Although 'Szilard talked to a number of people and convinced them that they had to join some sort of ... secret society' and 'he sent in this vein a number of cables to Joliot in France, but he did not get a favorable answer from him' (Fermi, *loc. cit.*). In fact, not only French, but also American scientists (including partly Szilard himself) continued to publish articles and notes on various aspects of nuclear fission until, toward the end of the year 1940, the situation really changed, and many results of the kind envisaged by Szilard in February 1939 ceased to appear in *Physical Review*.[1108]

However, Szilard succeeded in another point of his political programme. He got George B. Pegram, Chairman of the Physics Department at Columbia University, to inform Admiral S. C. Hooper (the Technical Assistant to the Chief of Naval Operations in Washington, D.C.) about the progress achieved in nuclear fission and the 'possibility that uranium might be used as an explosive that would

[1108] This change of policy may be traced first in the story of transuranic elements. After the recognition of nuclear fission, Emilio Segrè had stated in May 1939: 'The necessary conclusion seems to be that ... *transuranic elements have not yet been observed*' (Segrè, 1939, p. 1105). A year later, Edwin McMillan and Philip Hauge Abelson of the University of California and Carnegie Institution produced a previously observed 2.3-day (half-life) substance from uranium activated by neutrons in larger amounts, analyzed it, and put in into a 'second "rare-earth" group of similar elements starting with uranium'; that is, they associated it with the transuranic element No. 93 and even looked for the next element No. 94 (McMillan and Abelson, 1940, especially, p. 1186). However, the note in which the California team of Glenn T. Seaborg, E. M. McMillan, J. W. Kennedy, and A. C. Wahl established the existence of the element No. 94 was published only after World War II (1946), the reason being that this new element (named 'plutonium') could serve like the uranium isotope U^{235} as material for bombs. Still, this interruption of publication did not impede the members of the German nuclear energy project: von Weizsäcker in a (secret) report, entitled '*Eine Möglichkeit der Energiegewinnung aus* U^{238} (A Possibility of Obtaining Energy from U^{238})' and dated 17 July 1940, concluded independently from the Bohr–Wheeler theory of nuclear fission that the element No. 93 or better No. 94 may be fissionable even more easily than U^{235} and would therefore provide another nuclear explosive. It was also concluded in Germany, just as in the United States, that the new fissionable element would be produced in considerable amounts in a suitably operating 'uranium machine' ('reactor' or 'pile') and could be chemically separated from uranium.

liberate a million times as much energy per pound as any known explosive.' (Pegram to Hooper, 16 March 1939, quoted in Segrè, 1970, p. 111) Two days later, Fermi gave a lecture on the topic at the Department of the Navy and left with $1,500 to assist him in his investigations on the use of nuclear fission. Szilard did not give up political agitation; together with his fellow countrymen Edward Teller and Eugene Wigner, he persuaded the most famous Germany emigré scientist Albert Einstein (whose acquaintance Szilard had cultivated during his stay in Berlin), to address a letter to President Franklin Delano Roosevelt on 2 August 1939, explaining the practical consequences from the recent research on nuclear physics and arrange funding to speed up the experimental programme on the exploitation of nuclear energy in the United States. The letter from Einstein closed with the warning:

> I understand that Germany has actually stopped the sale of uranium from Czechoslovakian mines which she has taken over. That she should have taken such early action might perhaps be understood on the ground that the son of the German Under Secretary of State, von Weizsäcker is attached to the *Kaiser Wilhelm-Institut* in Berlin where some of the American work is now being repeated. (Einstein to Roosevelt, 2 August 1939, in Segrè, 1970, p. 114, and also quoted in most post–World War II biographies of Einstein)

The last statements, of course, were not literally true. First, Carl Friedrich von Weizsäcker was not yet involved in any work on nuclear energy. Second, after the discovery of nuclear fission at the *Kaiser Wilhelm-Institut für Chemie*, Otto Hahn and his collaborators continued to work on their chemical and physical investigations on nuclear fission (certainly not repeating the American work, but doing their own pioneering research work). However, the findings of Frédéric Joliot in France, and of others in England (e.g., L. L. Michiels, G. Parry, and George P. Thomson, 1939), Poland (Joseph Rotblat, 1939), and Germany (Gottfried von Droste and Hans Reddemann, 1939) on the multiplication of neutrons in the multiplication of neutrons in the fission process excited many scientists also in Germany. Thus, Wilhelm Hanle, immediately after seeing the last results of Joliot and his collaborators, reported in the Göttingen colloquium about the prospects of nuclear energy, upon which Georg Joos, the director of Hanle's institute, wrote a letter informing the German Ministry of Education on 22 April 1939, about the matter. The authorities in Berlin reacted unusually quickly by calling in a meeting of experts on 29 April, inviting besides Hanle and Joos, Peter Debye, Director of the *Kaiser Wilhelm-Institut für Physik* (who did not attend), the experimentalists Walther Bothe (who probably sent Wolfgang Gentner in his place), Hans Geiger, and Gerhard Hoffmann, to discuss 'the problem of a self-sustaining nuclear reaction' (Dames to Bothe, 24 April 1939, in the Bothe-*Nachlaß*). At the meeting in the building of the Ministry of Education, it was decided that Hanle, Joos, and their colleague Reinhold Mannkopff should prepare suitable experiments in Göttingen, but this first official German '*Uranverein* (Uranium Club)' was dissolved in August 1939 when all three participants were called for active military training. Meanwhile, the military authorities—informed by a letter of 24 April 1939 (written by the Hamburg physico-chemists Paul Harteck and Wilhelm Groth), about the possibility of 'an explosive, many orders of magnitude more powerful than the conventional ones'—had taken action (much more slowly than the Civil Ministry of Education). Finally, when on 1 September 1939, the European War started, the *Heereswaffenamt* (Army Weapons Office) formed its own committee, and invited Otto Hahn, Paul Harteck, Gerhard Hoffmann, Walther Bothe, Hans Geiger, Joseph Mattauch, Siegfried Flügge, and Georg Stetter to participate in the fist meeting on 16 September; soon, a second meeting was held, which included more experts—like the experimentalists Klaus Clusius from Munich and Robert Döpel from Leipzig, and the two theoreticians Werner Heisenberg and Carl Friedrich von Weizsäcker. The programme of this new German '*Uranverein*' was formulated by Dr. Basche, an official of the Army Weapons Office, as follows:

> It is the task of the participants to work out all preparatory steps in order to answer uniquely the question whether nuclear energy can be produced on a technical scale. Of course, it would be very nice if the answer turned out to be positive and if one succeeded in achieving a new source of energy. This would very probably also have military significance. A negative result, however, would be just as important, since one could then be certain that the enemy had no access to it either. (Bagge, 1985, pp. 30–31)

Thus began the secret German nuclear energy project of World War II, which involved a number of physicists and chemists (altogether about 100), working at several university institutes (primarily in Hamburg, Leipzig, and Munich) and the institutes of the *Kaiser Wilhelm Gesellschaft* (e.g., the *Institut für Chemie* of Otto Hahn, and the *Institut für Physik* of Peter Debye, both in Berlin, as well as the *Institut für medizinische Forschung* in Heidelberg) and the *Institut für Radiumforschung* in Vienna. Dr. Kurt Diebner of the Army Weapons Office installed himself as the administrative leader at Debye's institute (because Peter Debye, the director of the *Institut für Physik*, having refused to assume German citizenship, went on leave of absence to the United States). The military authorities abandoned the project in early 1942, because the leading scientists agreed that a nuclear weapon could not be built in the near future.[1109]

In the Western countries, which were opposed to Germany in the European War, notably, France and Great Britain, secret programmes to exploit nuclear energy had also been initiated. With the fall of France in June 1940, some members of the Paris group (Hans von Halban and Lew Kowarski) crossed the Channel and went to England, where especially the Jewish refugee scientists from Germany—like Otto Robert Frisch and Rudolf Peierls—concerned themselves actively with

[1109] From an account of the German nuclear energy projects, see the books of David Irving (1967) and Mark Walker (1989)—and Walker's Ph.D. thesis (1987)—the chronology of the most important events and results has been compiled by Rechenberg (1988).

the problem of the 'superbomb.'[1110] In the United States, first of all, the problem of getting a controlled nuclear chain reaction going (in a suitable arrangement of uranium and the bombarding slow neutrons) stood in the foreground, involving in particular Enrico Fermi and several physicists at Columbia University, notably, Herbert L. Anderson, Leo Szilard, and Walter Zinn. Toward the end of the year 1939, they obtained a sum of $6,000 from a Presidential 'Advisory Committee on Uranium' (actually the funds came from the Army and the Navy), to be spent on experiments with neutrons (slowed to thermal velocities by graphite) and uranium oxide, later the uranium metal. The 'National Defense Committee,' organized in June 1940, then supported the Columbia atomic 'pile' project as well as those installed at several other universities (e.g., Princeton) and institutions with much higher budgets.[1111] In connection with a further reorganization of the Committee—after the United States entered the war in December 1941—Arthur H. Compton, now responsible for the programme of producing the nuclear chain reaction concentrated the work in Chicago, and Fermi's group moved in spring 1942 to the University of Chicago. The official report described the crucial moment of this project:

> In July [1942] enough purified uranium oxide from Mallinckrodt was available to permit building intermediate pile No. 9. As in previous experiments, a radium-beryllium neutron source was placed at the bottom of the lattice structure and the neutron density decreased exponentially with increasing distance from the neutron source (hence the name often used for experiments of this type, "exponential pile") and that, from such rates of decrease, the multiplication constant k for an infinitely large pile of the same lattice properties could be calculated. For the first time the multiplication constant so calculated from experimental results came out greater than one. (The actual value was 1.007.) (Smyth, 1948, p. 987)

By looking into the 'Excerpts from Report C-207 for the week ending July 25, 1942,' this result of a multiplicative factor larger than one first occurred here (see Fermi, 1962b, pp. 203–205, especially, p. 204). However, the American break-through definitely occurred after the one achieved in Germany, where Robert and Klara Döpel—together with Heisenberg—worked with a quite different setup, namely, ['*Der experimentelle Nachweis der effektiven Neutronenvermehrung in einem Kugel-Schichtensystem aus* D_2O *und Uranmetall* (The Experimental Proof of the Effective Neutron Multiplication in a Spherical Layer System of D_2O and Uranium Metal)']:

[1110] For specific details of the British programme, see R. H. Dalitz and Rudolf Peierls (1997), especially the 'Complete Bibliography' on pp. 790–793. The British atomic bomb project, code-named 'Tube Alloys,' joined the American war effort in this field after 1942.

[1111] The history of the U. S. nuclear energy project has been told, without going into technical details—see the 'official' Smyth Report (Smyth, 1948). The initial steps of this project, until about the beginning of the middle of 1941, greatly resembled the situation in the German 'military' project; after that period, however, the personnel and industrial involvement and the budget quite surpassed the German efforts, and the goal of the Allied effort changed very much in favour of building the nuclear weapon.

> A spherical layer setup of D_2O and U-metal (layer width 17 cm D_2O, 4 cm U-metal—density 10—, in between 2 to 5 mm Al as support material) possesses a negative absorption coefficient. A simple enlargement of the layer arrangement described here [and consisting of two layers of heavy water and uranium metal] would therefore lead to a uranium-burner, from which energy of the order of the nuclear energy can be taken away. (R. and K. Döpel and W. Heisenberg, in Heisenberg, 1989a, pp. 536–544, especially, p. 537)[1112]

Like Enrico Fermi, Heisenberg had worked on achieving this goal by first developing a theory of the nuclear reactor—he did so in December 1939—and then collaborating with Robert Döpel, his experimental colleague in Leipzig (and the latter's wife as assistant), in establishing a suitable, simple apparatus to construct a 'uranium machine.' In contrast to Fermi's programme, the selected geometry—spherical shells of '*Bremssubstanz*' (the moderator) and uranium oxide—and their chosen heavy water as the moderator hindered Heisenberg and his collaborators to enlarge the apparatus quickly. Especially, the lack of enough amounts of D_2O ultimately prevented the German 'uranium machine' from becoming critical, i.e., it could not achieve the nuclear chain reaction before the end of World War II.

On the other hand, Fermi and his Chicago team—during summer and fall of 1942—continuously enlarged their pile consisting of an assembly of cubes of graphite (as 'moderator' for neutrons) and uranium oxide (as 'fuel'), with some absorption material (cadmium rods) in between to control the neutron flux. As Herbert Anderson recalled:

> From this plot we could tell that the pile would be critical when the 57th layer was completed, on the night between December 1st and 2nd, during my shift. That night the construction proceeded as usual, with all cadmium covered wood in place. When the 57th layer was completed, I called a halt to the work, in accordance with the agreement we had reached in the meeting with Fermi that afternoon. All the cadmium rods but one were then removed and the neutron count taken following the standard procedure which had been followed on the previous days. It was clear from the count that once the only remaining cadmium rod was removed, the pile would go critical. I resisted great temptation to pull the final cadmium strip and be the first to make a pile chain react. However, Fermi had foreseen this temptation and extracted a promise from me to make the measurement, record the result, insert all cadmium rods, and lock them all in place. The next morning, December 2, I was on hand, bright and early, to tell Fermi that all was ready. He took charge then.
>
> Fermi had prepared a routine for the approach to criticality. The last cadmium rod was pulled out step by step. At each step a measurement was made of the increase in the neutron activity, and Fermi checked the result with his prediction, based on the previous step. That day his little six-inch pocket slide rule was busy for this purpose. At each step he was able to improve his prediction for the following. The process converged rapidly, and he could make predictions with increased confidence of being accurate. So it was that when he arrived at the last step, Fermi was quite sure that

[1112] The report is undated, but the experiment *LIV*, in which the result was obtained, ended before an accident on 23 June 1942, destroyed the Leipzig apparatus.

> criticality would be attained then. In fact, once the cadmium rod was pulled out entirely, the pile went critical and the first self-sustaining chain reaction took place. (H. L. Anderson, in Fermi, 1962b, p. 269)

Only some 40 persons, mostly scientists who had done the work, witnessed the emergence of the 'age of nuclear energy' on 2 December 1942, among them the pioneers of nuclear physics like Leo Szilard and Eugene Wigner.[1113] This event was later often described as *the* triumph of modern atomic theory and sometimes also as the 'fall (*Sündenfall*) of modern science,' due to the consequences that it led to.

Certainly, the realization of nuclear energy constituted the result of a long process of evolution in nuclear physics, extending over more than thirty years, in which quantum theory played a decisive role. On the other hand, it might be somewhat exaggerated to call just this event—spectacular as it appeared then and does so especially in retrospect—as the main outcome of the efforts of the quantum physicists, although some of the most prominent representatives of their community were actually involved in the successful outcome. The possibility of acquiring nuclear energy was an obvious consequence of the circumstances connected with the fission of heavy nuclei, as discovered (perhaps accidentally) by chemists. Actually, the whole field of radioactivity, which had initiated the final push of the physicists into the inner structure of matter, rested largely on chemical methods. But it was the physicists who had built the structures of quantum and relativity theories, which made it possible to understand the properties of the basic constituents of matter—of electrons, protons, nuclei, and even such new particles as positrons and cosmic-ray mesotrons. Nuclear fission and its industrial and military consequences just constituted an example of the many applications that followed from the combination of the emergence of fundamental physical theories in the beginning of the 20th century and their reaching at least a preliminary completion in the 1930s. The pursuit of the understanding of the laws of nature based on these fundamental theories would, however, continue, and we shall summarize the main results in the Epilogue.

[1113] Fermi's atomic pile experiment was set up in a squash court below the Stagg field at the University of Chicago. When the pile became critical, Eugene Wigner produced a bottle of *Chianti* from a brown bag, and everyone toasted Fermi's achievement using paper cups. As Wigner remarked, 'It required more foresight to have acquired the bottle of *Chianti*—whose supply would be stopped or exhausted because of the war—than the faith that Fermi would succeed!' (Conversations with Jagdish Mehra on numerous occasions about Fermi's accomplishment of a successfully operating atomic pile.)

Epilogue: Aspects of the Further Development of Quantum Theory (1942–1999)

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Epilogue
1942–1999

In an 'Historical Overview of the Twentieth Century in Physics,' Philip W. Anderson praised the first half of the century as 'the triumph of physics' (Anderson, 1995, p. 2020). Without going into detail—we may say that he indeed had essentially quantum theory and its applications, which we have discussed in these volumes, in mind—he made a note of the popular appreciation of this science which 'attracted increasing support for the research from private philanthropy,' such as the Nobel or the Rockefeller Foundations, while governmental funding was just starting.[1114]

[1114] It is indeed true that after World War I, the Rockefeller Foundation and the International Education Board greatly helped in the building of institutions and scientific research through generous grants and fellowships in countries like Great Britain, Germany, Denmark, Sweden, Italy, and others. Yet in the United Kingdom and Western and Central Europe, a tradition of private philanthropy and governmental support for scientific research already existed. In previous centuries, there were the emperors, kings, princes, dukes, and wealthy aristocrats and merchants who directly encouraged the support of arts, sciences, architecture, and other creative pursuits; however, with the increasing wealth of governmental treasuries and industrial resources, they became the direct benefactors of such munificence and generosity. Thus, for instance, Frederick the Great of Prussia and Catherine the Great of Russia had been the great supporters of intellectual pursuits; later on, the Duke of Brunswick financed the education of Carl Friedrich Gauss, who had been a child prodigy, and Henry Cavendish—the elder son of a duke and inheritor of his title, and himself a noted chemist and physicist—established the Cavendish Laboratory in Cambridge, England, with James Clerk Maxwell as the first holder of the Cavendish Professorship of Experimental Physics. Maxwell's successors—Lord Rayleigh, Joseph John Thomson, Ernest Rutherford, and William Lawrence Bragg—added great lustre to the achievements of the sciences in Great Britain, from which other universities and research institutions benefited. Later, the British government instituted the University Grants Commission and the Department of Scientific and Industrial Research (D.S.I.R., now called the Science Research Council); under the umbrella of the latter, vast national laboratories sprang up in all fields of science and engineering, which also received great support from scientific industry (like the Imperial Chemical Industries). In Germany, the support to science, especially to physics and chemistry in the beginning, came from the *Kaiser Wilhelm-Gesellschaft*, first financed by private sponsors, and later by a mixture of private and governmental support, with Emil Fischer as the first director of its institute of chemistry and Albert Einstein as the first director of its physics institute; very soon after World War II, the *Kaiser Wilhelm-Gesellschaft* was reconstituted as the *Max Planck-Gesellschaft* (with Otto Hahn as President) and established also a large number of new research institutes (the Max Planck-Institutes) in various fields in due course, spread out in places throughout Germany. Furthermore, we should mention the *Physikalisch-Technische Reichsanstalt*, the first institution of metrology, which carried out since 1887 their vast enterprize with financial support jointly from government and industry, after World War II being replaced by *Physikalish-Technische Bundesanstalt*. Similar umbrella organizations, with large numbers of research institutions (such as the C.N.R.S. in France), were established in other European countries as well, including the many distinguished research institutes of the U.S.S.R. Academy of Sciences widely spread over the entire Soviet Union. However, during and after World War II, the United States of America led the world in the organization of huge and enormously successful research and development laboratories and other institutions which received their funding from government agencies, private industry, foundations, and corporations.

The events of World War II, especially the developments of radar, missile technology, jet aircrafts, electronic calculators, computers, and nuclear technology, caused a general change of attitude, that is:

> Governments emerged from World War II convinced that investments in scientific research were vital for their military strength and were economically valuable, and the major participants on both sides [the West and the U.S.S.R. and its satellites] developed, as they recovered economically, systems of national laboratories as well as schemes for supporting science in research universities.... Physics was a major beneficiary of all this activity. (Anderson, 1995, p. 2022)

Thus began the age of 'Big Science,' a name coined by Alvin Weinberg, who had participated in the American–British Manhattan Project (which despite certain predecessors, has often been considered to be the originator of all big-science projects). Weinberg compared the large scientific-technological enterprizes, created in the postwar period—employing hundreds of collaborators and a huge machinery of instruments—to similar large human undertakings of the past historical epochs, notably, the building of the Egyptian pyramids or the great European cathedrals (A. M. Weinberg, 1961).

Nuclear and elementary-particle physics would, in particular, benefit from the establishment of large national or international laboratories, such as the Brookhaven National Laboratory near New York, the Fermilab in Batavia near Chicago, the Stanford Linear Accelerator Center in Stanford, CERN (the European Centre for Nuclear Research) near Geneva, or the (likewise international) Soviet *Institute for Nuclear Research* at Dubna. Other fields of quantum physics, for example, solid-state and semiconductor physics, would flourish on a smaller scale, especially in industrial laboratories—e.g., the Bell Labs in the United States or the research laboratory of *Siemens-Schuckert-Werke* in Germany—as well as in research universities; these were also very often supported by government agencies. In the second half of the twentieth century, many research students and active contributors to scientific research were thus trained in all areas of theoretical and experimental physics, and they would create—together with their teachers and advisors—the so-called 'Atomic Age,' one of the greatest periods in the history of physics. In fact, most of the physical topics treated during this period were closely connected with the fruits of the earlier revolutionary quantum-theoretical developments.[1115] The leading physical journals became dominated by a vast number of papers submitted on atomic, nuclear, elementary-particle, molecular, and solid-state physics, and the new scientific journals—like *Nuclear Physics* and *Physica Status Solidi*—or the subseries of renowned ones (especially the *Physical Review*) became devoted entirely to the specific subfields of quantum physics. Similarly, the

[1115] See, for example, the fields treated in *Twentieth Century Physics*, Volumes II and III (Brown, Pais, and Pippard, 1995); out of 19 chapters, only about three refer to the development of concepts contained already in the classical theories.

journals devoted to chemistry and physical chemistry published an increasing fraction of articles containing results based on the applications of quantum theory; in fact, quantum chemistry advanced to the status of a respected subfield of chemistry itself, especially after electronic computers started to assist scientists in their complex calculations of molecular properties. Only the high expectations raised in the 1930's by Niels Bohr and others—when they hoped to explain the fundamental biological phenomena on the basis of a generalized complementarity concept—did not materialize.[1116] The present understanding of how living matter functions is rather determined by studying thermodynamical and statistical processes that occur far from equilibrium. Whether these processes demand the invocation of new physical principles (whether quantum or classical) has remained an open question.

Therefore, the development of quantum biology will not be treated in the following, in which we shall summarize certain important results derived from the enormous progress of quantum theory following World War II in three sections. Section 1, the comparatively most detailed one, will be devoted to questions of the innermost structure of matter, the nature and properties of subnuclear particles, and their fundamental interactions. In this case, the existing quantum-theoretical schemes had to be extended considerably, and many new physical concepts and mathematical methods were needed in order to achieve a successful description of the observed phenomena. However, during the same period, several already known concepts of the nonrelativistic quantum theory could be substantiated in technologically important applications, such as Albert Einstein's stimulated emission of radiation of 1916/1917 (through the invention of masers and lasers) and his concept of the degeneracy of an ideal quantum gas (Bose–Einstein condensation) or the theory of semiconductors (which led to the transistor effect and the subsequent technological revolution caused by microelectronics). On the other hand, a host of new effects, connected with the names of Rudolf Mössbauer, Yakir Ahar-

[1116] In an address delivered in August 1932 at the 'International Congress on Light Therapy' in Copenhagen, Bohr had considered 'life in biology as an elementary fact' and said: 'The asserted impossibility of a physical and chemical explanation of the function peculiar to life would in this sense be analogous to the insufficiency of the mechanical analysis for the understanding of the stability of atoms.' (Bohr, 1933, p. 458). Pascual Jordan, in an essay on '*Die Quantentheorie und die Grundprobleme der Biologie und Phychologie* (Quantum Theory and the Fundamental Problems of Biology and Psychology)' picked up Bohr's theme and sought to sharpen the latter's idea of a complementarity between the structure of living systems and their biological functions characterizing life (Jordan, 1932b); later on, Max Delbrück advocated similar views, although the pioneering paper which he wrote together with the biologist Nicolaj V. Timoféeff-Ressovsky and the radiologist K. G. Zimmer on the nature of gene mutation and gene structure (Timoféeff-Ressovsky, Zimmer, and Delbrück, 1935) did not establish any particularly new such complementary aspect. Finally, Erwin Schrödinger, in his Dublin lectures of 1943 on '*What is Life*?' also spoke about 'other laws of physics' that might play a role in biology (Schrödinger, 1945, especially, p. 68). The further development of biology, especially the discovery of the double-helix structure of the material building genes (by James Watson and Francis Crick, 1953) and the recognition of the mechanism of the replication of genes revealed, however: 'In order to solve the riddle of life, no retreat to deep-lying concepts (like complementarity) seemed to be necessary, at least not in genetics.' (Fischer, 1987, p. 19) anov, David Bohm, Brian Josephson, Klaus von Klitzing, and others, were discovered which found their explanation in the quantum-mechanical formalism. Moreover, the old riddle to understand superconductivity could be solved consistently in the context of quantum mechanics, although the theoretical understanding of the relatively recently discovered phenomenon of high-temperature superconductivity still remained unresolved. We shall discuss these developments, which occurred mainly in the quantum theory of condensed-matter systems, in Section 2, together with the advances achieved in the quantum-theoretical description of astrophysical phenomena and the evolution of stars and the processes which seem to have occurred in the early universe. Finally, since the early 1950s, a renewed examination has been undertaken of the physical interpretation of quantum mechanics and the philosophical consequences derived from it concerning the understanding of the microscopic world. In particular, several schemes alternative to the standard Copenhagen interpretation of quantum mechanics (which has been explained in Chapters II and IV) were proposed. Moreover, recent highly refined and ingenious experiments have made it possible to check the consequences of both the Copenhagen and the deviating interpretations. In Section 3, we shall delineate some of these philosophically quite captivating suggestions as well as the conclusions derived from the latest experiments, which certainly serve to clarify the peculiar nature of the quantum phenomena and their theoretical description.

1 The Elementary Constitution of Matter: Subnuclear Particles and Fundamental Interactions

The 20th-century physics began, even before the existence of atoms had been firmly established, with the dissolution of the concept of atoms as indivisible entities—which had been so regarded since times immemorial and were now found to possess a structure. After the discoveries of radioactivity and the electrons shortly before 1900, especially the spectroscopic analysis of the chemical atoms and molecules on the basis of quantum theory, led toward the end of the 1920s to a satisfactory theoretical description of the observed facts: The conclusion was that the molecules consisted of atoms bound by ionic or covalent forces, and the atoms were composed of atomic nuclei and electrons held together by electromagnetic forces. Then, in the 1930s, the fact that the atomic nuclei themselves possessed a constitution (which had been suspected already earlier) was proved: It was established that the nuclei are composed of protons and neutrons (together called the *nucleons*), which are closely held together by nuclear forces created by the exchange of Yukawa's mesons (or, perhaps, of electron–neutrino pairs).[1117] Two types of nuclear forces, different in strength, seemed to exist:

[1117] We have reported—in Chapter IV, Sections 3 and 5—the heated debates about the nature of nuclear forces.

the strong forces which ensure the binding of nucleons in nuclei, and the weak forces which cause at least the $\beta^{\pm}$-decays. Two questions remained open: First, it could not be decided whether the weak nuclear forces were also connected with the meson theory or whether Enrico Fermi's 1933 theory of β-decay rather introduced a force of an entirely different nature; second, the new 'mesotron' discovered in 1937 in the cosmic radiation exhibited puzzling features if identified (as seemed to be evident) with the 'heavy quantum' (meson) of Yukawa's theory of nuclear forces—for instance, the decay time of the mesotron came out to be too small by two orders of magnitude (see Section IV.5)—and the strong and electromagnetic coupling of this particle led one to assume rather complex properties (such as spin and coupling constants) of the mesotron. In the early 1940s, when World War II had already engulfed Europe, the second question was studied notably by several theoreticians in Japan and the United States.[1118] At that time, the European physicists mainly studied the problem of strong nuclear forces: Thus, Christian Møller and Léon Rosenfeld in Copenhagen proposed a mixture of scalar and vector mesons to account for the properties (binding energies and quandrupole moment) of the lightest nuclei (1939), and Gregor Wentzel in Zurich developed a strong-coupling approach characterized by a perturbation theory (for scalar mesons coupling with nucleons) in *falling* rather than *rising* powers of the coupling constant (Wentzel, 1940, 1941).[1119] Sin-itiro Tomonaga (in Tokyo) extended and generalized Wentzel's method; Wolfgang Pauli and his collaborators in Princeton examined both the strong-coupling approach and the Møller-Rosenfeld 'patent mixture' of two mesons without arriving at a satisfactory picture of the observed nuclear properties.[1120]

In 1942, the Japanese theoreticians Shoichi Sakata and Takesi Inoue considered a two-meson theory for another purpose, namely, to solve the different behaviour of the observed cosmic-ray 'mesotron' and Yukawa's 'heavy quantum' of (strong) nuclear forces: In particular, they associated the cosmic-ray object with a Fermi particle having spin-$\frac{1}{2}$ and the object of Yukawa's theory with a Bose particle having spin-0 or spin-1.[1121] When this proposal came to the attention of Western physicists four years later (Sakata and Inoue, 1946), several new experiments were ready to confirm it. Already in 1943, members of Enrico Fermi's former group at the University of Rome started (partly in collaboration with Gilberto Bernardini at the University of Bologna) performed a series of experiments which led finally to quite unexpected conclusions (Conversi, Pancini, and Piccioni, 1947).[1122] Bruno

[1118] For details, see Brown and Rechenberg, 1996, Sections 10.6, 11.2, and 11.3.

[1119] Actually, the meson coupling constant—if properly expressed by a dimensionless parameter—turned out to be larger than unity; hence, the conventional perturbation theory would not apply.

[1120] For details of the efforts made by Wentzel, Tomonaga, and Pauli, we refer to Brown and Rechenberg, 1996, Section 11.4.

[1121] The facts of the Japanese two-meson theory have been described by Kawabe, 1991c.

[1122] The history of the Rome experiment has been summarized in Brown and Rechenberg, 1996, Section 13.3. See there for references to the accounts of the participants.

Pontecorvo (another Italian member of the old Fermi team in Rome, who had emigrated to Canada) expressed the main result as follows:

> An immediate consequence of the experiments of the Rome group is that the usual interpretation of the β-process as a "two-step" process ("probable" production of a virtual [Yukawa] meson and subsequent β-decay of the meson) completely loses its validity, since it would predict too long β-lifetimes; the meson is no longer the particle responsible for nuclear β-processes, which are to be described according to the original Fermi picture (without the mesons). (Pontecorvo, 1947, p. 246)

The Rome experiment indeed implied that the cosmic-ray mesotron could not be identified with the strongly interacting meson of Yukawa's theory of nuclear forces. Shortly afterward, Cecil Frank Powell and his collaborators discovered a new particle in cosmic radiation which replaced the previous mesotron as the Yukawa particle (Lattes, Occhialini, and Powell, 1947a, b).

Besides these fundamental problems of nuclear forces and the meson theory—the solution of which in the late 1940s we have indicated—another problem of nuclear theory survived the prewar efforts but was also attacked successfully before 1950: The question, in particular, was whether a shell structure of the nucleons in nuclei existed similar to the shell structure of the electrons in atoms? Already in the 1930s, Walter Elsasser, then in Paris, had offered theoretical arguments for the preference of certain numbers—namely, 8, 20, 50, and 82—for the existence of stable assemblies of protons and neutrons in nuclei (Elsasser, 1933; 1934a, b), but the authoritative review of nuclear states by Hans A. Bethe and Robert F. Bacher had not found much empirical evidence for the specific numbers given, except perhaps for 8 or 20: 'Therefore it seems that the naive theory of neutron and proton shells fails for higher atomic numbers.' (Bethe and Bacher, 1936, p. 177) However, after World War II, Hans Suess from Hamburg—while discussing the cosmic abundance of isotopes of the semiheavy to heavy elements—noticed the frequent occurrence of the numbers 20, 50, and 82 for nucleons (Suess, 1947a, b); and the same conclusion was reached by Maria Goeppert Mayer of Chicago from the observations of particularly stable isotopes detected in nuclear fission products; she also pointed to 126 as the next preferred number (Goeppert Mayer, 1948). In a letter to *Naturwissenschaften*, received on 6 January 1949, Erich Bagge proposed a special numerical model to explain the nuclear shell structure by using two quantum numbers n and m (Bagge, 1949). On the following page of the same *Naturwissenschaften* issue, dated April 1949, there appeared another letter entitled '*Zur Interpretation der ausgezeichneten Nucleonenzahlen in Bau der Atomkerne* (On the Interpretation of the Excellent Numbers of Nucleons in the Constitution of Atomic Nuclei),' and received on 12 February, which provided a more physical interpretation. The authors of this letter—Otto Haxel (of the *Max Planck-Institut für Physik* in Göttingen), J. Hans D. Jensen (of the University of Heidelberg), and again Hans E. Suess (of the University of Hamburg)—especially organized the states of the nuclei, due to 'the strong spin-orbit coupling of the single nucleons' (Haxel *et al.*, 1949a, p. 376). That is, it was not the Russell–Saunders (L, S) coupling, but the spin-orbit coupling of individual nucleons (l, s) which seemed to be preferred physically, and thus explained the preferred numbers for nucleons in stable states.[1123] On the other hand, Maria Goeppert Mayer sent a letter dated 4 February 1949, to *Physical Review*, in which she arrived at the same conclusions as her colleagues in Germany (Goeppert Mayer, 1949). 'Thanks are due to Enrico Fermi for the remark, "Is there any indication of spin-orbit coupling?", which was the origin of the paper,' she wrote (Goeppert Mayer, *loc. cit.*, p. 1970) and concluded her presentation by stating:

> If strong spin-orbit coupling, increasing with angular momentum, is assumed, a level assignment . . . is obtained. This assignment encounters a very few contradictions with experimental facts and requires no major crossing of the levels from those of a square well potential. The magic numbers 50, 82, and 126 occur at the places of the spin-orbit splitting of levels of higher angular momentum. (Goeppert Mayer, *loc. cit.*, p. 1969)

Thus arose the shell model of nuclear structure which became perhaps *the* dominant description of the energy states of atomic nuclei.[1124] Further important aspects of nuclear structure were derived from the spheroidal nuclear model of James Rainwater and the quasi-molecular model of Aage Bohr and Ben Mottelson, which were developed in 1950 and clarified the role of rotational motions within the nuclei.[1125] Although considerable efforts have been devoted in the post–World War II decades to elucidate successfully the properties of nuclear structure and reactions, we shall not enlarge further on this topic; we will rather focus in the following section on the deeper structure of matter as presented by the elementary particles and their behaviour under the influence of fundamental interactions. Finally, it will turn out that some of the elementary particles, recognized as such at first, especially the protons and neutrons, were perhaps not so elementary—as revealed, e.g., by their elastic collisions with high-energy electrons (Hofstadter, 1956, 1989). According to the so-called *Standard Model*, the subnuclear quarks, together with the leptons, represent the smallest constituents of matter. The present section will deal with the main historical steps leading to today's standard concepts and theories of these elementary structures and their fundamental interactions. Thus, we shall arrive at an enormous extension of the domain of relativistic quantum mechanics, although the end of these endeavours

[1123] See also the following letters to *Naturwissenschaften*: Suess, Haxel, and Jensen, 1949; Jensen, Suess, and Haxel, 1949; and the letter of Haxel, Jensen, and Suess to *Physical Review*, dated 18 April 1949 (Haxel *et al.*, 1949b).

[1124] For a discussion of the scientific literature on the shell model, see Mladjenović, 1998, Chapter 15. In 1963, Maria Goeppert Mayer and Hans Jensen would share the Nobel Prize in Physics with Eugene Wigner.

[1125] For a historical summary of these developments, we refer to Mladjenović, 1998, Chapter 16.

has not yet been achieved, as all the problems involved in this enterprize have by no means been solved.

1.1 Some Progress in Relativistic Quantum Field Theory and the Formulation of the Alternative *S*-Matrix Theory (1941–1947)

(a) E. C. G. Stueckelberg: 'New Mechanics' (1941)

In the beginning of the 1940s, relativistic quantum field theory was still plagued by the infinities that had been discovered a decade earlier. Three theoretical physicists—separated from each other by vast distances, with no mutual personal acquaintance or awareness of each other's existence—sought to improve upon this situation by examining more carefully the relativistic structure of quantum mechanics: These physicists were Ernst C. G. Stueckelberg in Geneva, Switzerland; Richard Phillips Feynman in Princeton, New Jersey, and Sin-itiro Tomonaga in Tokyo, Japan. First of all, Stueckelberg presented—at the Basle meeting of the Swiss Physical Society in September 1941—a paper on '*La signification du temps propre en mécanique ondulatoire* (The Significance of Proper Time in Wave Mechanics),' in which he discussed the description of charged particles in four-dimensional space and time (Stueckelberg, 1941a). If the reaction of those particles on the electromagnetic field were considered, the latter would exhibit discontinuities in the hyperplane $x_4 = t = 0$; in the case of an accelerated particle, Stueckelberg thus found, besides an effect of refraction, also reflection 'which corresponds to the worldline of a particle having the same mass but *opposite charge*' (Stueckelberg, *loc. cit.*, p. 322). He published some details of his new approach in two papers submitted to *Helvetica Physica Acta* during the following month (Stueckelberg, 1941b, 1942). In particular, he considered there the process of electron–positron pair creation and annihilation which could be visualized in a space-time diagram: The positron appeared like an electron running backward in time (Stueckelberg, 1941b, pp. 590, 592, Figs. 1 and 2). By a slight formal modification of Einstein's mechanics—avoiding the root in the relativistic extremum condition $\int m\, ds = \int m\sqrt{-\dot{q}_\mu \dot{q}^\mu}\, d\lambda$ by rather choosing $\frac{1}{2}\int \dot{q}_\mu \dot{q}^\mu\, d\lambda$, which did not alter any conclusion in the classical theory—Stueckelberg arrived at what he called '*la mécanique nouvelle* (the new mechanics),' i.e., a relativistic quantum field theory of charged particles having a normalizable invariant four-dimensional density $|\psi|^2$(Stueckelberg, 1942).

(b) The Principle of Least Action in Quantum Mechanics (Feynman and Tomonaga, 1942–1943)

At about the same time, the American graduate student Richard Feynman worked on his Ph.D. thesis, entitled 'The Principle of Least Action in Quantum

Mechanics,' with John Archibald Wheeler at Princeton University; he received his doctoral degree during the University's commencement celebration in June 1942.[1126] During his graduate studies at Princeton, Feynman had explored with his advisor John Wheeler the possibility of formulating an action-at-a-distance electrodynamics in order to eliminate divergence problems, especially the self-energy of charged particles and the zero-point energy of the electromagnetic field. Wheeler had made great plans to publish a series of five papers to deal with and solve all the outstanding problems of electrodynamics, including its quantum-theoretical version, but Feynman just had no faith in this programme. The classical part of the theory, written entirely by Wheeler (and published later on as Wheeler and Feynman, 1945, 1949), resisted all efforts at quantization.[1127] Hence, Feynman pursued the work on his thesis along a different path; he had been searching vigorously for a Lagrangian formulation of quantum mechanics until he ran into Herbert Jehle, who had recently arrived from Europe, at a beer party at the Nassau Tavern at Princeton, and Feynman learned from him that Paul Dirac had indeed proposed just such a scheme nearly ten years earlier. In his paper on the Lagrangian formulation of quantum mechanics, Dirac had pleaded for replacing the usual Hamiltonian formulation of quantum field theory by a Lagrangian one, and had argued:

> In the first place the Langrangian method allows one to collect together all equations of motion and express them as the stationary property of a certain action function.... Secondly, the Lagrangian method can easily be expressed relativistically, on account of the action function being a relativistic invariant. (Dirac, 1933, p. 64)

In working out this proposal, Dirac had considered in particular the quantity which carried the wave function $\psi(x_1)$ of a particle at time t_1 to the wave function $\psi(x_2)$ at time t_2, and assumed it to be 'analogous' to $\exp[(i/\hbar)S]$, with S denoting the classical action function (which depended only on the initial and final space-time points of the particle). Feynman now thought at first that the 'analogous' in Dirac's words essentially meant 'equal,' but he soon realized that it meant 'proportional.' In a simple, nonrelativistic example, he proved that this assumption led to the following evolution equation for the wave function,

$$\psi(X, t+\varepsilon) = \int \exp\left[\frac{i}{h}\varepsilon\mathscr{L}(X, t+\varepsilon; x, t)\right]\psi(x, t)\frac{dx}{A}, \tag{790}$$

[1126] For complete details of Richard Feynman's educational background at Far Rockaway High School, at MIT, and Princeton, as well as the scientific problems which preoccupied him—including the work for his doctoral thesis problem—see Mehra, 1994, Chapters 2 to 6.

[1127] For details of the Wheeler–Feynman action-at-a-distance theory of electrodynamics, see Mehra, 1994, Chapter 5. But, in any case, by 1949—when the second Wheeler–Feynman paper was published—also the renormalized quantum electrodynamical theories of Feynman, Schwinger, and Tomonaga had been published.

with $\mathscr{L}$ the Lagrangian and $t_2 = t + \varepsilon$ (ε small), whose solutions could be identified with those of the Schrödinger equation. Upon using Eq. (790) N times (N being large), Feynman obtained the general expression for the evolution integral

$$\int \cdots \int \exp\left\{ \frac{i}{\hbar} \sum_{i=1}^{N-1} \mathscr{L}[(x_{i+1} - x_i)/(t_{i+1} - t_1), x_{i+1}](t_{i+1} - t_i) \right\} \frac{dx_N}{A_N} \cdots \frac{dx_1}{A_1}, \quad (791)$$

where $t = 0, t_1, t_2, \ldots, t_N = T$ are certain instants of time, which divide the time interval from the initial instant to the final instant T into a large number of small intervals from t_1 to t_{i+1} of duration ε ($i = 1, 2, \ldots, N$), such that $t_i = t + \varepsilon$, and $A_1 = A_2 = \ldots A_N = (2\pi i\hbar\varepsilon/m)^{1/2}$. Then, in the limit when ε goes to zero, we reach the exact quantum function. In this limit, the expression in the exponent in Eq. (791) resembles Riemann's integral for the classical action functional with $\mathscr{L}$ denoting the Lagrangian function):

$$\mathscr{A} = \lim_{\varepsilon \to 0} \left(\sum_{t=0}^{N-1} \mathscr{L}[(x_{i+1} - x_i)/(t_{i+1} - t_i), x_{i+1}](t_{i+1} - t_i) \right). \quad (792)$$

Feynman's conclusion was that Eq. (790) 'is equivalent to Schrödinger's differential equation for the wave function ψ. Thus, given a classical system described by a Lagrangian, which is a function of velocities and coordinates only, a quantum-mechanical description of an analogous system may be written down directly, without working out a Hamiltonian.' (Feynman, 1942, p. 34) This approach thus promised to solve the main problem, which Feynman was trying to attack in his thesis: that is, the quantization of a classical system without knowing its Hamiltonian. In addition, it turned out that he obtained a new general procedure of quantization for classical systems. The physical meaning of expression (791) and the meaning of the underlying limiting procedure was treated by Feynman only six years later in his paper on the 'Space-Time Approach to Nonrelativistic Quantum Mechanics.' (Feynman, 1948a)

In this later paper, Feynman presented in detail his new approach to quantum mechanics. This *third way* of formulating quantum mechanics was based on the new physical interpretation of the mathematical method which he had developed in his thesis. The important step was to arrive at the correct physical interpretation of Eq. (791), which gives the amplitude K for a finite time as the limit of the integration performed multiple times on the coordinates. What could this procedure mean physically? After some general considerations of the relation between probabilities and quantum magnitudes, Feynman arrived at an extremely nice and simple answer to this principal question. To explain how this might be done, he assumed that he had a particle moving in one dimension, which could take up various values of a coordinate x. Then, he wrote the formula (791) in the form

$$K = \lim_{\varepsilon \to 0} \int_R \exp\left(\frac{i}{\hbar} \sum_i S(x_{i+1}, x_i) \ldots \right) \frac{dx_{i+1}}{A} \frac{dx_i}{A} \cdots, \quad (793)$$

where A is a normalization factor (see above). Now, the coordinates $x_1, x_2, x_3 \ldots$, which lie in some region R, could be considered as coordinates of the particle at corresponding times $t_1, t_2, t_3 \ldots$. 'From the classical point of view, the successive values $x_1, x_2, x_3 \ldots$ of the coordinates practically define the path $x(t)$. Eventually, we expect to go to the limit $\varepsilon \to 0$.' (Feynman, 1948a, p. 370) By varying the values of coordinate x_i, we would have various paths in the range R.

The quantity $S(x_{i+1}, x_i)$ in Eq. (793) is simply the classical action on the corresponding path from point x_{i+1} to point x_i. Hence, the sum in the exponent in Eq. (793) in the limit $\varepsilon \to 0$ goes to the classical action on the path $x(t) : S = \lim_{\varepsilon \to 0} \Sigma_i S(x_{i+1}, x_1)$. Finally, the many-time integration in Eq. (793) evidently means a summation over all possible paths in the range R, since by varying the path of the integration we will have all possible paths in this range. But this means just the *interference of terms* $\exp(iS/\hbar)$, which corresponds to every possible path in R. Hence, Feynman's postulate was: '*The paths contribute equally in magnitude, but the phase of their contribution is the classical action* (*in units of* $\hbar$), *i.e., the time integral of the Lagrangian taken along the path.*' (Feynman, *loc. cit.*, p. 371)

Later on, Feynman explained this postulate as follows:

> The total amplitude can be written as the sum of amplitudes of each path—for each way of arrival. For every $x(t)$ that we could have—for every possible imaginary trajectory—we have to calculate an amplitude. Then we add them all together. What do we take for the amplitude for each path? Our action integral tells us what the amplitude for a single path ought to be. The amplitude is proportional to some constant times $\exp(iS/\hbar)$, where S is the action for the path. That is, if we represent the phase of the amplitude by a complex number, Planck's constant $\hbar$ has the same dimensions. It is the constant $\hbar$ that determines what quantum mechanics is important. (Feynman and Hibbs, 1965, p. 19)

Thus, Feynman's postulate ultimately yielded the principle of least action, $\delta\mathscr{A} = 0$, where $\delta\mathscr{A}$ is the variation of the action functional $\mathscr{A} = \int mv\, ds$ in the problem of the motion of a mechanical particle between two fixed centres in the plane in a gravitational field, gives us the right explanation as to where this principle is coming from. As far as the principle of least action, *the* most fundamental principle of classical physics, is concerned, one implies the classical dynamical equations in all the fundamental classical theories, and one can truly say that it was Feynman who discovered the deepest import of this principle.[1128]

[1128] A derivation of the principle of least action from quantum-mechanical reasoning was first given by Paul Dirac (Dirac, 1933). This served as the point of departure for Feynman's investigations on the path-integral method. Considering Eq. (793) as an approximation to the exact quantum transition function from the initial to the final instants of time, Dirac discovered that the quantum analogue of Hamilton's action principle, $\delta\mathscr{A} = 0$, is absorbed in the composition law (793), and the classical requirement that all values of the intermediate coordinates shall make the action stationary corresponds to the condition in quantum mechanics that all values of the intermediate coordinates shall make the action stationary. Then, Dirac considered the limiting case when $\hbar$ is small. Thus, the multiple integral (793):

When he published the contents of his thesis in his paper on 'Space-Time Approach to Non-Relativistic Quantum Mechanics' (Feynman, 1948a), Feynman developed the new formalism of quantum mechanics and proved its equivalence to the older formulations of Heisenberg and Schrödinger. He would show then how one can introduce the wave function in his path-integral approach, and derive the Schrödinger equation for this wave function. There, he also employed his new notion of the 'transition amplitude,' which can now be found in textbooks on quantum mechanics. Given two quantum states with wave functions $\psi(x,t)$ and $\chi(x,t)$, Feynman called the expression $\int \chi^*(x,t)\psi(x,t)\,dx$ the 'transition amplitude.' Here, $\chi^*(x,t)$ is the function conjugate to $\chi(x,t)$ at the instant of time t'', and $\psi(x,t)$ is taken at another instant of time t'. Thus, the transition amplitude give us the quantum amplitude for the transition from the state ψ at the time t' to the quantum state χ at the time t''. Feynman showed that in his path-integral method, the average of the transition amplitude may be regarded as unity; that is

$$\langle \chi_{t''}|1|\psi_{t'}\rangle = \lim_{\varepsilon\to 0}\int\ldots\int \chi^*(x'',t'')\times\exp(iS/\hbar)\psi(x',t')\frac{dx_0}{A}\ldots\frac{dx_{j-1}}{A}\,dx_j. \quad (794)$$

In the language of ordinary quantum mechanics, if the quantum Hamiltonian operator $\mathbf{H}$ does not depend on time, this transition amplitude is the matrix element of the quantum evolution operator $\exp[-i(t''-t')\mathbf{H}/\hbar]\psi(x,t')$. As a generalization of formula (794), Feynman wrote the formula for the averages of any functional F of the coordinates x_i for $t' < t_i < t''$. He defined the transition element' of the functional F between the states ψ at t' and χ'' at t'' for the action S as

$$\langle \chi_{t''}|F|\psi_{t'}\rangle = \lim_{\varepsilon\to 0}\int\ldots\int \chi^*\psi(x'',t'')F(x_0,x_1,\ldots x_j)$$
$$\times\exp\left(\frac{i}{\hbar}\sum S(x_{i+1},x_i)\cdot\psi(x',t')\right)\frac{dx_0}{A}\ldots\frac{dx_{j-1}}{A}\,dx_j. \quad (795)$$

Then, he used these basic formulas to obtain several fundamental results from his new formulation of quantum mechanics, including—first of all—the new formu-

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> contains the quantum analogue of the action principle, as far as the importance of our considering any set of values for the intermediate [coordinates] is determined by the importance of this set of values in the integration. If we now make $\hbar$ tend to zero, this statement goes over into the classical statement that ... the importance of our considering any set of values for intermediate [coordinates] is zero unless these values make action stationary. This statement is one way of formulating the classical action principle. (Dirac, 1933, p. 70)

From the above remarks, it is clear that Dirac was very close to the interpretation of Eq. (793) as a summation over all virtual paths, and he had found the formulation as an extremely nice and important way to explain the principle of least action as a result of quantum laws. However, Dirac was not able to complete this line of his investigation on quantum mechanics because his point of view was that the exponent of the classical addition in the form of Hamilton's principal function is only an approximate semiclassical relation. Dirac was interested only in a general question: What is the quantum analogue of the classical principle of least action?

lation of the so-called perturbation theory in quantum mechanics and the formulation of a completely new derivation of Newton's equations and the commutation relations. Now, Eq. (795) already appeared in his doctoral dissertation of 1942, where he presented an outline of the generalization of the path-integral method with applications to solve certain problems. The background of his doctoral dissertation at Princeton, and his later formulation in 1948, of the space-time approach to nonrelativistic quantum mechanics would play a decisive role in his systematic approach to developing the space-time approach to relativistic quantum mechanics within a short time. (For more details, see Mehra, 1994, Chapter 10.)

When Feynman received his Ph.D. from Princeton University in June 1942, the senior physicist Sin-itiro Tomonaga contemplated about the same problem in far away Tokyo. In his Nobel lecture,[1129] Tomonaga recalled:

> Around 1942, [Hideki] Yukawa wrote a paper [in Japanese, *Kogaku* **12**, 249 (1943)] emphasizing the unsatisfactory aspect of the quantum field theory. He thought it necessary to use the idea of the g.t.f. (generalized transformation function) proposed by Dirac [Dirac, 1933] to correct this defect of the theory.... Yukawa's idea was to introduce as the basis of a new theory a concept which generalized the conventional conception of the probability amplitude. However, as pointed out also by Yukawa, we encounter the difficulty that, in doing this, cause and effect cannot be clearly separated from each other. According to Yukawa, the inseparability of cause and effect would be an essential feature of quantum field theory, and without abandoning the causal way of thinking which strictly separates cause and effect, it would not be possible to solve various difficulties appearing in quantum field theory ... I thought, however, that it might be possible (without introducing such a drastic change as Yukawa and Dirac tried to do) to remedy the unsatisfactory, unpleasant aspect of the Heisenberg-Pauli theory of having a common time at different space points. In other words, it should be possible, I thought to define a relativistically meaningful probability amplitude which would be manifestly relativistically covariant, without being forced to give up the causal way of thinking. In having this expectation I was recalling Dirac's many-time theory which had enchanted me 10 years before.
>
> When there are N particles in Dirac's many-time theory, we assign a time t_1 to the first particle, t_2 to the second, and so on, thus introducing N different times, $t_1, t_2, ..., t_N$, instead of the common time t. Similarly, I tried in quantum field theory to see whether it was possible to assign different times, instead of one common time, to each space point. And in fact I was able to show that this was possible [See Tomonaga, 1946; Koba, Tati, and Tomonaga, 1947; Kanazawa and S. Tomonaga, 1948a, b]. (Tomonaga, 1966, pp. 26–27)

Tomonaga published the result in a Japanese paper in 1943, an English translation of which would appear under the title 'On a Relativistically Invariant Formulation

[1129] Sin-itiro Tomonaga shared the Nobel Prize for Physics in 1965 with Richard Feynman and Julian Schwinger; however, he was unable to attend the Nobel ceremonies in Stockholm in December because he had suffered an accident during the celebration following the announcement of the news of the Nobel Award earlier in October 1965.

of the Quantum Theory of Wave Fields' three years later in *Progress of Theoretical Physics* (Tomonaga, 1943/1946). Similar to Feynman, Tomonaga arrived at an extension of the Schrödinger equation involving a generalized probability amplitude $\Psi[C]$, with C a space-like surface in the four-dimensional world, and the transformation operator [corresponding to the Dirac–Feynman quantity Eq. (791)] assuming the form

$$T[C_2, C_1] = \prod_{c_1}^{c_2} \left(1 - \frac{1}{\hbar} H_{12}\, d\omega\right), \tag{796}$$

with H_{12} denoting the interaction energy density and $d\omega$ denoting a 'world element' surrounded by two space-like surfaces. 'We have thus shown that the quantum theory of wave fields can be really brought into a form which reveals directly the invariance of the theory against Lorentz transformations,' Tomonaga concluded, and explained:

> In our formalism the theory consists of two sections, one of which gives the laws of the behaviour of the fields when they are left alone, and the other of which gives the laws determining the deviation from this behaviour due to interactions. This way of separating the theory can be carried out relativistically.
>
> Although in this way the theory can be brought into a more satisfactory form, no new contents are added thereby. So the well known divergence difficulties of the theory are inherited also by our theory.... Thus a more profound modification of the theory is required in order to remove this fundamental difficulty. (Tomonaga, *loc. cit.*, English translation, pp. 41–42)

We shall deal with the details of Tomonaga's paper and the continuation of the work in Tokyo immediately after the war, which was to a large extent independent of the work in America but arrived at essentially similar results—see below.

(c) Heisenberg's *S*-Matrix (1942–1947)

While all of the above-mentioned attempts were directed toward improving the relativistic quantum field-theoretical description, Werner Heisenberg, who—together with Wolfgang Pauli—had not only pioneered the development of the theory but also become—toward the end of 1930s—one of the most severe critics of its limitations, contemplated about more radical means to remove the known divergence difficulties. In Chapter IV, Section 7, we have already mentioned two ideas by means of which he proposed to change the existing formalism, especially the introduction of a small 'universal length' and the use of nonlinearity in the equations. In a series of three papers, submitted between September 1942 and May 1944 to *Zeitschrift für Physik* (Heisenberg, 1943b, c; 1944), Heisenberg pursued a different goal, as he stated in the summary of his first paper on the subject: 'In view of the later alteration (*Abänderung*) of the theory, the present investigation attempts to isolate from the conceptual scheme of the quantum theory of wave fields those concepts which probably will not be affected by the future changes [in the theory of elementary particles] and which may therefore represent an integral part (*Bestandteil*) also of the future theory.' (Heisenberg, 1943b, p. 513)[1130] As such concepts, Heisenberg selected 'the "observable quantities" of the present theory,' because 'the future theory should, in the first place, also contain relations between "observable quantities"'—though he admitted that 'only the final theory will decide which quantities are "really observable"' (Heisenberg, *loc. cit.*, p. 514). In any case, the following two quantities should always be considered as observable: the discrete energy values of the stationary states of closed systems and the asymptotic behaviour of wave functions in scattering, emission, and absorption processes. Moreover, Heisenberg noticed that these two basic observable properties could be related to each other, since the phase difference between the incoming and outgoing wave functions should yield the discrete energy states as well, provided the scattering system was enclosed in a sphere of large radius around the scatterer. To obtain the desired relations, Heisenberg introduced his unitary 'characteristic' S-matrix, which he expressed as

$$S = \exp(i\eta) \tag{797}$$

in terms of a relativistically invariant Hermitean matrix η that contained all of the observable variables of the theory. In his second paper on the subject, received by the journal in October 1942, Heisenberg discussed two examples which could not be adequately treated in the existing quantum field theory: (i) a distance-dependent interaction resulting in finite cross sections for the scattering of particles with arbitrarily high energies; and (ii) an interaction leading to the creation of many new particles (Heisenberg, 1943c).

Despite the unfortunate conditions during World War II, which hindered the propagation of Heisenberg's S-matrix theory considerably, his publications and talks—presented in Switzerland (1942 and 1944), The Netherlands (1943), and Denmark (1944)—attracted several physicists, who started to work immediately on the new theory, notably, Ernst C. G. Stueckelberg in Geneva (Stueckelberg, 1944a, 1945, 1946), Hendrik Kramers in Leyden (Kramers, 1944), and Christian Møller in Copenhagen (Møller, 1945, 1946). Even in far away Cambridge, Massachusetts, the news about Heisenberg's S-matrix was brought by Wolfang Pauli (who spent the war years at the Institute for Advanced Study in Princeton, New Jersey, and who visited MIT in the fall of 1944 to lecture at the Radiation Laboratory on the latest developments in meson physics). He was then in the process of

[1130] In the summer of 1937, John Archibald Wheeler had been the first to propose how to derive the properties of light nuclei, such as energy levels and transition probabilities; with the help of a unitary matrix, they were connected with the transition from incoming to outgoing groups, consisting of a few protons and neutrons within the nuclei in question (Wheeler, 1937b, c)—see Section IV.5. A detailed account of the origin and development of Heisenberg's new theory, later called the S-matrix theory, has been given by Rechenberg, 1989.

writing a book on *Meson Theory of Nuclear Forces* which appeared shortly after the war (Pauli, 1946). In his lectures, all of which Julian Schwinger (who spent the war years at the MIT Radiation Laboratory) attended, Pauli gave a detailed account of Heisenberg's work on the *S*-matrix. Pauli's lectures came at a very opportune time. The method of the scattering matrix could be easily generalized to describe the scattering of any kinds of waves, not only the Schrödinger waves of quantum physics. Schwinger found the analogy strikingly handy and used the concept of the scattering matrix for isolating those characteristics of microwave propagation through waveguides that are essentially independent of the detailed nature of discontinuities in the waveguides. As Schwinger recalled:

> Here you are trying to describe what is going on in a certain junction with various inputs from different waveguides. You send something in, something comes out. And there is the same question of how far you can go without knowing in detail what's inside. It was not the *S*-matrix theory as Heisenberg had developed, which would be to separate the *S*-matrix from everything else, but it was a computational tool. That was, of course the amusing thing, because physicists naturally will talk about not so much the **S**-matrix but reflection and transmission amplitudes. The engineers will talk about impedances than something else. . . . This is the period in which I developed for practical purposes the variational method that I transferred to scattering theory. (Julian Schwinger to Jagdish Mehra, mid-March 1988; quoted in Mehra and Milton, 2000, Chapter 4)

Hendrik Kramers greatly liked the entire formalism and discussed aspects of it in an exchange of correspondence with Heisenberg, especially the question on whether the *S*-matrix of a given system would describe—besides all the scattering of cross sections—also the position of bound states. In his third paper, Heisenberg declared that 'This gap, which existed in the present considerations, has been closed by a remark of Kramers, according to which one can treat the matrix $(k_i'|S|k_i'')$ as an analytic function of the state variables ([i.e., the four-momenta] k_i' and k_i'') and derive the stationary states from its behaviour in the complex plane,' (Heisenberg, 1944, p. 94), and added:

> The zeros of the matrix S for imaginary k_i' determine the position of the stationary states. For the eigenvalues of η this result signifies that the poles on the imaginary k-axis determine the position of the stationary states. (Heisenberg, *loc. cit.*, p. 95)

On the other hand, Christian Møller from Copenhagen wrote a long letter to Heisenberg on 28 December 1943, concerning the details of the *S*-matrix theory, and he took advantage of the opportunity to discuss matters orally with Heisenberg during the latter's visit to the Danish capital in April 1944. He accepted the analyticity property of the characteristic matrix and published two long papers on the theory, in which he worked out the formalism accurately in full mathematical detail (Møller, 1945, 1946); moreover, he served as a spokesman on behalf of Heisenberg's theory immediately after the war (when his German colleague could no longer travel freely), by delivering a series of lectures at the University of Bristol in spring 1946 and at several conferences. In particular, at the first international conference of that period on ‘Fundamental Particles and Low Temperature Physics’ in Cambridge, England, the *S*-matrix theory received quite some attention, and besides Møller, Pauli, Heitler, and Stueckelberg focused on it. As against Møller and Stueckelberg who spoke very favourably about the possibilities of Heisenberg’s approach, Pauli expressed considerable criticism, and in particular, he said:

> Heisenberg did not give any law or rule to determine mathematically the *S*-matrix in the region where the usual theory fails because of the well-known divergences. Hence his proposal is at present still an empty scheme. (Pauli, 1947, p. 6)

Pauli also felt that the concept of analytic continuation was ‘a bit alien to physics and you never know whether it works or not,’ and finally complained that ‘the rather complicated formalism does not contain classical mechanics as a limiting case’ (Pauli to Møller, 24 September 1945 and 18 August 1946; see Pauli, 1993, p. 313, and *Werner-Heisenberg-Archiv*, Munich).

Actually, Pauli showed little sympathy with the alternative to quantum field theory. He would claim, for instance, that his collaborators Shih-Tsun Ma (1946) and Res Jost (1947) had found ‘wrong zeros,’ which could not be associated with physical particles.[1131] In spite of the disagreement concerning details, Heisenberg —after World War II—did share Pauli’s skeptical judgment, namely, ‘that the *S*-matrix is not a concept of which we may expect that it would occur in a future theory as a primary fundamental concept’ and that ‘it might hardly be suitable to lead us beyond the present wave mechanics’ (Heisenberg to Pauli, 9 September 1946, in Pauli, 1993, pp. 382–383). Indeed, he would soon turn again to the discussion of quantum field theories. Still, he was glad to assist Richard Eden, a student of Paul Dirac’s, in working out a thesis on the *S*-matrix formalism (Eden, 1949a, b). After 1948, the general interest in the alternative theory of elementary particles declined. The triumph of quantum field theory in quantum electrodynamics left to the *S*-matrix only the role of a sometimes useful mathematical tool.

1.2 The Renormalized Quantum Electrodynamics (1946–1950)

(a) The Shelter Island Conference (1947)

In the preface to his ‘*Selected Papers on Quantum Electrodynamics*,’ Julian Schwinger first described the prewar status of the theory and then went on:

[1131] Many years later, Reinhard Oehme wrote: ‘Today we know that the “false” singularities … are just remnants of what appears as crossed-channel singularities in amplitudes obtained from relativistic field theories.’ (Oehme, 1989, especially, p. 607)

> Further progress came only with the spur of experimental discovery. Exploiting the wartime development of electronic and microwave techniques, delicate measurements disclosed that the electron possessed an intrinsic magnetic moment slightly greater than that predicted by the relativistic quantum theory of a single particle [Foley and Kusch (1948)], while another prediction of the latter concerning the degeneracy of states in the excited levels of hydrogen was contradicted by observing a separation of the states [Lamb and Retherford (1947)]. (Historically, the experimental stimulus came entirely from the latter measurement; the evidence on magnetic anomalies received its proper interpretation only in consequence of an additional spin magnetic moment.) If these new electron properties were to be understood as electrodynamic effects, the theory had to be recast in a usable form. The parameters of mass and charge associated with the electron in the formalism of electrodynamics are not the quantities measured under ordinary conditions. A free electron is accompanied by an electromagnetic field which effectively alters the inertia of the system, and an electromagnetic field is accompanied by a current of electron-positron pairs which effectively alters the strength of the field and of all charges. Hence a process of renormalization must be carried out, in which the initial parameters are eliminated in favor of those with immediate physical significance. (Schwinger, ed., 1958, pp. x–xi)

The decisive experimental and theoretical progress occurred largely—though not entirely—in the USA, and it gave the relativistic quantum field theory of electromagnetic interaction quite a new shape, despite the fact that most of the concepts that entered into the new theory had already been advanced in the 1930s. However, the new systematic renormalization procedure did provide a mathematical recipe for reliably computing from clear prescriptions the results of the refined postwar experiments in the USA. Remarkably enough, the three decisive experiments were performed during the same year at the same place, namely, at Isidor I. Rabi's laboratory (the Pupin Laboratory) at Columbia University; besides the two experiments already mentioned above by Schwinger, also John F. Nafe, E. B. Nelson, and I. I. Rabi (1947), carried out a third one preparing the ground for the result of Foley and Kusch. The determination of the fine structure energy difference between $2^2S_{1/2}$ and $2^2P_{1/2}$ states of hydrogen by Lamb and Retherford made use of the radiofrequency resonance method pioneered by Rabi before the war, while in the case of the Nafe–Nelson–Rabi experiment, some old prewar apparatus was employed.[1132]

The earlier prewar involvement of Willis E. Lamb, Jr., in the question on whether the observed hydrogen spectrum satisfied the Dirac equation and the then standard quantum electrodynamics has been reported in Section IV.7.[1133] During

[1132] For a detailed account of the experimental and theoretical contributions, especially by American physicists to quantum electrodynamics, we refer to the books of Mehra (1994), Schweber (1994), and Mehra and Milton (2000).

[1133] Willis E. Lamb, Jr., born on 12 July 1913, in Los Angeles, California, first studied chemistry (in which he majored) at the University of California in Berkeley, but at the same time he took many courses in physics and mathematics. In the fall of 1934, Lamb became a graduate student in physics at Berkeley, where he took J. Robert Oppenheimer's course in quantum mechanics; by the end of his second semester as a graduate student, he had been exposed to Dirac's equation and had received an

World War II, Willis Lamb worked on various problems in nuclear and molecular beam physics, and in 1943, he joined the secret radar project at the Columbia Radiation Laboratory. When, in 1946, Robert Retherford (who, before the war, had begun research work under Jerome Kellogg) returned to Columbia University as a graduate student, he became an ideal collaborator for Lamb, for he knew a lot about vacuum and other techniques necessary for the task of investigating the spectrum of hydrogen. For that purpose, Willis Lamb designed an absorption experiment, since he was familiar with building continuous-wave magnetrons operating in the centimetre range. As Lamb recalled later: 'The experiment first succeeded on Saturday, 26 April 1947, and turned out very much as expected, except for the location of the resonances' (Lamb, 1983).[1133a] Lamb and Retherford reported at the time:

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introduction to quantum electrodynamics. Soon he became a member of Oppenheimer's research group in theoretical physics, and beginning in 1935, he began to join in the annual migrations of Oppenheimer and his group between Berkeley and Caltech. During the summer of 1932, Lamb attended the Michigan Summer Symposium at Ann Arbor, where he listened to the lectures by Enrico Fermi, Felix Bloch, George Uhlenbeck, and Samuel Goudsmit among others. The research students in Oppenheimer's group worked on problems in nuclear physics, quantum electrodynamics, and, soon after the publication of Yukawa's proposal of the meson, on meson theory—all of them being topics at the very frontiers of modern physics. Weekly seminars alternated between Berkeley and Stanford (where Felix Bloch was), and Lamb became very close to Bloch for the rest of his life. Inspired by a talk given by Arnold Siegert on exchange currents that contribute to radiative interactions, Lamb eventually worked on this problem for his thesis, taking his doctorate at Berkeley in 1938. The relations between Oppenheimer and Lamb had always been rather rocky; Oppenheimer did not seem to care much for the kind of problems in which Lamb was interested, 'nor did he suffer fools gladly; I occasionally annoyed him by my ignorance and obtuseness.' (Willis Lamb in conversations with Jagdish Mehra) Willis Lamb met I. I. Rabi for the first time during the latter's visit to Stanford in the summer of 1938; Rabi encouraged him to apply for a position at Columbia University, which Lamb did and was offered an instructorship there. Several decades later, Rabi wrote to Lamb:

> I flatter myself as the first man to recognize your genius in the most practical way of giving you a job. It is one of my deeds for which I have no regrets. Your coming to Columbia turned out to be one of the great events in the history of physics. The Lamb shift and its theory were the great events that were decisive for the development of QED. You also established yourself as one of the small group of physicists who could do both experiments and mathematical theory in the spirit of Enrico Fermi. (Rabi, in ter Haar and Scully, 1978, p. XLII)

[1133a] In his talk at the Fermilab symposium on 'The Birth of Particle Physics' in May 1980, Willis Lamb gave an account of the steps that led him to his famous experiment on the fine structure of hydrogen (Lamb, 1983, pp. 321–322). Willis Lamb reconstructed the successful moment in an interview with Mehra, in which he said:

> The experiment first succeeded on Saturday, 26 April 1947. Retherford worked with me on that Saturday morning, then we stopped at noon and he [Retherford] went back to New Jersey, where he lived. I went home and then later in the evening, on Saturday evening, the temptation to see if the experiment still worked grew large. Let me say a little more about how the experiment worked.... When the apparatus was sufficiently evacuated, we could turn on the microwaves and we would get everything turned on. There were many switches to throw and we would vary the magnetic field from some low value, perhaps a few hundred gauss to some high value, perhaps a few thousand gauss, and if we did that we would see that as the magnetic field changed the spotlight would move. Of course there had to be all sorts of tests to make sure that this motion was not coming from some spurious effect that we were not interested in. We were

> The results indicate clearly that, contrary to the theory but in essential agreement with Pasternack's hypothesis (1938), the $2^2S_{1/2}$ state is higher than the $2^2P_{1/2}$ by about 1000 Mc/sec. (0.033 cm^{-1}) or about 9 percent of the spin-relativity doublet separation. (Lamb and Retherford, 1947, p. 242)

Even before the results of the Lamb–Retherford investigation were submitted for publication, Rabi and his collaborators reported about a new measurement of the hyperfine structure of hydrogen and deuterium (Nafe, Nelson, and Rabi, 1947). The interaction of the electron's magnetic dipole moment with the nuclear magnetic field caused a splitting of the $2^2S_{1/2}$ ground state into two components. If the experimentally observed splitting (1421.3/mc) was compared to that calculated with the gyromagnetic factor $g_S = 2$ of the electron (1416.9 ± 0.54/mc), a discrepancy showed up which was 'five times greater than the claimed probable error in the natural constants' (Nafe *et al.*, *loc. cit.*, p. 915). Hence, Rabi's team sought further checks, which were undertaken by Foley and Kusch; they investigated the cases of the hyperfine structures of gallium and sodium and obtained the values

$$g_S = 2.00229 \pm 0.00008 \quad \text{and} \quad 2.00244 \pm 0.00006, \tag{798}$$

respectively (Kusch and Foley, 1947; Foley and Kusch, 1948).

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concerned with transitions between $2S$ and $2P$ states and in fact at the time we were using a clystron with a frequency in the 3-centimeter region and so we were seeing transitions between $2S_{1/2}$ and $2P_{3/2}$. As I explained, the frequencies depended on the magnetic field and the frequencies of the atomic transitions were brought into resonance with the frequency of the clystron. We weren't able to tune the clystron very much. We could tune it a little bit but it was a very tedious operation and hardly worth the trouble. But it was easy to change the magnetic field. The resonant transitions are not infinitely sharp; they are quite broad. They are broadened because of what is called radiation damping. The fact that you are dealing with a $2P$ state with a finite lifetime, like 1.6×10^{-9} sec., means that the width of the resonance curves that one gets would be about 100 megacycles and that translates into something like 100 gauss depending on the transition. As we varied the magnetic field from a low value to high value there would be a range of magnetic fields perhaps 100 gauss wide at some finite magnetic field where the spot of light would be changing and as we increased the field, the light would come back again. The spot of light would move along the scale as we went through the resonance and come back again.

So that evening, Saturday evening, I wanted to go over and see if the experiment was still working because we had to break it off a little early. It clearly had worked, but there would always be the possibility that what we had seen was only a spurious effect, something of no interest. In order to run the apparatus myself I didn't quite have enough hands because when you do this you want to take some data or you want to turn some knobs to change the magnetic field or do a number of things. So it seemed desirable to have some more hands. So Ursula, my wife, came along, and we walked from where we lived on 122nd Street over to where one could get into Columbia University through the nearest gate or gates, which would have been on 119th Street and Broadway or 120th Street, halfway between Amsterdam Avenue and Broadway, but you had to go up and get a special key from the University office in the Low Library, where the key that they gave you would be connected to a large brass disc which had teeth cut into it....

I turned on the apparatus. It was being pumped all the time so there wasn't much to do, but the magnetic field and the clystron had to be turned on. I went through the procedure that we had done earlier that day [with Retherford] and the effect was still there. I imagine that on that weekend Rabi had been in Washington, but by Sunday I had called him on the phone and told him about it. I think there was a party at his house that night, rather incidental, having nothing

The investigations at Columbia University went on during a conference sponsored by the National Academy of Sciences (*NAS*) that was held from 2 to 4 June 1947, at Ram's Head Inn on Shelter Island at the tip of Long Island.[1133b] According to the organizers, the Shelter Island Conference on 'Fundamental Problems of Quantum Mechanics' should bring together especially young and promising American scientists with some well-established (though not all Ameri-

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to do with hydrogen fine structure, but just some people who had been to the Washington meeting [of the American Physical Society] were in New York and Rabi gave them a party. Rabi had gone to attend it, but I had not, because I was quite interested in what was going on in the Lab. At that time I didn't show anybody the experiment, but I told them what had happened. I think Rabi was very pleased. There were a number of people at the party; I remember that George Uhlenbeck was there and he was, I think very pleased to hear about it. [Ursula Lamb recalled that Uhlenbeck grabbed Willis by the arm and congratulated him.] After all, he had something to do with it through the invention of the electron spin magnetic moment. Well, that was in April. We were doing a number of things. We set about making a better apparatus to make measurements more precisely. We also wrote a letter to the *Physical Review*. . . .

As we expressed it at the time, of course, we identified the magnetic field, it went through a displacement like that you had to identify peak and we had various ways of trying to do that and we located that peak field and then we translated that into frequencies, into Zeeman effect.

From the magnetic field of the peak we could make a calculation and it would tell us what the frequency separation between $2S_{1/2}$ and $2P_{3/2}$ was, and if the $2S$-level was where it was supposed to be according to the Dirac theory, then of course the $2S_{1/2}$ level would be coincident with the $2P_{1/2}$ level and there were good reasons for thinking that we know the space in between $2P_{1/2}$ and $2P_{3/2}$ so that therefore measuring it the transition between $2S_{1/2}$ and $2P_{3/2}$ would give us an idea of whether the $2S_{1/2}$ was at the right position. And it seemed to be off by what we described as 1,000 megacycles. That can be expressed either as wavelength or a frequency can be given in megacycles or in wave numbers. In wave numbers it was about .03. So it was completely clear that the $2S$ state was not where it should have been. Now, as I said, we were using clystrons that were intended for use around 3 centimeters. They were appropriate for the transition $2S_{1/2}$ to $2P_{3/2}$, but very quickly we got hold of much longer wavelength oscillators which worked in the region of maybe 30 or 100 megacycles and then we would look at the transitions between $2S_{1/2}$ and $2P_{1/2}$, and those frequencies were in complete accord with the ones we had obtained in the early work. (Willis Lamb, Interview wtih Jagdish Mehra, 26 March 1988)

[1133b] As Willis Lamb recalled later:

Then there was the Shelter Island Conference to which I was invited early in June [1947]. And from that very quickly came the theoretical explanation that Bethe gave; our paper was published in the August 1, and Bethe arranged that his paper should be published in the August 15 issue. Our paper was received on June 18.

On Sunday morning [27 April 1947] I realized that this experiment was of the Nobel quality. In my book, doing an experiment of Nobel quality is more important than getting the prize. . . . I just knew that the energy levels of hydrogen were not quite what they were predicted by Dirac to be. In fact, there really is not a new law of nature. It was all in the theory to begin with but nobody worked it out. It's a consequence of the quantum theory of radiation and the inverse square law and the quantum mechanics of electrons and protons. (Willis Lamb to Jagdish Mehra, 26 March 1988, in Tucson, Arizona)

Ivar Waller made the presentation speech when Willis Lamb was awarded the Nobel Prize on 10 December 1955. He remarked that 'it does not often happen that experimental discoveries exert an influence on physics as strong and invigorating as did your work. Your work led to the reevaluation and a reshaping of the theory of the interaction of electrons and electromagnetic radiation, thus initiating a development of utmost importance to many of the basic concepts of physics, a development the end of which is not yet in sight.' (*Nobel Foundation*, ed., 1964, p. 285) can) theoretical physicists.[1134] Finally, twenty-five participants met at the Shelter Island Conference, all except Hendrik Kramers coming from the United States. Victor Weisskopf, J. Robert Oppenheimer and Hendrik Kramers had prepared a list of topics to be discussed at the Conference, which they selected from the fields of quantum electrodynamics, the theory of nuclear forces and elementary particle theory, but also from recent experiments. In fact, on the first day, the Conference opened with a presentation of experimental results: A talk given by Willis Lamb was followed by one of Rabi's in the morning, and in the afternoon, Bruno Rossi spoke about some recent cosmic-ray experiments. On 3 June, theoretical talks were given by Hendrik Kramers (on the classical electron theory), by Victor Weisskopf (on quantum electrodynamics), and by Hans Bethe and John von Neumann (on aspects of quantum field theory). In the discussion session on the last day, the problems of meson theory were treated, and as a mark of special interest, Richard Feynman was invited to present his space-time approach to quantum mechanics.

(b) Hans Bethe and the Initial Calculation of the Lamb Shift (1947)

The Shelter Island Conference had an immediate impact on Hans Bethe, who recalled more than twenty years later:

Freeman Dyson wrote to congratulate Willis Lamb on the occasion of his 65th birthday as follows:

> Your work on the hydrogen fine structure led directly to the wave of progress in quantum electrodynamics on which I took a ride to fame and fortune. You did the hard, tedious, exploratory work. Once you had started the wave rolling, the ride for us theorists was easy. And after we had zoomed ashore with our fine, fancy formalisms, you still stayed with your stubborn experiment. For many years thereafter you were at work, carefully coaxing the hydrogen atom to give us the accurate numbers which provided the solid foundations for all our speculations.
>
> Those years, when the Lamb shift was the central theme of physics, were golden years for all the physicists of my generation. You were the first to see that this tiny shift, so elusive and hard to measure, would clarify in a fundamental way our thinking about particles and fields. (Dyson, in ter Haar and Franken, 1978, p. xxxvi)

Willis Lamb has always belonged in the highest rank as both a theoretical and an experimental physicist in the style of Rabi, Fermi, and Bloch. After many years spent at Columbia University, Lamb was appointed Wykeham Professor of Physics at Oxford University, from where he returned to join Stanford University and then as Willard J. Gibbs Professor of Physics at Yale University, then he moved to the University of Arizona, Tucson, where he continued to devote himself to research on a variety of problems; he has especially enjoyed his investigations on the interpretation of quantum mechanics over the years. On the evening of 21 September 1999, Willis Lamb called J.M. and declared: 'I have just completed a paper on *the* interpretation of quantum mechanics! My work in this domain has given me greater pleasure than even the investigations on the fine structure of hydrogen [the Lamb shift, for which he was awarded the Nobel Prize for Physics in 1955]; I am sending you a copy of my latest paper.'

[1134] The genesis and the course of development of the *NAS* conferences on the fundamental questions of physics, held at Shelter Island in 1947, at Pocono Manor in 1948, and at Oldstone-on-Hudson in 1949 has been described in detail in Mehra, 1994, Chapters 11, 12, and 13, and in Schweber, 1994, Chapter 4.

> In 1947, Lamb and Retherford had discovered the shift of the $2S$-state of hydrogen upward in energy. We had a very beautiful conference on Shelter Island in which these and other experimental results were discussed, together with the state of theoretical physics which was plagued by the infinities of self-energy. Of course, people had struggled with self-energy infinities for a long time.... Kramers suggested that what one really ought to do was to renormalize the mass of the electron, taking into account its interaction with its own electromagnetic field. Then only those parts of the self-energy which are not contained in the mass of the particle would be observable and amenable to experimentation.
>
> I found this suggestion very interesting, and thought that it ought to be possible to get Lamb's result by applying the idea of Kramers. So on the train from Shelter Island [actually New York City] to the General Electric Company in Schenectady [where Bethe was going to do some consulting work].... I wrote down some elementary equations of radiation interaction and found out that the effect on the $2S$-state or any state of hydrogen would involve the logarithm of the energy. Inside the logarithm, the numerator was some energy which I did not know, while in the denominator there was something like the binding energy of the electron in hydrogen. So without doing anything about it, this expression would depend only logarithmically on the upper limit of the energy of the interacting quantum. This sounded very hopeful: I had used entirely nonrelativistic theory, and we know that, for instance, the electromagnetic mass diverges linearly if one takes the nonrelativistic theory, but diverges only logarithmically if one takes [the] relativistic theory. (Bethe, 1968b, pp. 14–15)

Hence, Bethe realized that 'it was reasonable to conclude that relativistic theory would gain us one power, which meant that the logarithmic divergence would be replaced by convergence.' By means of certain bold manipulations, which turned out to be justified on closer inspection, he 'got a result of about a thousand megacycles, which was about the right answer' (Bethe, *loc. cit.*, p. 15).

Bethe completed his paper, entitled 'The Electromagnetic Shift of Energy Levels' soon afterward and sent copies of it to selected participants of the Shelter Island Conference; it was received on 27 June by *Physical Review* and published in August (Bethe, 1947). The main idea in Bethe's calculation was to use Kramers's renormalization procedure (although in a quantum, rather than a classical context) for the self-energy of the electron in a nonrelativistic consideration of this problem. (For details, see Mehra, 1994, Chapter 12.) Bethe recalled: 'I also heard of Kramers's renormalization procedure for the first time at that time, namely, the idea that self-energy that you get for a bound electron. So, after Shelter Island I took that famous train ride to Schenectady and tried to write down what this difference of self-energies might be, and it turned out that you could fairly easily subtract one from the other.' (Bethe, 1985; Interview with Jagdish Mehra, 23 February 1988)

For the self-energy W of the bound electron in a quantum state m in the hydrogen atom, Bethe used the standard formula of the ordinary radiation theory:

$$W = -\frac{2e^2}{3\pi\hbar c^3}\int_0^K k\,dk \sum_n |v_{mn}|^2/(E_n - E_m + k), \tag{798}$$

where $k = \hbar\omega$ is the energy of the light-quanta of the radiation field, and v_{mn} are the matrix elements of the velocity of the electron (in the nonrelativistic theory, $\mathbf{v} = \mathbf{p}/m = \nabla$). The sum of Eq. (798) goes over all atomic states n, which have energies E_n, and the integral is over all photon energies from zero up to some maximum value k, which has to be chosen later. In case of the free electron, this self-energy is given by the formula

$$W_0 = -\frac{2e^2}{3\pi\hbar c^3}\int_0^K k\,dk \sum_n |v_{mn}|^2/k. \tag{799}$$

After integration over k and making some manipulations, and using the properties of the hydrogen wave functions, which he knew by heart, Bethe obtained the difference

$$W'_{ns} = \frac{8}{3\pi}\left(\frac{e^2}{\hbar c}\right)^3 \mathrm{Ry}\frac{Z^4}{n^3}\ \ln\frac{K}{(E_n - E_m)_{av}}, \tag{800}$$

where Ry stands for the Rydberg constant, which is the ionization energy of the ground state of hydrogen, 13.6 eV, Ze is the charge of the nucleus, and the average excitation energy $(E_n - E_m)_{av}$ was calculated numerically.

Now, the nonrelativistic result in Eq. (800) was still divergent, but it diverged only logarithmically (instead of linearly), when K goes to infinity, because as a result of the subtraction procedure, what Bethe computed was $W - W_0$, in which the linearly divergent terms in the self-energy of the bound electron and of the free electron cancel each other. Bethe suggested that in the relativistic theory, where the self-energy of the electron is itself only logarithmically divergent, the difference W'_{ns}, which ought to give the Lamb shift, should be finite: 'Since we expect that relativity theory will provide a natural *cutoff* for the frequency K, we shall assume that in [(800)]

$$K = mc^2. \qquad [(801)]$$

... This would set an effective upper limit of the order of mc^2 to the frequencies k of light which effectively contribute to the shift of the level of a bound electron.' (Bethe, 1947, p. 340) Using this value for K, he obtained for the Lamb shift $W'_{ns} = 1040$ megacycles 'in excellent agreement with the observed value of 1000 megacycles. [Thus, Bethe had shown that:] (1) the level shift due to interaction with radiation is a real effect and is of finite magnitude; (2) the effect of the infinite electromagnetic mass of a point electron can be eliminated by proper identification in terms in the Dirac radiation theory; (3) an accurate experimental and theoretical investigation of the level shift may establish relativistic effects (e.g., Dirac hole theory). These effects will be of the order of unity in comparison with the [large] logarithm in equation [(800)].' (Bethe, *loc. cit.*, p. 341)

Bethe completed his calculation of the Lamb shift by 9 June 1947, and sent a preliminary draft of a short paper to those participants at the Shelter Island Conference who were directly interested in the problem of the theoretical calculation of the Lamb shift. In the accompanying cover letter to Oppenheimer, Bethe wrote that the calculation 'does work out,' and further: 'Also, the second term already gives a finite result and is not zero as we thought during the conference. In fact, its logarithmic divergence makes the order of magnitude correct. It also seems that Vicki [Weisskopf] and Schwinger are correct that the hole theory is probably important in order to obtain convergence. Finally, I think it shows that Kramers cannot get the right result by his method.' (Bethe to Oppenheimer, 9 June 1947; Oppenheimer Collection, Library of Congress, Washington, D.C.)

Bethe's objection concerned Kramers's method of modifying the conventional Hamiltonian at the classical level in terms of the experimental mass of the electron. Only then, in Kramers's approach, can one use the perturbation theory without any subtraction procedure. In 1948, Kramers finally arrived at the complete fulfillment of his nonrelativistic program, in which one has no difficulties with the self-energy of the electron, but his numerical results turned out to be quite unsatisfactory because his method did not take into account the relativistic effects and the recoil effects in the interaction of the electron with radiation. Nevertheless, Kramers did not much appreciate Bethe's calculation. His comment was that 'It is difficult to make (Bethe's) argument quite rigorous, but it has certain physical plausibility.' (Kramers, 1956, p. 867) He did not believe that relativity would provide a natural cutoff at mc^2, as in Eq. (801), for the upper limit K of the integral in Eq. (799), and he considered Bethe's treatment as highly arbitrary. However, Bethe's achievement in calculating the Lamb shift was highly appreciated by Weisskopf. He wrote to Bethe that he was:

> quite enthusiastic about the result. It is a very nice way to estimate the effect and it is most encouraging that it comes out just right. I am very pleased to see that Schwinger's and my approach seems to be right after all. Your way of calculating is just an unrelativistic estimate of our effect, as far as I can see.
>
> I am all the more pleased about the result since I tried myself unsuccessfully to estimate the order of magnitude of our expression. I was unable to do this, but I got more and more convinced that the method was sound.
>
> That the $2^2S_{1/2} - 2^2P_{1/2}$ split has something to do with radiation theory and hole theory was proposed by Schwinger and myself for quite some time. We did not do too much about it until shortly before the conference. We then proposed to split an infinite mass term from other terms and get a finite term shift, just as I demonstrated at the conference. Isn't it exactly what you are doing? Your great and everlasting deed is your bright idea to treat this at first unrelativistically. (Weisskopf to Bethe, 17 June 1947; Bethe Papers, Cornell University Archives, Ithaca, New York)

However, years later, Weisskopf also expressed some unhappy feelings concerning Bethe's nonrelativistic calculation of the Lamb shift:

> When he [Hans Bethe] sent me this note [Bethe's draft of his calculation], I was actually really unhappy. First of all, he could have told me [that he was going to do this calculation]. I was interested in the Lamb shift problem even before the war; at that time it was called the Pasternack effect. At the Ann Arbor [University of Michigan] Summer School in 1940 I had a lot of conversations with Kramers, with whom I was very close since the old Copenhagen days. He believed, as did I, that the Pasternack effect was real and he asked me to calculate it. He first brought to me the idea that true enough the self-energy is infinite, but maybe the self-energy difference between a bound and a free electron can be calculated and will be finite, and that [later on, in 1947] should be the Lamb shift. From then on I was sort of living with this problem. During the war I became occupied with other problems [at the Manhattan Project], and the Pasternack problem was put on the back burner. But, after the war, I again wanted to take it up and I definitely knew about the problem when I came to MIT [from Los Alamos after the war]. Then came the Lamb shift, Lamb's observation that Pasternack was right and one even had quantitative results.
>
> Schwinger and I went together on the train to New York [to attend the Shelter Island Conference], and we discussed this problem; we arrived at the conclusion that the nonrelativistic part could be calculated with matrix elements. Then I talked a lot with Hans [Bethe] about where the difficulty lies and that the nonrelativistic part is not so difficult; the difficulty lies in the relativistic region, but I did not know how to do that.
>
> So when he sent me that note [Bethe's preliminary calculation], [I was unhappy] because first of all he could have told me about it, and in some ways my name should have been on that paper. Personally I think that he should have asked me to publish this note together with him.
>
> I could actually have made the calculation myself of what then was the Pasternack effect, already in the early forties. And when Lamb measured the shift accurately, I should have won the Nobel Prize.' (Weisskopf, Interview with Jagdish Mehra, 7 May 1988. See also Weisskopf, 1990, pp. 168–169.)

It was essentially Bethe's nonrelativistic calculation of the Lamb shift that got Richard Feynman started on his relativistic formulation of quantum electrodynamics. Bethe's work also stimulated Bruce French and Victor Weisskopf at MIT and Norman F. Kroll at Columbia.

Victor Weisskopf, with his graduate student Bruce French, sought to turn Bethe's procedure into a fully relativistic scheme. More than a year later, Norman Kroll and Willis Lamb joined forces and 'calculated the electromagnetic shift of the bound electron on the basis of the usual formalism of relativistic quantum electrodynamics and positron theory'; however, they employed noncovariant procedures for calculation, which had been described in Heitler's book on the quantum theory of radiation (Heitler, second edition, 1944); at that time, the relativistically invariant approach to calculate the quantum electrodynamic effects was hardly used, and they obtained 'a finite result of 1051 megacycles per second for the shift $2^2S_{1/2} - 2^2P_{1/2}$ in hydrogen in close agreement with the nonrelativistic calculation of Bethe' (Kroll and Lamb, 1949, p. 388). French and Weisskopf, on the other hand, were rather unlucky; they had made a correct relativistic electrodynamical calculation, but their result did not agree with those of

Feynman and Schwinger, who had made identical mistakes in their respective calculations. When Feynman pointed this out to them, they published their paper which appeared after the one of Kroll and Lamb (French and Weisskopf, 1949). Meanwhile, two other participants of the Shelter Island Conference—Richard Feynman and Julian Schwinger—worked on a new, fully covariant approach to renormalized quantum electrodynamics to replace the 'usual formalism,' as did Sin-itiro Tomonaga far away in Tokyo, Japan.

At the end of the Shelter Island Conference, Oppenheimer and Schwinger took a seaplane from Port Jefferson to Bridgeport, Connecticut, where a connection to modern transportation could again be found, and Schwinger took a plane all the way to Boston to save time because of his approaching wedding. He felt very unwell, and the return from the Shelter Island Conference marked a major change in Schwinger's habits:

> I had been a heavy smoker up to that time, probably due to the baleful influence of Oppenheimer, who set the model for everybody. I reproached myself for following that particular habit. At the Shelter Island Conference I had a severe stomach upset just before leaving, and had wondered whether the wedding ceremony would have to be postponed. Actually I thought that sickness was a secret I kept to myself, but of course I told Clarice [soon to be his wife] later. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

Julian recovered quickly and the wedding took place as planned on 8 June 1947. A few days after their wedding, Julian and Clarice Schwinger left for a two-month-long honeymoon, visiting all the places where Julian had lived: first Chicago, where they met Robert Sachs; then Madison, Wisconsin, and from there to Yellowstone and California; in California, they made two long stops, first at Berkeley and then at Los Angeles, everywhere meeting Julian's old friends and acquaintances. At Berkeley, they of course visited the Oppenheimers at their beautiful house called 'Eagle's Nest.' They had a very pleasant visit in Berkeley, where they also saw Robert and Charlotte Serber, then the next stop was Los Angeles, from where they continued toward Los Alamos. At the Los Alamos National Laboratory, Schwinger was invited to give a talk at eight in the morning! From there, the Schwingers took the southern route home.

(c) The Anomalous Magnetic Moment of the Electron (1947)

Upon his return to Harvard, Norman Ramsey, who had just joined the Harvard faculty, reassured Schwinger that perhaps the electron had an intrinsic magnetic moment that was different from the value predicted by the Dirac equation. As he remarked later: 'Julian really cross-examined me as to whether the hyperfine anomaly was true. He thought he knew how to explain the anomaly in the hyperfine interaction. [Gregory] Breit (1947a, b) had previously pointed out that you could explain the anomaly in the hyperfine interaction by assuming an anomalous magnetic moment of the electron.' Breit had made two mistakes in the first draft of his paper, one of which, that the magnetic moment of the proton was unchanged, was corrected by Ramsey before publication. The other error consisted in effect of treating the anomalous magnetic moment not as that due to a circulating current, but as separated north and south poles. (Norman Ramsey, Interview with K. A. Milton, in Mehra and Milton, 2000, Chapter 7.)

In a note 'On Quantum Electrodynamics and the Magnetic Moment of the Electron,' dated 30 December 1947, Julian Schwinger wrote:

> Attempts to evaluate radiative corrections to electron phenomena have heretofore been beset by divergence difficulties attributable to self-energy and vacuum polarization effects. Electrodynamics unquestionably requires revision at ultra-relativistic energies, but is presumably accurate at moderate relativistic energies. It would be desirable, therefore, to isolate those aspects of the current theory that essentially involve only moderate energies and are thus relatively trustworthy. This goal has been achieved by transforming the Hamiltonian of the current hole theory electrodynamics to exhibit explicitly the logarithmically divergent self-energy of a free electron, which arises from the absorption and emission of light-quanta [which] can be ascribed to the electromagnetic mass, which must be added to the mechanical mass. Indeed the only meaningful statements of the theory involve the combination of masses, which is the experimental mass of a free electron. (Schwinger, 1948a, p. 416)

In a footnote (*), Schwinger referred to Hendrik Kramers's remarks about mass renormalization at the Shelter Island Conference and continued:

> It is important to note that the inclusion of the electromagnetic mass with the mechanical mass does not avoid all divergences; the polarization of the vacuum produces a logarithmically divergent term proportional to the interaction energy of the electron in an external field. However, it has long been recognized that such a term is equivalent to altering the value of the electron charge by a constant factor, only the final value being properly identified with the experimental charge. Thus the interaction between matter and radiation produces a renormalization of the electron charge and mass, all divergences being contained in the renormalization factors. (Schwinger, *loc. cit.*)

A preliminary account of this work was presented by Schwinger at the Tenth Washington Conference on Theoretical Physics (13–15 November 1947),[1135] which was also attended by Richard Feynman, and attracted not only his interest but that of J. Robert Oppenheimer's as well. As Feynman recorded at the time: '[Schwinger's] talk was [indeed] interesting because it got Oppy [Oppenheimer] so excited but I did not have time to understand exactly what Schwinger had done. It

[1135] At that meeting, which was organized by Edward Teller and George Gamow and devoted to 'Gravitation and Electromagnetism' and which was held at George Washington University, Schwinger recalled doing clandestine calculations, in lieu of note-taking, using hydrogenic wave functions to understand the large value of the Bethe logarithm in the Lamb shift, obtaining an estimate within about 10% of the exact value (Schwinger, 1983a, p. 334). Schwinger further recalled that 'I was astonished that Bethe didn't actually do the numbers, because he was perfectly capable of doing it.' (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988) had to do with the electromagnetic self-energy problems. One thing he did point out that was very interesting though, was that the discrepancy in the hyperfine structure of hydrogen noted by Rabi [the anomalous magnetic moment of the electron], can be explained on the same basis as that of electromagnetic self-energy, as can the line shift of Lamb. The rest of the meeting was concerned with gravitation and the curvature of the universe and other problems for which there are very powerful mathematical equations—lots of speculation but very little evidence....' (Feynman to Herbert and Mulaika Corben, 19 November 1947)

As we have reported above, Julian Schwinger sent his first report on renormalized quantum electrodynamics to the *Physical Review* on 30 December 1947. He evaluated, by employing the renormalization of charge and mass, energy of an electron in an external magnetic field, which yielded the predicted additional magnetic moment of magnitude $\frac{\alpha}{2\pi}\mu$ (where $\mu = e/2mc$ is the Bohr magneton and $\alpha = e^2/\hbar c$ is the fine structure constant); that is,

$$\frac{\delta\mu}{\mu} = \left(\frac{1}{2}\pi\right) e^2/\hbar c = 0.001162, \tag{802}$$

which agreed perfectly with the earlier estimates of Nafe, Nelson, and Rabi (1947). Schwinger also pointed out that not only are the hyperfine discrepancies accounted for, but also the more accurate recently available atomic-moment measurements in states of sodium and gallium. Schwinger believed correctly that the simplest example of a radiative correction was that for the energy of an electron in an external magnetic field. The detailed application of the theory showed that the radiative correction to the magnetic interaction energy corresponded to an additional magnetic moment associated with the electron spin, as expressed in Eq. (802). New experimental data confirmed this prediction, as Schwinger wrote:

> Measurements on the hyperfine splitting of the ground states of atomic hydrogen [Nafe, Nelson, and Rabi, 1947; Nagle, Julian, and Zacharias, 1947] have yielded values that are definitely larger than those expected from the directly measured nuclear moments and an electron moment of one Bohr magneton. These discrepancies can be accounted for by a small additional electron spin magnetic moment. [G. Breit (1947a, b)] Recalling that the nuclear moments have been calibrated in terms of the electron moment, we find the additional moment necessary to account for the measured hydrogen and deuterium hyperfine structures to be $\delta\mu/\mu = 0.00126 \pm 0.00019$ and $\delta\mu/\mu = 0.00131 \pm 0.00025$, respectively. These values are not in disagreement with the theoretical prediction. More precise confirmation is provided by measurements of the g values for ${}^2S_{1/2}$, ${}^2P_{1/2}$ and ${}^2P_{3/2}$ states of sodium and gallium. [See Kusch and Foley, 1947, and Foley and Kusch, 1948.] To account for these results, it is necessary to ascribe the following additional magnetic moment to the electron, $\delta\mu/\mu = 0.00118 \pm 0.00003$.
>
> The radiative correction to the energy of an electron in a Coulomb field will produce a shift in the energy levels of hydrogen-like atoms, and modify the scattering of electrons in a Coulomb field. Such energy-level displacements have recently been observed in the fine structures of hydrogen [Lamb and Retherford, 1947], deuterium

> and ionized helium [Mack and Austern, 1947]. The values yielded by our theory differ only slightly from those conjectured by Bethe [1947] on the basis of a nonrelativistic calculation, and are, thus, in good accord with experiment. Finally the finite relativistic correction to the elastic scattering of electrons by a Coulomb field provides a satisfactory termination as a subject that has been beset with much confusion. (Schwinger, 1948a, p. 416)

(For full details of the story of the anomalous magnetic moment of the electron, see Mehra and Milton, 2000, Chapter 7.)

Schwinger stated finally that the fine structure shift of the hydrogen lines, as measured by Lamb and Retherford (1947), also followed from the proposed renormalized scheme and announced 'a [forthcoming] paper dealing with the details of this theory' (Schwinger, 1948a, p. 416).[1136] At the Washington meeting mentioned above, Richard Feynman independently calculated the radiative correction to the gyromagnetic ratio for the electron (which in Dirac's theory is given by $g_S = 2$, but the experimental value obtained by Foley and Kusch, as reported above, was $g_S = 2.000244 \pm 0.00006$). Feynman considered the radiationless scattering of the electron in the external magnetic field and calculated the transition amplitude to the first order of perturbation in the radiative corrections.

As mentioned earlier (in Section IV.5), this problem had been treated already by Sidney Dancoff within the noncovariant perturbation theory of the day. Dancoff's mistake was first established by Koba and Tomonaga (1948) and rediscovered by H. W. Lewis, who found that after mass renormalization the amplitude for radiationless scattering did not contain any ultraviolet divergences, although it was infrared divergent (Lewis, 1948). By using his relativistic cutoff procedure, Feynman calculated the amplitude of the radiationless scattering and obtained the result that the radiative correction to the scattering in any potential is equivalent to the first-order correction in $e^2/\hbar c$ to the potential itself. In terms of the Dirac Hamiltonian, the finite radiative corrections to the radiationless scattering were found by Feynman to be

$$\Delta H_{\mathrm{Dirac}} = \frac{e^2}{2\pi\hbar c}\left(-\frac{\hbar e}{2mc}[B\sigma \cdot \mathbf{B} - i\beta\alpha \cdot \mathbf{E}]\right), \tag{803}$$

[1136] Schwinger was slightly upset by Bethe's publication, as was Victor Weisskopf, who was annoyed with Bethe for single-handedly taking credit for this result (see Mehra, 1994, Chapter 12, p. 226):

> It struck me roughly the same way, but not quite as forcibly as it struck Weisskopf, who I think has been quoted as being rather angry at Bethe's so rapidly stealing the thunder. Because the essence of it was what Weisskopf and I talked about and I think we had a somewhat different version of it, that if one calculated the two energy levels, the S and P levels, and looked at their difference, all the ultraviolet divergences would cancel and one would end up with a finite result. I was not personally upset about it, because to me the challenge was the relativistic calculation, which Bethe did not touch. Clearly a large part of the Lamb shift was nonrelativistic, so my interest shifted to what was clearly a totally relativistic effect, the magnetic moment [of the electron]. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

where **E** is the electric field, **B** is the magnetic field, α and β are the Dirac 4×4 matrices, and σ is the Pauli matrix. Equation (803) showed that the interaction of the electron with radiation changes its magnetic moment by a fraction $\frac{\alpha}{2\pi} = \frac{e^2}{2\pi\hbar c}$, which was first discovered, as mentioned above, by Schwinger in a different way (here, α denotes, as usual, the dimensionless electromagnetic coupling constant, or the fine structure constant). (Schwinger, 1948a; see Mehra and Milton, 2000, Chapter 7)

On his way back to Boston after the Washington conference, Julian Schwinger gave a talk on his calculation of the magnetic moment of the electron at Columbia University. Rabi was overjoyed by Schwinger's visit, but he found it 'very regretful and melancholy' that Julian should spend his days 'in self-imposed exile, in a barren land where fish is consumed as brain food, in large quantities, with results that fall short of highest expectations.' (I. I. Rabi to Julian Schwinger, 12 December 1947, quoted in Mehra and Milton, 2000, p. 225) Rabi also wrote to Hans Bethe: 'It certainly seems very likely that the g-value of the electron is greater than 2 by slightly over 1/10 of 1% and that the Schwinger theory of our hyperfine structure anomaly is as correct as your theory of the Lamb-Retherford effect—God is great.' (I. I. Rabi to H. A. Bethe, 2 December 1947; quoted in Mehra and Milton, *loc. cit.*) Bethe immediately replied to Rabi: 'I have heard about Schwinger's theory and find it very wonderful. Nobody, so far, has been able to give me a complete account of his theory of the hyperfine structure or of the g-factor. But I am sure it is alright. It is certainly wonderful how these experiments of yours have given a completely new slant to a theory and how the theory has blossomed out in a relatively short time. It is as exciting as in the early days of quantum mechanics.' (H. A. Bethe to I. I. Rabi, 4 December 1947; quoted in Mehra and Milton, *loc. cit.*)

Schwinger later rhetorically asked: 'After reporting that finite radiative corrections were attained in both bound-state and scattering calculations, why was I not specific about their precise values?' (Schwinger, 1983a, p. 335) Soon, however, Schwinger himself would give a complete answer publicly. The 1948 New York meeting of the American Physical Society was held from 29 to 31 January 1948, at Columbia University, and Julian Schwinger was invited to give a paper on the recent developments in quantum electrodynamics. Schwinger gave his talk on 31 January, and by popular acclaim, it had to be repeated two more times on the same day, in ever larger lecture halls. He reported his initial results on the Lamb shift and the calculation of the anomalous magnetic moment of the electron; he mentioned some discrepancy between his calculations of the anomalous magnetic moment in the Coulomb field in the atom and the magnetic moment of the free electron, which he had worked out to be $\alpha/2\pi = e^2/2\pi\hbar c$. Feynman, who attended Schwinger's lecture at the APS meeting, mentioned after the lecture that he had computed the same things as Schwinger had done. He confirmed Schwinger's results about the Lamb shift and the magnetic moment of the free electron, but he stressed the point that he had obtained the same result for the magnetic moment of the electron in the atom as for that of the free electron, contrary to Schwinger's result. The reason for this discrepancy [which was, in effect with the second term in Eq. (803)] was that Schwinger's calculation was not relativistically invariant. When the calculation procedure is relativistically invariant, there is no problem in showing that Feynman was right, and the magnetic moment of the electron in the atom also equals $\alpha/2\pi = e^2/2\pi\hbar c$. Thus, the complete covariant result for the magnetic moment is as given by Feynman in Eq. (803).

Many years later, Richard Feynman recalled Schwinger's talk at the APS Meeting and what he had done:

> So I got up after Schwinger's talk and said, "I have computed the same thing, and I agree with Professor Schwinger in all of his results, but that the magnetic moment of the electron is the same in the atom and out of the atom."
>
> I was not showing off, I was just trying to say that there's no problem, for I had done the same thing that he had done, and it had all come out all right. Now, Schwinger was already well known, and many people had not heard of me. Schwinger had done many things, great things, before the war, in the theory of deuteron, scattering of neutrons by helium to polarize neutrons, and other things. People knew Schwinger, but most of them did not know me. I heard later from several people who were at the APS Meeting that I sounded funny to them. "The great Julian Schwinger was talking when this little squirt got up and said, 'I have already done this. Daddy, you're in no trouble at all! Everything will be OK!'" Actually, I was quite surprised when he reported that he got another value for the electron's magnetic moment in the atom. I was trying to tell him that there's no difficulty at all! I had caught up with him, and I knew that everything was fine! (Feynman, Interviews and Conversations with Jagdish Mehra, April 1970 and January 1988)

At the APS Meeting, Schwinger mentioned that there was the covariant method of calculation, but he had not applied it yet, and 'no doubt that these problems in covariance would be resolved with the new formulation,' and he continued: 'That explained why [J. Robert] Oppenheimer then said that "you know, [Sin-itiro] Tomonaga has done this." I said, no, I didn't know that Tomonaga had come up with the same formulation.' (Schwinger in Interviews and Conversations with Jagdish Mehra, March 1988) We shall discuss Tomonaga's work, and Schwinger's reaction to it, later on. As Schwinger recalled further:

> Until the APS Meeting in New York in early 1948, I had not heard the name of Tomonaga as a physicist. Of course, I knew about [Hideki] Yukawa, because since 1935 the idea of mesons—which Yukawa had put forth—had been around. Anyway, Oppenheimer said that this covariant formulation I had written down had already been put forward by Tomonaga [1943/1946]. I said, "That's interesting; I'll have to read the paper." I think Rabi was aware of it, and I have a feeling that Rabi had sent me a copy of Tomonaga's paper; in any event, my attention was directed to it. I did ask Tomonaga [when I first met him] as to whom he had sent his papers, and he told me that he had sent them to Oppenheimer. [Actually, Rabi had been in Japan in 1946, where he met the important physicists and brought back papers of what they were working on.] That 1943 paper of Tomonaga's would have been one of the

> many; everybody then was worrying about corrections to scattering, and I am sure they—the Japanese—were doing scattering. Tomonaga didn't solve the problem by renormalization. He was doing what everybody else was doing: compensating fields, something else that provided a minus sign, what [Abraham] Pais did, for example (Pais, 1946). [Schwinger's] line of development was different. (Schwinger, *loc. cit.*)

Schwinger's paper containing the detailed relativistic theory and the applications (promised in Schwinger, 1948a) had to wait. By the time of the Pocono Conference four months later—this being the second conference on the fundamental problems of physics, which following the Shelter Island Conference in 1947—Schwinger had already constructed a covariant formulation, making the technique underlying this first paper obsolete. As we know, all was not so well, because: 'Finally, the finite radiative correction to the elastic scattering of electrons by the Coulomb field provides a satisfactory termination to a subject that has been beset with much confusion.' This is how Schwinger later referred to Dancoff's incorrect calculation (Dancoff, 1939), to which we have already alluded:

> In 1939 or 1940 Oppenheimer, I presume, suggested to Dancoff that he do a relativistic calculation of the electrodynamic corrections to scattering of an electron by a nucleus. He did that calculation and made a mistake, as a result of which it was not immediately obvious that all the electrodynamic corrections could be explained by uniting an electromagnetic mass with a mechanical mass. History might have been very different if that mistake had not been made. I think the Lamb shift could have been predicted. (Schwinger, Interviews with Jagdish Mehra, March 1988)

It is important to recognize that, of course, Schwinger was well aware of the problems of electrodynamics from his earliest student days.[1137] Moreover, he wrote a paper when he was 16, which he never submitted to a journal, entitled 'On the Interaction of Several Electrons' (Schwinger, 1934, unpublished), which discussed the Møller interaction (Møller, 1931, 1932), based on the Dirac–Fock–Podolsky electrodynamics (1932); Schwinger's first effort was noteworthy for the introduction of the interaction representation (see Schwinger, 1983a, pp. 329–331). Later, when he went to Berkeley, he discovered that Oppenheimer was obsessed with the subject. Indeed, Oppenheimer and Schwinger wrote a joint paper on 'Pair Emission in the Proton Bombardment of Fluorine' (1939), where the explanation of the observed effect turned out to be the existence of vacuum polarization, the virtual creation, for short periods of time, of electron–positron pairs (although, to Schwinger's annoyance, Oppenheimer insisted on inserting remarks about a possible—nonexistent—nonelectromagnetic coupling between electrons and nuclear particles). Thus, Schwinger began with an advantage over Feynman,

[1137] Schwinger's student notebooks at City College already contained detailed notes of major papers on field theory by Dirac, Heisenberg, Pauli, and Weisskopf from the 1920s and 1930s. (See the Julian Schwinger Papers—Collection 371—Department of Special Collections, University Research Library, University of California, Los Angeles.)

who failed to recognize the reality of vacuum polarization for the first few years of the development of quantum electrodynamics.

Crucial for Schwinger's stunning progress in quantum electrodynamics after the war was his development of electromagnetic theory at the MIT Radiation Laboratory and, in particular, his perfection of the theory of synchrotron radiation, immediately after the end of the war. As he recalled in 1980:

> What was significant was the radiation emitted by relativistic electrons moving in circular paths under magnetic field guidance. It is an old problem, but the quantitative implication of relativistic energies had not been appreciated. In attacking this classical relativistic situation, I used the invariant proper-time formulation of a charge. That self-action contained a resistive part and a reactive part, to use the engineering language I had learned. The reactive part was the electromagnetic mass effect, here automatically providing an invariant supplement to the mechanical action and thereby introducing the physical mass of the charge. Incidentally, in the paper on synchrotron radiation that was published several years later, a more elementary expression is used, and the reactive effect is dismissed as "an inertial effect with which we are not concerned" [Schwinger, 1949]. But here was my reminder that electromagnetic self-action, physically necessary in one context, was not to be, and need not be, omitted in another context. And in arriving at a relativistically invariant result, in a subject where relativistic invariance was notoriously difficult to maintain, I had learned a simple but useful lesson: to emerge with relativistically invariant physical conclusions, use a covariantly formulated theory, and maintain covariance throughout the calculation. (Schwinger, Interviews with Jagdish Mehra, March 1988)

Hendrik Kramers is usually mentioned as the father of the concept of renormalization. Yet his approach was classical. In a book review, Schwinger summarized his position on Kramers: 'It is a common mistake to think that Kramers had anticipated post-war mass renormalization. His idea was to begin with the classical nonrelativistic Hamiltonian expansion in terms of the physical mass, and quantize it. But the relativistic effects change the nature of this self-mass.' (Julian Schwinger Papers, University of California, Los Angeles)[1138]

[1138] Schwinger commented later:

> 'Kramers wrote a book on quantum mechanics [Kramers, 1938a] in which he goes through some pedestrian development and, I believe, points out the infinite self-energy and then says that clearly we have quantized the wrong classical theory. The correct classical theory should already have removed from it this deficiency of classical electromagnetism, namely the infinite mass of a point charge. And when you corrected the classical theory, then that is the proper thing to quantize.' But this approach to mass renormalization does not work, 'Because you cannot find a classical theory on which you can superimpose phenomena like pair creation and other things which are necessary and part of the relativistic quantum electrodynamics. It is a dead end. Nevertheless, it looks superficially as though Kramers invented mass renormalization.' (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

What Schwinger meant was his renormalization scheme of relativistic quantum electrodynamics did not work in the case of relativistic classical electrodynamics (with linear divergences, etc.)

(d) The Pocono Conference (1948)

From 30 March to 2 April 1948, the second conference on the problems of fundamental physics was held at the Pocono Manor Inn, located approximately midway between Scranton, Pennsylvania, and the Delaware Water Gap. Pocono Manor offered the same kind of undisturbed setting as had Ram's Head Inn on Shelter Island. Twenty-eight physicists participated. Kramers, MacInnes, Nordsieck, Pauling, and Van Vleck, who had attended the Shelter Island Conference, were absent. The new participants were Niels and Aage Bohr, Eugene Wigner, Gregor Wentzel, Paul Dirac, and Walter Heitler.

The Pocono Conference was Julian Schwinger's first opportunity to learn what Richard Feynman was doing with quantum electrodynamics; earlier he had only seen his work with John A. Wheeler on classical electrodynamics, and the idea of abolishing the electromagnetic field, in a fundamental sense, did not appeal to Schwinger at all (Wheeler and Feynman, 1945). But by the time of the Pocono Conference, Feynman had reworked almost all of quantum electrodynamics by his new technique of space-time diagrams. He had reached the most important part of his new results: namely, the relativistic formulation of quantum electrodynamics and, especially, of perturbation theory, the relativistic cutoff and the renormalization of mass, closed expressions for the transition of amplitude and causal propagators, a new operator calculus, rules for the calculation of the contribution to the transition amplitude in each order of perturbation theory, and the idea of corresponding visualization of these rules by diagrams. He had calculated the Lamb shift and the anomalous magnetic moment of the electron, and cross sections for different processes. However, before the Pocono Conference, Feynman had not published anything on quantum electrodynamics and he did not have the mathematical proofs of all of his results. We shall discuss Feynman's work on various aspects of his space-time approach to quantum electrodynamics and its mathematical formulation a little later. These were the investigations which he had completed during the period between the Shelter Island and Pocono Conferences, but published only during the years between the Pocono and the Oldstone Conferences.

At the Pocono Conference, Julian Schwinger gave a marathon lecture on his version of quantum electrodynamics; his scheme was rooted in the earlier work of Dirac, Fock, and Podolsky (1932), and Schwinger proceeded to present a systematic approach based on a series of canonical transformations. This covariant approach was published as the series 'Quantum Electrodynamics' (Schwinger 1948c; 1949a, c). He gave an exact calculation of the Lamb shift and the anomalous magnetic moment of the electron on the basis of his methods, which he described in detail. As Feynman recalled at Schwinger's sixtieth birthday celebration in 1978:

> Each of us had worked out quantum electrodynamics and we were going to describe it to the tigers. He [Schwinger] described his in the morning, first, and then gave one of those lectures which are intimidating. They are so perfect that you don't want to ask any questions because it might interrupt the train of thought. But the people in

> the audience like Bohr, and Dirac, Teller, and so forth, were not to be intimidated, so after a bit there were some questions. A slight disorganization, a mumbling, confusion. It was difficult. We didn't understand everything, you know. But after a while ... he would say, "perhaps it will become clearer if I proceed," so he continued this, continued it.... (Feynman, 1989, pp. 91–93; Schwinger's lecture lasted well into the afternoon.)

Notes based on Schwinger's lectures, as well as those of the other speakers at the Pocono Conference, were widely circulated, and are still prized possessions of many physicists (see the copy of the Pocono lectures on deposit at the AIP Niels Bohr Library). The notes taken of Schwinger's presentation by John Wheeler, consisting of some 37 pages, have been recounted in some detail in Schweber's book (1994), and so we will concentrate on the high points. Schwinger's idea was that to identify the infinite terms one had to have a treatment which was both gauge and relativistically invariant. He began by introducing propagation functions defined in terms of commutators, both for the photon field A_μ and the matter field ψ. In terms of the latter, Schwinger wrote the Schrödinger equation including the interaction; in general, with free Hamiltonians H_1 and H_2 and the interaction Hamiltonian H_{12}, that equation is

$$i\hbar\frac{\partial}{\partial t}\Psi(t) = (H_1 + H_2 + H_{12})\Psi(t), \tag{804}$$

where the operators are time-independent. But now, 'following Dirac and Tomonaga, we make a contact transformation,'

$$\Psi_{\text{New}} = e^{(i/\hbar)(H_1+H_2)t}\Psi_{\text{Old}}, \tag{805}$$

which gives rise to the (later, so-called interaction representation) in which both the operators and the wave function are time-dependent

$$i\hbar\frac{\partial}{\partial t}\Psi_{\text{New}}(t) = H_{12}(t)\Psi_{\text{New}}(t), \tag{806}$$

where

$$H_{12}(t) = e^{(i/\hbar)H_0 t}H_{12}e^{(-i/\hbar)H_0 t}, \quad \text{with } H_0 = H_1 + H_2. \tag{806a}$$

This, of course, was not a completely covariant formulation. The 'interaction picture' (in the) Schrödinger equation may be 'regarded as the result of setting times equal in an infinite set of equations of the many-time formalism.' Thus, Schwinger generalized by introducing a time for each point of a space-like hypersurface, $\sigma(x)$, the generalization process indicated by

$$\Psi(t)_{\text{Dirac}} \to \Psi(t_1, t_2 \ldots)_{\text{Dirac–Tomonaga}} \to \Psi(t(x)). \tag{807}$$

Now, he introduced a Hamiltonian density $\mathscr{H}$, and obtained the Tomonaga–Schwinger functional equation

$$i\hbar c \frac{\delta\Psi(\sigma)}{\delta\sigma(\mathrm{x})} = \mathscr{H}(x)\Psi(\sigma). \tag{808}$$

For the case of electrodynamics, the Hamiltonian density is $\mathscr{H} = -\frac{1}{c} j_\mu A_\mu$. A supplementary condition had to be satisfied as well,

$$\Omega\Psi(\sigma) = 0, \tag{809}$$

where

$$\Omega = \frac{\partial A_\mu}{\partial x_\mu}(x) + \frac{1}{ic}\int_\sigma D(x - x')j_\mu(x')\, d\sigma_\mu, \tag{809a}$$

and D is defined by the commutator,

$$[A_\mu(x), A_\nu(x')] = \frac{\hbar c}{i}\delta_{\mu\nu}D(x - x'). \tag{809b}$$

Schwinger showed that this condition was consistent, in that it held at all points,

$$\frac{\delta}{\delta\sigma(x)}[\Omega\Psi(\sigma)] = 0. \tag{810}$$

The person who took notes of Schwinger's lectures then stated that 'these equations contain nothing more than the Heisenberg-Pauli formalism and would not be required if one knew how to carry out *H-P* calculations consistently. One can get back to the Dirac many-time formalism by putting a suitable number of delta functions in the current.' Schwinger concluded this part of his lecture by showing that the theory is gauge invariant.

Schwinger went on to treat perturbation theory up to order e^2. He did this by making another contact transformation,

$$\Psi \to \exp[-iS(\sigma)\Psi(\sigma)], \tag{811}$$

where

$$S(\sigma) = \frac{1}{\hbar c}\int_{\pm\infty}^{\sigma} \mathscr{H}(x')\, d\omega'. \tag{812}$$

[The volume element is denoted by $d\omega'$.] To this order, then, the equation of motion reduced to

$$\hbar c \frac{\delta\Psi}{\delta\sigma} \approx \frac{1}{2}[S(\sigma), \mathscr{H}(x)]\Psi, \tag{813}$$

and the supplementary condition to

$$\frac{\partial}{\partial x_\mu} A_\mu \Psi = 0. \tag{814}$$

By writing Eq. (813), that was to be solved, as

$$i\hbar c \frac{\delta \Psi}{\delta c} = \mathscr{H}' \Psi, \tag{815}$$

with

$$\mathscr{H}' = \frac{i}{2}[S, \mathscr{H}] = \frac{i}{2\hbar c^3} \int_{\pm\infty}^{\sigma} [j_\mu(x') A_\mu(x'), j_\mu(x) A_\nu(x)] d\omega', \tag{816}$$

Schwinger then broke this interaction into two parts, one of which described the Møller interaction (when two particles are present) and the self-energy (if only one particle is present), while the second 'accounts for virtual pair production, self-energy of the photon, and real interactions between light-quanta and electrons.' At this point, Schwinger remarked that this treatment could be extended to processes involving arbitrarily many electrons, but Niels Bohr objected that 'one may not be able to treat all physical problems without a fundamentally new idea.'

He then treated the photon self-energy, and showed that it could be rendered finite and, therefore, necessarily zero. The electron self-energy is logarithmically divergent, but *independent of the state of motion of the electron*. Schwinger obtained the same result as Weisskopf had earlier, apart from some numerical errors in the paper:

$$\delta mc^2 = \frac{3}{2\pi} \alpha mc^2 \left[\ln \frac{2}{mc\sqrt{\varepsilon}} - \frac{1}{2}\gamma - \frac{1}{6} \right], \tag{817}$$

where $\varepsilon \to 0$ is the lower limit of integration, and $\gamma = 0.577\ldots$ is Euler's constant. Finally, he turned to electrons moving in given external fields. Again, using a method of canonical transformation, he arrived at the following relativistic formula for the Lamb shift, that is the ${}^2S_{1/2} -{}^2 P_{1/2}$ splitting in hydrogen:

$$\Delta E \propto \left[\ln \frac{mc^2}{\Delta W} - W - \ln 2 + \frac{3}{8} + \frac{1}{8} \right] \frac{8\alpha}{9\pi}, \tag{818}$$

where the logarithmic term is that obtained by Bethe (1947, p. 341), with Ry the ionization energy of the ground state of hydrogen $K \approx mc^2$,

$$W'_{ns} = \frac{8}{3\pi} \left(\frac{e^2}{\hbar c} \right)^3 \mathrm{Ry} \frac{Z^4}{n^3} \ln \frac{K}{\langle E_n - E_m \rangle_{\mathrm{Av}}}. \tag{819}$$

(The 1/8 in Eq. (818) was probably a transcription error by the notetaker; it should be 1/2 coming from the anomalous magnetic moment coupling. Apparently the vacuum polarization contribution, which gives a term of $-1/5$, is not included here.)

The impact of Schwinger's lecture at Pocono spread far and wide. Chen Ning Yang recalled:

> I did not make it to the meeting. I was just a graduate student. From Chicago, Fermi, Teller, and Wentzel went. Fermi usually did not takes when he went to a conference. But this time, he took voluminous notes because he was aware that it was a historical event to listen to what Schwinger had to say. After they came back to Chicago, there was the question of how to digest these notes. Fermi gathered Teller and Wentzel and four graduate students, viz., Geoffrey Chew, Murph Goldberger, Marshall Rosenbluth, and myself, into his office, and we spent weeks trying to digest what Fermi had written down as what Schwinger had said. This lasted from April to May, 1948. Murph [Goldberger] kept notes. I still have a copy of these; it totals 49 pages. After about six weeks of meeting several times a week in Fermi's office for something like two hours each session we were all very tired, and none of us felt that we had understood what Schwinger had done. We only knew that Schwinger had done something brilliant, namely he had produced this $(\alpha/2\pi)$ and he was already into the calculations of the Lamb shift.
>
> At the end of our six weeks of work, somebody asked, "Wasn't it true that Feynman also talked?" All three said, "Yes, yes, Feynman did talk." "What did he say?" None of them could say. All they remembered was Feynman's strange notation: *p* with a slash through it. (Yang, 1996, p. 176)

All those present at the Pocono Conference—including the new participants—were deeply impressed by Schwinger's ideas and talk. Afterward, Feynman gave his lecture, entitled 'Alternative Formulation of Quantum Mechanics.' Later on, he recalled his procedure and the response of the participants in greater detail:

> This meeting at Pocono was very exciting, because Schwinger was going to tell how he did things and I was to explain mine. I was very nervous there and didn't sleep at all, I don't know why. But the meeting was very exciting. Schwinger and I would talk to each other, and we would compare notes as to our respective results. He would tell me where his terms came from, and I would tell him my result for the same; we did not know how each of us had done it, but we agreed on the answer. We would talk about the physical ideas, and see what the result of our respective calculations was. We could talk back and forth, without going into details, but nobody there understood either of us. But Schwinger and I could talk back and forth with each other. When he tried to explain his theory, he encountered great difficulty. Now and then he would remark: "Well, let's look at it physically." As soon as he would try to explain the ideas physically, the wolves would descend on him, he had great difficulty. Also, people were getting more and more tired.
>
> Taking a cue from the response that Schwinger got, Bethe said to me: "You should better explain things mathematically and not physically, because every time Schwinger tries to talk physically he gets into trouble." Now the problem for me was that all my thinking was physical. I did things by cut and try methods, which I had

myself invented. I didn't have a mathematical scheme to talk about. Actually I had discovered one mathematical expression, from which all my diagrams, rules and formulas would come out. The only way I knew that one of my formulas worked was when I got the right result from it. So, in a sense, I did have a mathematical scheme, but it was not organized in a way that I could explain it in terms that would be familiar to other people; it could not be put into any familiar mathematical language. My way of looking at things was completely new, and I could not deduce it from other known mathematical schemes, but I knew what I had done was right.

So, following Bethe's advice, I said in my talk: "This is my mathematical formula, and I'll show you that it produces all the results of quantum electrodynamics." Immediately I was asked: "Where does the formula come from?" I said, "It doesn't matter where it comes from; it works, it's the right formula!" "How do you know it's the right formula?" "Because it works, it gives the right results!" "How do you know it gives the right answers?" "It will become evident from what I do with it. I'll show you how the formula works and I'll do one problem after another with its help." So I tried to explain the meaning of the symbols I had employed, and I applied it to solve the problem of the self-energy of the electron. They got bored when I tried to go into the details. Then Bethe tried to help me by asking: "Don't worry about the details, explain to us how the formula works," and so on. Question: "What made you think that the formula was right in the first place?" Then I tried to go into the physical ideas. I got deeper and deeper into difficulties, everything became chaotic. I tried to explain the tricks I had employed. For instance, take the exclusion principle, which says that you can't have two electrons in the same state; it turns out that you don't have to pay much attention to that in the intermediate states in perturbation theory. I had discovered from empirical rules that if you don't pay attention to it, you get the right answers anyway, and if you do pay attention to it then you have to worry about this and that.

Then they asked: "But what about the exclusion principle?" "It doesn't make any difference in the intermediate states!" Then Teller asked: "How do you know?" "I know because I have worked it out!" Then Teller said: "How could that be? It is fundamentally wrong that you don't have to take the exclusion principle into account." I replied: "We'll see that later."

Already in the beginning I had said that I'll deal with single electrons, and I was going to describe this idea about a positron being an electron going backward in time, and Dirac asked, "Is it unitary?" I said, "Let me try to explain how it works, and you can tell me whether it is unitary or not!" I didn't even know then what "unitary" meant. So I proceeded further a bit, and Dirac repeated his question: "Is it unitary?" So I finally said: "Is what unitary?" Dirac said: "The matrix which carries you from the present to the future position." I said, "I haven't got any matrix which carries me from the present to the future position. I go forwards and backwards in time, so I don't know what the answer to your question is."

Every one of these people had something in mind, and they acted as if I should know what they thought. Dirac had proved somewhere that in quantum mechanics, since you progress only forwards in time, you have to have a unitary operator. But there is no unitary way of dealing with a single electron. Dirac could not think of going forwards and backwards, and he wanted to know whether the theorem concerning unitarity applied to it. Each one of them, for different reasons, thought that there were too many gimmicks in what I was doing, and it proved to be impossible to tell them that you could actually go ahead with what I was doing.

[Niels] Bohr was also at the meeting. After I had tried many times to explain what I was doing and didn't succeed, I talked about trajectories, then I would swing back—I was being forced back all the time. I said that in quantum mechanics one could describe the amplitude of each particle in such and such a way. Bohr got up and said: "Already in 1925, 1926, we knew that the classical idea of a trajectory or a path is not legitimate in quantum mechanics; one could not talk about the trajectory of an electron in the atom, because it was something not observable." In other words he was telling me about the uncertainty principle. It became clear to me that there was no communication between what I was trying to say and what they were thinking. Bohr thought that I didn't know the uncertainty principle, and was actually not doing quantum mechanics right either. He didn't understand at all what I was saying. I got a terrible feeling of resignation. I said to myself, I'll just have to write it all down and publish it, so that they can read it and study it, because I know it's right! That's all there is to it.

Of course, there was not personal criticism in all this, no personal antagonism. Dirac was mumbling, "Is it unitary?" Teller was excited about the exclusion principle and the proper use of quantum mechanics—well, it didn't make me angry, it just made me realize that he [Bohr] didn't know what I was talking about, and it was hopeless to try to explain it further. I gave up, I simply gave up, and decided to publish my work because I knew it was all right.

Obviously, I had started backwards and I hadn't explained my ideas rightly in the first place; everything was tumbled around, and all the places were out of joint. I was trying to explain the pieces of the puzzle rather than explaining the pattern. However, with regard to Schwinger things were different. In the lunch periods, and at other times outside the meeting and discussions, he and I would compare notes on formulas for special problems, and see that both of us had the same results. We knew where everything came from and we both knew that each of us was right, that we were both respectable. I could trust him, and he could trust me. We came at things entirely differently, but we came to the same end. So there was no problem with my believing that I was right and everything was OK. That I did not explain things properly is correct, but the rumors that I was depressed were not quite true; I just felt that there had been no communication. (Feynman, Interviews and Conversations with Jagdish Mehra, April 1970, January 1988)

(e) Vacuum Polarization (1948)

For Feynman, vacuum polarization remained the main unresolved problem of quantum electrodynamics in the spring of 1948. Feynman recalled the situation at the time of the Pocono Conference at the end of March 1948 as follows: 'When it was my turn to talk, I began by saying, "I can do everything but I can't do closed loops, the self-energy of the photon." Schwinger immediately got up and said, "I can do everything including vacuum polarization." And he worked something out; he got a term which looked something like vacuum polarization, and was able to treat it.... Actually, I had everything, too, only it took me just a little longer to realize that I had it.' (Feynman, Interviews and Conversations with Jagdish Mehra, January 1988) Schwinger, on the other hand, maintained that:

as for vacuum polarization, he [Feynman] did not have it. He simply did not have it and [there is] nothing to be said about it. Obviously, I had it. If one could discover the [actual] notes of my lecture at Pocono, one would see that when I talk about the Lamb shift I give specific contributions to the various pieces and there is a $-\frac{1}{5}$ that's vacuum polarization.[1139] Remember my 1939 work [with Oppenheimer] in which an excited fluorine atom emits an electron and a positron, that's vacuum polarization too [Oppenheimer and Schwinger, 1939]. And so I was not likely to leave it out. It was very important psychologically, because I had known it for many years. Now Feynman often said that in contrast with other people who write down equations and solve them, I write down solutions, this is what puts people off. How do you know the solutions? Of course, if you write down the solutions, then you're doing it piece by piece, you have no general theory to refer to, and you realize that vacuum polarization has difficulties; therefore you leave it out. This was the problem, and it went back to his [Feynman's] attitude. He thought that he rendered his theory finite by putting in a form factor between the coupling of the charges and the electromagnetic field, and if you do that then you would get a finite electron mass and so forth. So he didn't know what to do with vacuum polarization and said, well, maybe it isn't there.

He argued further:

Vacuum polarization [originally was] a phenomenon [in which] out of the decaying nucleus there comes an electron-positron pair. Vacuum polarization is just a handy word for meaning that there are phenomena in which electron-positron pairs are created; it is a catchword to indicate that class of phenomena and you can't get rid of it. It does not mean more than the fact that an electron-positron combination is coupled to the electromagnetic field and may show itself really or virtually..... I put the vacuum polarization in because it was there; Feynman found difficulty with it in his formulation and therefore speculated that it was not to be included. When the experiments had advanced to a greater level of accuracy, such as the Lamb shift, then there was no doubt that vacuum polarization was there, that it was a real phenomenon, and it had to be included. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988.)

Finally, he emphasized that:

the subject of vacuum polarization is a point on which, throughout [the] 1948 period and beyond, Feynman and I disagreed, a point not of individual mathematical style but of fundamental physics. [As} Bethe said, "the polarization of the vacuum is consciously omitted in Feynman's theory." [Bethe, unpublished report to the eighth Solvay Conference, 1948, quoted in Oppenheimer, 1950, p. 281] The reasoning went this way: A modification of the electromagnetic interaction made the electromagnetic mass [of the electron] finite but did nothing for the apparently more severely divergent—here it is again—photon mass. Therefore, things would be simpler if all such effects (as closed loops, in Feynman's graphical, acausal language) were omitted. But I knew that the virtual photon emitted by the excited oxygen [or

[1139] In fact, that particular problem did not appear explicitly in the rather sketchy extant notes from Pocono, although vacuum polarization is treated differently. (See the copy of the Pocono lectures on deposit in the AIP Niels Bohr Library.)

> fluorine] nucleus created an electron-positron pair; the vacuum is polarizable. In a later paper [Schwinger 1951b] I would use this very example to illustrate a manifestly gauge-invariant treatment of the problem. (Schwinger, *loc. cit.*)

In contrast to Schwinger's opinion, Feynman recalled that he and Schwinger

> got together in the hallway and although we'd come from the ends of the earth with different ideas, we had climbed the same mountain from different sides and we could check each other's equations.... Our methods were entirely different. I didn't understand about those creation and annihilation operators. I didn't know how these operators that he [Schwinger] was using worked, and I had some magic from his point of view. We compared our results because we worked out problems and we looked at the answers and kind of half described how the terms came. He would say, "Well, I got a creation and then annihilation of the same photon and then the potential goes...." "Oh, I think that might be that," and I'd draw a picture. He didn't understand my pictures and I didn't understand his operators, but the terms corresponded and by looking at the equations we could tell, and so I knew, in spite of being refused admission by the rest, by conversation with Schwinger, that we both had come to the same mountain and that it was a real thing and everything was all right. (Feynman, 1989, pp. 91–93)

The discussion between Feynman and Schwinger continued after the Pocono Conference. In fact, several weeks after the conference, they discussed these problems during Feynman's visit to MIT (Feynman, Interviews and Conversations with Jagdish Mehra, April 1970). In particular, Feynman recalled after decades:

> We discussed matters at Pocono and later also over the telephone and compared results. We did not understand each other's method but trusted each other to make sense—even when others did not trust us. We could compare final quantities and vaguely see in our own way where the other fellow's terms or error came from. We helped each other in several ways. For example, he showed me a trick for integrals that led to my parameter trick, and I suggested to him that only one complex propagator function ever appeared rather than his two separate real functions. Many people joked we were competitors—but I don't remember feeling that way. (Feynman, Interview with S. S. Schweber, quoted in Schweber, 1994, p. 444)

(f) The Michigan Summer School: Freeman Dyson at Julian Schwinger's Lectures (1948)

At the Summer Symposium at the University of Michigan (in Ann Arbor) in July, Schwinger presented lectures, whose content was largely that given in the 'Quantum Electrodynamics' papers which we shall describe later on: The covariant formulation of quantum electrodynamics, described by Schwinger already in the Pocono lectures, ended like those lectures by computing the Lamb shift:

$$\Delta E \propto \ln - \ln 2 + \frac{3}{8} - \frac{1}{5} + \frac{1}{2}, \tag{820}$$

where ln stands for the Bethe logarithmic term, $-\frac{1}{5}$ for the vacuum polarization, and $\frac{1}{2}$ for the magnetic moment effect, which is not correctly incorporated. As we know, the correct French–Weisskopf calculation would replace the 3/8 by 5/6; and a few months later, Schwinger would discover his error.

His lectures were attended by a newcomer from England, who soon became deeply involved in the theory of quantum electrodynamics. Freeman J. Dyson, as a young, brilliant, 24-year old Winchester- and Cambridge-trained mathematician, had come to study theoretical physics under Hans A. Bethe at Cornell University, Ithaca, New York (with the advice of the Cambridge hydrodynamicist Sir Geoffrey Ingram Taylor and the support of a Commonwealth Fund Fellowship grant), where—with Geoffrey Taylor's recommendation addressed directly to Bethe (see Schweber, 1994, pp. 490–493)—he was immediately accepted as his regular graduate student and a doctoral committee consisting of Bethe (chairman) and Robert R. Wilson was assigned to monitor his progress. Dyson enrolled in Bethe's course on 'Advanced Quantum Mechanics' and attended Wilson's lectures on 'Experimental Nuclear Physics,' as well as a course on the theory of solids, which was taught by an instructor named Smith. He was aware of Bethe's calculation of the level shift in hydrogen in a simplified model, in which relativity and the spin of the electron had been ignored; Bethe had turned the resulting infinite answer into a plausible finite one which agreed with experiment, and he handed over the problem of the exact calculation to Richard Scalettar, one of his graduate students, just a couple of days before Dyson arrived at Cornell. Hence, Bethe assigned him an interim problem to work out the calculation of the Lamb shift for a spin zero electron by using the correct relativistic wave equations; all Dyson had to do was to take Bethe's nonrelativistic calculation and repeat it by using relativistic electrodynamics and doing the mass renormalization a little bit more carefully. It was Dyson's first research problem in physics; of course, there was no experiment to compare it with since there were no spin-zero electrons (however, the spin corrections are small): His paper on 'The Electromagnetic Shift of Energy Levels' containing the results appeared in the 15 March 1948, issue of the *Physical Review* (Dyson, 1948); there, he made full use of his knowledge of the quantum theory of fields that he had gained from Wentzel's book on the quantum theory of wave fields (Wentzel, 1943), which he had studied earlier with Nicholas Kemmer in Cambridge. A few weeks earlier, at the January 1948 APS Meeting in New York, Freeman Dyson also reported on his work on the electromagnetic shift to the spinless electron, and Schwinger recalled that he 'turned to whoever was sitting next to me, saying why on earth would anybody spend time doing that, since there was no real application. So I had seen Dyson, but my initial impression was a little strange.' (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

After the Shelter Island Conference, Richard Feynman began to perfect his space-time approach to quantum electrodynamics, in which the path-integral formulation of nonrelativistic quantum mechanics (Feynman, 1948a) played a fundamental role. During the academic year 1947–1948, Feynman often discussed his methods with anyone who would listen. One attentive listener was Freeman Dyson, who interacted with Feynman, as he recalled:

> mostly by listening. At that time [Feynman] was working extremely hard to develop his version of quantum electrodynamics; it was still not finished. He had the relativistic cutoff and he knew how to deal with positrons, pair creation and closed loops by means of diagrams. But he hadn't yet got it all together into a workable scheme. It was still something that only he knew about how to do, and he had problems communicating with other people. He had these ideas that were so different from the conventional ones. I listened a great deal to him and I was convinced that he had something valuable, but that it needed to be understood. That was one of the things I set out to do. During that year I spent a fair amount of time just listening to Feynman talk about all kinds of things. I was in Cornell just nine months, from September to June; during that time I picked up everything from Feynman. (Dyson, Interviews and Conversations with Jagdish Mehra, 4 November 1971, and 25 February 1987)

But Dyson did not complete his Ph.D., and later on life, he turned completely against the American Ph.D.-granting system! It was in the early summer of 1948 that Richard Feynman travelled by car to Albuquerque, New Mexico. On a completely unplanned trip, Freeman Dyson drove with him from Cleveland to Albuquerque for three or four days, 'and that was the time when I really got to talk to Feynman—twenty-four hours at a stretch. We talked about everything: his theory and his whole approach to life and physics.' (Dyson, *loc. cit.*) Dyson had been aware of Feynman's approach to quantum electrodynamics since September 1947, when he arrived at Cornell from Cambridge, England, to work with Hans Bethe as a graduate student.

After Feynman and Dyson's joint automobile trip West from Cleveland to Albuquerque, Dyson returned to Ann Arbor, Michigan, and from 19 July to 7 August 1948, he spent a period of three weeks at the University of Michigan Summer School, where Julian Schwinger gave 'his very polished lectures describing his way of doing the Lamb shift and his version of recent developments in quantum electrodynamics.' (Dyson, *loc. cit.*) As Schwinger reported later:

> It seems that I supplied the notes for the first part of the course, which must have been the manuscript for the paper received by the *Physical Review* on July 29 [Schwinger, 1948c]. The notes for the second part were taken by David Park. I have read … words to the effect that what I presented there was like a cut and polished diamond, with all the rough edges removed, brilliant and dazzling. Or, if you don't care for that simile, you can have "a marvel of polished elegance, like a violin sonata played by a virtuoso—more technique than music." I gather I stand accused of presenting a finished elaborate formalism from which had been excised all the physical insights that provide signposts to its construction.… The paper to which I have referred [1948c] has a long historical and physical introduction that motivates the development and sets out the goals of relativistic renormalization theory. Beyond that, the lectures presented the explicit working out of the interaction of a nonrelativistic electron with the radiation field, in the dipole approximation. The canonical transformation that isolates the electromagnetic mass is an elementary one, and the further

details leading to the solution of the bound state and scattering problems were provided. This was the simple model on which the relativistic theory was erected. It was good enough for the immediate purposes but ... still quite primitive. I needed no one to tell me that it was but a first step to an aesthetically satisfactory and effective relativistic theory of coupled fields. (Schwinger, 1983a, p. 341)

And Dyson recalled that Schwinger's lectures were in the mornings, and:

I sat in the afternoons working through them, calculating myself and reproducing what he had done, and at Ann Arbor I had very close contact with Schwinger. So I understood it, so to say, from the inside. The methods of Schwinger and Feynman led to the same results, but it was not at all clear why, because they looked so different. Also Tomonaga was doing essentially the same thing that Schwinger was doing, only Tomonaga explained things in a much less elaborate fashion so that it was easier to understand, but he did not go so much into detail. But Tomonaga's way and Schwinger's way were essentially the same, but Feynman's was totally different. He didn't even write down a Hamiltonian or anything; he just wrote down the answer, just gave you a set of rules for writing down the answer. (Dyson, Interviews and Conversations with Jagdish Mehra, 4 November 1971, and 25 February 1987)

(g) The Immediate Impact of Schwinger's Lectures (1948)

Freeman Dyson never forgot his first experience with Julian Schwinger in Ann Arbor. He first met Schwinger at 'the summer school in Michigan in the summer of 1948':

We were both there for five to six weeks, I guess. This was sort of the main event of the summer. He lectured three times a week, or something of that kind, and it was a leisurely affair. People stayed for several weeks, only two or three lectures a day, maybe fewer, so we had lots of time. Uhlenbeck and, I think, Chandrasekhar also lectured.

Schwinger's lectures were incomprehensible. I always like the quote of [J.] Robert Oppenheimer—Schwinger was after all his protégé—that "other people give talks to tell you how to do it, but Julian gives talks to tell you how only he can do it." He gave this extraordinarily elaborate formalism and never really explained very much why he did it that way. It was a rather bewildering morass. But when I got him quietly alone, then it was very different.... Then afterwards I would go and talk to him and he was very friendly and then he talked in a totally different way, telling me what it was all about, why he did it that way. So it was strange that his public persona was so different from his private one. When I had him to myself it was actually delightful. Of course, he also talked a lot about other things, such as what he had been doing in classical electromagnetism, and so on. I don't remember any details but I have a feeling he had just done [classical] synchrotron radiation calculation at that time. I found that very interesting.

We were there for five weeks. At the end of that time, I felt I understood what Schwinger had done (that had not yet been published). I was very lucky to have that completely explained, clear and understandable, more or less in detail, so I could go on and do my own work.

> That was the good luck. I'd been with Feynman all through the winter and had gone with Feynman to Alburquerque just before Michigan. I spent the rest of the summer in Berkeley, and I think it was September that things sort of came together. (Dyson, Interviews and Conversations with Jagdish Mehra, *loc. cit.*, and K. A. Milton, 12 March 1999)

Upon the completion of Julian Schwinger's lectures at Ann Arbor, Freeman Dyson went to Berkeley, California, on a vacation, as part of sightseeing required by the terms of his Commonwealth Fund Fellowship which had taken him to Cornell. He later recalled that on the bus ride back from Berkeley to Chicago, where he was going to stay with friends for a week:

> it became clear to me in my head what Feynman's theory really was. Since I was more in contact with Feynman than anybody else, I realized quite soon that it was a very great opportunity to translate Feynman into the language that other people could use. That was essentially my job.... Then in Chicago I really worked out the essential outline of the paper which I put together [Dyson, 1949a, b]. (Dyson, Interviews and Conversations with Jagdish Mehra, *loc. cit.*)

Dyson was more concerned with Schwinger being offended by his prior publication than Feynman, because:

> I remember I wrote some sort of polite apology to Julian for stealing his thunder, because I was publishing his stuff before he had got around to publishing it. He was very friendly. He never took any umbrage.... I remember saying I was going to reverse the tactics of Marc Antony, saying "I come to praise Schwinger not to bury him." That was when I was giving a talk at the New York meeting of the [American] Physical Society in January 1949. I think I was having a conversation with Oppenheimer at that point. I was careful to be extremely polite and as admiring as I could. (Dyson, *loc. cit.*)

Let us complement Dyson's impressions with those of Robert Finkelstein (who attended the Michigan Summer School that year, and later became Schwinger's friend and colleague at UCLA). He would summarize Schwinger's achievement as follows:

> It was in the 1948 Michigan lectures that Julian first described his breakthrough in quantum electrodynamics to a wider audience. Among the young people present were Dyson, Kroll, Lee and Yang. (Yang told me that he had never heard anyone speak English so rapidly.) One may give a feeling for the impact of these lectures by quoting Dyson who wrote home that "in a few months we shall have forgotten what pre-Schwinger physics was like." The work Julian was then describing grew out of the experimental discoveries of Lamb, Rabi, and Kusch, and led to the mid-century revolution in theoretical physics. Bethe at that time described this period as the most exciting in physics since the great days of 1925–30 when quantum mechanics was being discovered.
>
> Although very many others, and of course particularly Tomonaga and Feynman,

> contributed to this development, it was Julian [Schwinger] who made the major breakthrough by first understanding the full consequence of the new experiments, by constructing the first manifestly covariant theory, and by first calculating in lowest order all the previously inaccessible consequences of QED. Other simpler formalisms soon followed: Feynman's Michigan lectures came the following summer, and Dyson's lectures came the third year, but a special place in our Pantheon belongs to Julian [Schwinger] who first climbed the mountain and dominated the earliest developments. To show that this mountain could be climbed at all was a very great achievement because up to that time quantum electrodynamics appeared to be fatally flowed. (Finkelstein, 1996, p. 106)

(h) Schwinger's Covariant Approach (1948–1949)

The covariant approach to quantum electrodynamics, which Schwinger presented in 'Quantum Electrodynamics. I' (Schwinger, 1948c), 'II' (Schwinger, 1949a), and 'III' (Schwinger, 1949c), was essentially identical to that first outlined at the Washington Meeting of the American Physical Society (Schwinger, 1948b), held in late April 1948, and then given in detail at the Michigan Summer School that year. These papers were also the basis for his successful application for the Charles L. Mayer Nature of Light Award in October 1949. The first of these papers was submitted just three months after his announcement of the solution of the problem of quantum electrodynamics (Schwinger, 1948a), in July 1948, with the second and third reaching the hands of the editors of *Physical Review* in November and the following May, respectively (Schwinger, 1949a, c).

Why was it necessary for Schwinger to abandon the noncovariant approach which so successfully yielded the $\alpha/2\pi$ correction to the magnetic moment of the electron (1948a)? It was the difficulty of correctly carrying out a relativistic calculation of the Lamb shift, that is, the electrodynamic displacement of hydrogen energy levels from the values predicted by the Dirac equation. Although Schwinger advertized in his note (1948a) also success on this front, it was not satisfactory. Let us quote Schwinger himself, from his introductory remarks in his collection of the most important papers in the field, entitled *Quantum Electrodynamics*: After recounting the progress since Kramers (1938a), which had been spurred by experiment, he first mentioned Bethe's work (1947), and continued:

> While this is a possible nonrelativistic procedure, it is not a satisfactory basis for relativistic calculations where the difference of two individually divergent terms is generally ambiguous. It was necessary to subject the conventional Hamiltonian electrodynamics to a transformation designed to introduce the proper description of single electron and proton states, so that the interaction among these particles would be characterized from the beginning by experimental parameters. As a result of this calculation [Schwinger, 1948a], performed to the first significant order of approximation in the electromagnetic coupling, the electron acquired new electrodynamic properties, which were completely finite. These included an energy displacement in an external magnetic field corresponding to an additional spin magnetic moment, and a displacement of energy levels in a Coulomb field. Both predictions were in good

accord with experiment, and later refinements in experiment and theory have only emphasized that agreement. However, the Coulomb calculation disclosed a serious flaw; the additional spin interaction that appeared in an electrostatic field was not that expected from the relativistic transformation properties of the supplementary spin magnetic moment, and had to be artificially corrected [Schwinger, 1949a; see also Oppenheimer, 1950, footnote 5, p. 269]. Thus a complete revision in the computational techniques of the relativistic theory could not be avoided. The electrodynamic formalism is invariant under Lorentz transformations and gauge transformations, and the concept of renormalization is in accord with these requirements. Yet, in virtue of the divergences inherent in the theory, the use of a particular coordinate system or gauge in the course of computation could result in a loss of covariance. A version of the theory was needed that manifested covariance at every stage of the calculation. The basis of such a formulation was found in the distinction between the elementary properties of the individual uncoupled fields and the effects produced by the interaction between them [Tomonaga, 1946; Schwinger, 1948c]. The application of these methods to the problems of vacuum polarization, electron mass, and the electromagnetic properties of single electrons now gave finite, covariant results which justified and extended the earlier calculations [Schwinger, 1949c]. Thus to the first approximation at least, the use of a covariant renormalization technique had produced a theory that was devoid of divergences and in agreement with experience, all high energy difficulties being isolated in the renormalization constants. Yet, in one aspect of these calculations, the preservation of gauge invariance, the utmost caution was required [Tomonaga, 1948], and the need was felt for less delicate methods of evaluation. Extreme care would not be necessary if, by some device, the various divergent integrals could be rendered convergent while maintaining their general covariant features. This can be accomplished by substituting, for the mass of the particle, a suitably weighted spectrum of masses, where all auxiliary masses eventually tend to infinity [W. Pauli and F. Villars, 1949]. Such a procedure has no meaning in terms of physically realizable particles. It is best understood, and replaced, by a description of the electron with the aid of an invariant proper-time parameter. Divergences appear only when one integrates over this parameter, and gauge-invariant, Lorentz-invariant results are automatically guaranteed merely by reserving this integration to the end of the calculation [Schwinger, 1951a].

Throughout these developments the basic view of electromagnetism was that originated by Maxwell and Lorentz—the interaction between charges is propagated through the field by local action. In its quantum-mechanical transcription it leads to formalisms in which charged particles and field appear on the same footing dynamically. But another approach is also familiar classically; the field produced by arbitrarily moving charges can be evaluated, and the dynamical problem reformulated as the purely mechanical one of particles interacting with each other, and themselves, through a propagated action at a distance. The transference of this line of thought into quantum language [Feynman, 1949a, b and 1950] was accomplished by another shift in emphasis relative to the previously described work. In the latter, the effect on the particles of the coupling with the electromagnetic field was expressed by additional energy terms which could then be used to evaluate energy displacements in bound states, or to compute corrections to scattering cross sections. Now the fundamental viewpoint was that of scattering, and in its approximate versions led to a detailed space-time description of the various interaction mechanisms. The two approaches are equivalent; the formal integration of the differential equations of one method supplying the starting point of the other [Dyson, 1949a]. But if one excludes the consideration of bound states, it is possible to expand the elements of scattering matrix in powers of the coupling constant, and examine the effect of charge and mass renormalization, term by term, to indefinitely high powers. It appeared that, for any process, the coefficient of each power in the renormalized coupling constant was completely finite [Dyson, 1949b]. This highly satisfactory result did not mean, however, that the act of renormalization had, in itself, produced a more correct theory. The convergence of the power series is not established, and the series doubtless has the significance of an asymptotic expansion. Yet, for practical purposes, in which the smallness of the coupling parameter is relevant, this analysis gave assurance that calculations of arbitrary precision could be performed.

The evolutionary process by which relativistic field theory was escaping from the confines of its nonrelativistic heritage culminated in a complete reconstruction of the foundations of quantum dynamics. The quantum mechanics of particles had been expressed as a set of operator prescriptions superimposed upon the structure of classical mechanics in Hamiltonian form. When extended to relativistic fields, this approach had the disadvantage of producing an unnecessarily great asymmetry between time and space, and of placing the existence of Fermi-Dirac fields on a purely empirical basis. But the Hamiltonian form is not the natural starting point of classical dynamics. Rather, this is supplied by Hamilton's action principle, and action is a relativistic invariant. Could quantum dynamics be developed independently from an action principle, which being freed from the limitations of the correspondence principle, might automatically produce two distinct types of dynamical variables? The correspondence relation between classical action, and the quantum-mechanical description of time development by a transformation function, had long been known [Dirac, 1933]. It had also been observed that, for infinitesimal time intervals and sufficiently simple systems, this asymptotic connection becomes sharpened into an identity of the phase of the transformation function with the classically evaluated action [Feynman, 1948a]. The general quantum-dynamical principle was found in a differential characterization of transformation functions, involving the variation of an action operator [Schwinger, 1951b]. When the action operator is chosen to produce first-order differential equations of motion, or field equations, it indeed predicts the existence of two types of dynamical variables with operator properties described by commutators and anti-commutators, respectively [Schwinger, 1953]. Furthermore, the connection between statistics and the spin of the particles is inferred from invariance requirements, which strengthens the previous arguments based upon properties of non-interacting particles [Pauli, 1940]. The practical utility of this quantum-dynamical principle stems from its very nature; it supplies differential equations for the construction of the transformation functions that contain all the dynamical properties of the system. It leads in particular to a concise expression of quantum electrodynamics in the form of coupled differential equations for electron and photon propagation functions [Schwinger, 1951c]. Such functions enjoy the advantages of space-time pictorializability, combined with general applicability to bound systems on scattering situations. Among these applications has been a treatment of that most electrodynamic of systems—positronium, the metastable atom formed by a positron and an electron. The agreement between theory and experiment on the finer details of this system is another quantitative triumph of quantum electrodynamics [Karplus and Klein, 1952].

The post-war developments of quantum electrodynamics have been largely dominated by questions of formalism and technique, and do not contain any fundamental improvement in the physical foundations of the theory. Such a situation is not new in

> the history of physics; it took the labors of more than a century to develop the methods that express fully the mechanical principles laid down by Newton. But, we may ask, is there a fatal fault in the structure of [quantum] field theory? Could it not be that the divergences—apparent symptoms of malignancy—are only spurious byproducts of an invalid expansion in powers of the coupling constant and that renormalization, which can change no physical implication of the theory, simply rectifies this mathematical error? This hope disappears on recognizing that the observational basis of quantum electrodynamics is self-contradictory. The fundamental dynamical variables of the electron-positron field, for example, have meaning only as symbols of the localized creation and annihilation of charged particles, to which are ascribed a definite mass without reference to the electromagnetic field. Accordingly it should be possible, in principle, to confirm these properties by measurements, which, if they are to be uninfluenced by the coupling of the particles to the electromagnetic field, must be performed instantaneously. But there appears to be nothing in the formalism to set a standard for arbitrarily short times and, indeed, the assumption that over sufficiently small intervals the two fields behave as though free from interaction is contradicted by evaluating the supposedly small effect of the coupling. Thus, although the starting point of the theory is the independent assignment of properties to the two fields, they can never be disengaged to give those properties immediate observational significance. It seems that we have reached the limits of the quantum theory of measurement, which asserts the possibility of instantaneous observations, without reference to specific agencies. The localization of charge with indefinite precision requires for its realization a coupling with the electromagnetic field that can attain arbitrarily large magnitudes. The resulting appearance of divergences, and contradictions, serves to deny the basic measurement hypothesis. We conclude that a covergent theory cannot be formulated consistently within the framework of present space-time concepts. To limit the magnitude of interactions while retaining the customary coordinate description is contradictory, since no mechanism is provided for precisely localized measurements. (Schwinger, 1958, pp. xi–xvi)

An indication of the impact of Schwinger's breakthroughs, as seen by his peers at the time, was given by J. Robert Oppenheimer's remarks at the 1948 Solvay Conference in Brussels, to which Schwinger was invited, but did not attend, due to some mixup in the invitation. After reviewing the failures of the old quantum field theory, Oppenheimer stated:

> Such a procedure would no doubt be satisfactory, if cumbersome, were all quantities involved finite and unambiguous. In fact, since mass and charge corrections are in general represented by logarithmically divergent integrals, the ... procedure serves to obtain finite, but not necessarily unique or correct, reactive corrections for the behaviour of an electron in an external field; and a special tact is necessary, such as that implicit in Luttinger's derivation [Luttinger, 1948] of the electron's anomalous gyromagnetic ratio, if results are to be, not merely plausible, but unambiguous and sound. Since, in more complex problems, and in calculations carried to higher orders in e, this straightforward procedure becomes more and more ambiguous, and the results are more dependent on the choice of Lorentz frame and of gauge, more powerful methods are required. Their development has occurred in two steps, the first largely the second wholly, due to Schwinger. (Oppenheimer, 1950, p. 172)

Let us now summarize the contents of Schwinger's main contributions of 1948 and 1949. (Schwinger, 1948c) We start with 'Quantum Electrodynamics. I. A Covariant Formulation' received by *Physical Review* on 29 July 1948, which is a comprehensive development of the theory first presented in an all-day talk at the Pocono meeting on March 31 of that year, and which, with the first draft of 'Quantum Electrodynamics II,' was used as the basis for his lectures at the Michigan Summer School during the period 19 July through 7 August 1948. The paper began with an extended abstract that summarized the matter brilliantly:

> Attempts to avoid the divergence difficulties of quantum electrodynamics by multilation of the theory have been uniformly unsuccessful. The lack of convergence does indicate that a revision of electrodynamic concepts at ultrarelativistic energies is indeed necessary, but no appreciable alteration of the theory for moderate relativistic energies can be tolerated. The elementary phenomena in which divergences occur, in consequence of virtual transitions involving particles with unlimited energy, are the polarization of the vacuum and the self-energy of the electron, effects which essentially express the interaction of the electromagnetic and matter fields with their own vacuum fluctuations. The basic result of these fluctuation interactions is to alter the constants characterizing the properties of the individual fields, and their mutual coupling, albeit by infinite factors. The question is naturally posed whether all divergences can be isolated in such unobservable renormalization factors; more specifically, we inquire whether quantum electrodynamics can account unambiguously for the recently observed deviations from the Dirac electron theory, without the introduction of fundamentally new concepts. This paper, the first in a series devoted to the above question, is occupied with the formulation of a completely covariant electrodynamics. Manifest covariance with respect to Lorentz and gauge transformations is essential in a divergent theory since the use of a particular reference system or gauge in the course of calculation can result in a loss of covariance in view of the ambiguities that may be the concomitant of infinities. It is remarked, in the first section, that the customary canonical commutation relations, which fail to exhibit the desired covariance since they refer to field variables at equal times and different points of space, can be put in covariant form by replacing the four-dimensional surface $t =$ const. by a space-like surface. The latter is such that light signals cannot be propagated between any two points on the surface. In this manner, a formulation of quantum electrodynamics is constructed in the Heisenberg representation, which is obviously covariant in all its aspects. It is not entirely suitable, however, as a practical means of treating electrodynamic questions, since commutators of field quantities at points separated by a time-like interval can be constructed only by solving the equations of motion. This situation is to be contrasted with that of the Schrödinger representation, in which all operators refer to the same time, thus providing a distinct separation between kinematical and dynamical aspects. A formulation that retains the evident covariance of the Heisenberg representation, and yet offers something akin to the advantage of the Schrödinger representation, can be based on the distinction between the properties of non-interacting fields, and the effects of coupling between fields. In the second section, we construct a canonical transformation that changes the field equations in the Heisenberg representation into those of non-interacting fields, and therefore describes the coupling between fields in terms of a varying state vector. It is then a simple matter to evaluate commutators of field quantities at arbitrary space-time points. One thus obtains an obviously covariant and practical form of

> quantum electrodynamics, expressed in a mixed Heisenberg-Schrödinger representation, which is called the interaction representation. The third section is devoted to a discussion of the covariant elimination of the longitudinal field, in which the customary distinction between longitudinal and transverse fields is replaced by a suitable covariant definition. The fourth section is concerned with the description of collision processes in terms of an invariant collision operator, which is the unitary operator that determines the over-all change in state of a system as a result of interaction. It is shown that the collision operator is simply related to the Hermitian reaction operator, for which a variational principle is constructed. (Schwinger, 1948c, p. 1439)

The interaction representation indeed seems to have been Schwinger's invention, although he noted in a footnote that 'The interaction representation can be regarded as a field generalization of the many-time formalism, from which point of view it has already been considered by S. Tomonaga (1946)' (Schwinger, *loc. cit.*, p. 1448, footnote 14) In that representation, the evolution of state vector Ψ on a particular spacelike surface σ is given by a covariant Schwinger equation [or Tomonaga equation, as Oppenheimer would call it—see Eq. (806) and below],

$$i\hbar c \frac{\delta \Psi[\sigma]}{\delta \sigma(x)} = \mathscr{H}(x)\Psi[\sigma], \tag{821}$$

where $\mathscr{H}$ is the interaction Hamiltonian,

$$\mathscr{H}(x) = -\frac{1}{c} j_\mu(x) A_\mu(x), \tag{822}$$

j_μ being the electric current density of the electrons, and A_μ being the electromagnetic vector potential. (In this paper, and implicitly in all his early papers, Schwinger used an imaginary fourth-component of the four-vector position: $x_\mu = (\mathbf{r}, ict)$.)

Schwinger's first paper is largely devoted to setting up the machinery. Most interesting, perhaps, is the final section, which begins with the words: 'While the interactions between fields and their vacuum fluctuations are conveniently regarded as modifying the properties of the non-interacting fields, other types of interactions are often best viewed as producing transitions among the states of the individual fields. We shall conclude this paper with a brief discussion of a covariant manner of describing such transitions.' (Schwinger, 1948c, p. 1459) Thus, the state vector on an arbitrary spacelike surface σ is related to that on an initial surface σ_1 by a unitary operator:

$$\Psi[\sigma] = U[\sigma, \sigma_1]\Psi[\sigma_1], \tag{823}$$

where U satisfies the equation of motion,

$$i\hbar c \frac{\delta}{\delta \sigma(x)} U[\sigma, \sigma_1] = \mathscr{H}(x) U[\sigma, \sigma_1], \tag{824}$$

subject to the initial condition

$$U[\sigma_1, \sigma_1] = 1. \tag{825}$$

The differential equation (824) is equivalent to a functional integral equation,

$$U[\sigma, \sigma_1] = 1 - \frac{i}{\hbar c} \int_{\sigma_1}^{\sigma} \mathscr{H}(x') U[\sigma', \sigma_1] \, d\omega', \tag{826}$$

where the last term is a space-time integral over the volume between the two surfaces σ_1 and σ. If we let those surfaces recede to $\mp\infty$, respectively, we obtain the collision operator S, which determines the overall change in state of the system as the result of interaction,

$$S = U[\infty, -\infty]. \tag{827}$$

This unitary operator may be written in terms of a Hermitian reaction operator K,

$$S = \frac{1 - iK}{1 + ik}. \tag{828}$$

Schwinger concluded this paper by showing that K satisfies a variational principle.

The second paper, 'Quantum Electrodynamics. II. Vacuum Polarization and Self-Energy' reached the editors of *Physical Review* on 1 November 1948, and was published the following February (Schwinger, 1949a). Now, Schwinger got down to work: 'The covariant formulation of quantum electrodynamics, developed in a previous paper, is here applied to two elementary problems—the polarization of the vacuum and the self-energies of the electron and photon.' (Schwinger, *loc. cit.*, p. 651) He first defined 'the vacuum of the isolated electromagnetic field to be that state for which the eigenvalue of the energy, or better, an arbitrary time-like component of the energy-momentum four-vector, is an absolute minimum.' In that state, the energy-momentum tensor has vanishing expectation value, 'the only result compatible with the requirement that the properties of the vacuum be independent of the coordinate system,' (Schwinger, *loc. cit.*) because the energy-momentum tensor of the electromagnetic field is traceless. As for the matter—that is, the electron—fields, the vacuum must be such that the vacuum expectation value of the electron energy-momentum tensor is not necessarily zero, but can be so redefined.

Armed with these properties, Schwinger went on to compute the polarization of the vacuum. That is, as a consequence of fluctuations in the electron–positron fields, the vacuum expectation value of the electromagnetic current $\langle j_\mu(x) \rangle$ is no longer zero in the presence of an external current J_μ. The result is particularly simple if the latter is time independent, and then has the form

$$\langle j_\mu(x) \rangle = -\frac{\alpha}{6\pi^2} \int K(\mathbf{r} - \mathbf{r}') \nabla'^2 J_\mu(\mathbf{r}') \, d\tau', \tag{829}$$

where $d\tau'$ is an element of volume, and (unlike Schwinger, we set $\hbar = c = 1$)

$$K(\mathbf{r}) = \frac{3}{2}\frac{1}{|\mathbf{r}|}\int_0^1 \frac{1 - \frac{1}{3}v^2}{1 - v^2} e^{-2mi|\mathbf{r}|(1-v^2)^{-1/2}} v^2\, dv. \tag{830}$$

Note that Schwinger in 1948 was using what would later universally be referred to as a Feynman parameter v, which Feynman would introduce only in 1949 to combine his propagators in momentum space. This result can be expressed as a correction to the Coulomb potential, for short distances, having the form

$$D(\mathbf{r}) = \frac{1}{|\mathbf{r}|}\left[1 + \frac{2\alpha}{3\pi}\left(\log\frac{1}{m|\mathbf{r}|} - \gamma - \frac{5}{6}\right)\right], \quad 2m|\mathbf{r}| \ll 1. \tag{831}$$

(Here, we have not followed Schwinger's original notation, but have used the usual notation that $\gamma = 0.57721\ldots$ is Euler's constant.) This result had first been found by Uehling (1935).

Schwinger next went on to calculate the self-energy of the electron. The outcome was a logarithmically divergent result for the electromagnetic mass of the electron or positron. Either by using the lower limit of a parameter integral, $w_0 \to 0$, or a large momentum scale, $K \to \infty$, to define the divergent integral, he found for the ratio of the electromagnetic mass δm to the bare mass m_0 (*loc. cit.*, p. 675)

$$\frac{\delta m}{m_0} = \frac{3\alpha}{4\pi}\left[\log\frac{K^2}{m_0^2} + \text{constant}\right], \tag{832}$$

where the constants are different with the two different 'cutoffs.' He then showed that $m = m_0 + \delta m$ may be consistently used as the actual electron mass. [As we will see, this result, which was first derived in the hole theory by Weisskopf with Furry's help, was actually given by a covariant derivation nearly six months before Schwinger by Feynman (1948c)!]

The old guard in Europe was not altogether satisfied with Schwinger's breakthroughs. Gregor Wentzel objected to Schwinger's claim at the Pocono Conference that the photon self-energy vanished; in the meantime, Schwinger had developed an improved treatment of this question, which he had presented at the Michigan Summer School, and which appears in QED.II, but Wentzel still had mathematical objections (Wentzel, 1948). Not surprisingly, even more confrontational was the reaction of Wolfgang Pauli. Schwinger sent a copy of QED.II to him, and Pauli wrote back a detailed letter in January 1949. Pauli also objected to certain details of the vacuum polarization calculation, and strongly advocated his own regularization technique which, as we have seen, Schwinger loathed. An extract of this letter appears in Schweber's book (1994, pp. 348–350). Schwinger did not reply, but rather passed the letter on to his student Bryce DeWitt, who responded without consulting Schwinger further. It was a reasonable argument involving the requirement of gauge invariance. Pauli then wrote a caustic letter to Oppenheimer:

> My discussion with Schwinger, in which he never participated himself, makes me think on "His Majesty's" psychology. (An evening seminar on this subject—ladies admitted—would be very funny. I can also tell experimental material from earlier times.) His Majesty permitted one of his pupils (B. Seligmann) [B. DeWitt] to break the "blockade" of the ETH/Zurich by Harvard and write to me a letter, but he refused to read the letter himself! [In fact, DeWitt never showed Schwinger the letter.] The content of this diplomatic note (it was a very long one) is only this, that His Majesty had a kind of revelation on some Mt. Sinai, to put always $\frac{\partial \Delta^{(1)}}{\partial x_v} = 0$ for $x = 0$ (in contrast to $\frac{\partial \delta(x)}{\partial x_v}$ which has same symmetry properties) wherever it occurs. We are calling here this equation "the revelation" but it did not help our understanding. The B. Seligmann and also a Mr. Glauber want to come here next spring, but both are unable to obtain a scientific recommendation from His Majesty who prefers to "sacrifice" both of them rather than write to me. I am enjoying this situation very much. (Pauli to Oppenheimer, February/May, 1949, in Pauli, 1993, pp. 626–627)

In fact, Schwinger did write strong letters of recommendation for both DeWitt and Glauber, and the following summer, Schwinger visited Pauli in Zurich in an attempt to smooth ruffled feathers.

'Quantum Electrodynamics. III. The Electromagnetic Properties of the Electron—Radiative Corrections to Scattering' (Schwinger, 1949c) was submitted six months after writing 'QED.II,' as the third communication of this series. It is important to recognize that Schwinger was also involved in several other completely independent projects at the same time. Thus, he submitted a paper on diffraction with Harold Levine in January 1949 and another paper on 'Classical Radiation of Accelerated Electrons' to *Physical Review* in March as well. But clearly QED was now the focus. It may be helpful to quote the opening paragraphs of 'QED.III':

> A covariant form of quantum electrodynamics has been developed, and applied to two elementary vacuum fluctuation phenomena in the previous article of this series. These applications were the polarization of the vacuum, expressing the modifications in the properties of the electromagnetic field arising from its interaction with the matter field vacuum fluctuations, and the electromagnetic mass of the electron, embodying the corrections to the mechanical properties of the matter field, in its single particle aspect, that are produced by the vacuum fluctuation of the electromagnetic field. In these problems, the divergences that mar the theory are found to be concealed in unobservable charge and mass renormalization factors.
>
> The previous discussion of the polarization of the vacuum was concerned with a given current distribution, one that is not affected by the dynamical reactions of the electron-positron matter field. We shall now consider the more complicated situation in which the original current is that ascribed to an electron or positron—a dynamical system, and an entity indistinguishable from the particles associated with the matter field vacuum fluctuations. The changed electromagnetic properties of the particle will

> be exhibited in an external field, and may be compared with the experimental indications of deviations from the Dirac theory that were briefly discussed in [QED] I [Schwinger, 1948c]. To avoid a work of excessive length, this discussion will be given in two papers. In this paper we shall construct the current operator as modified, to the second order, by the coupling with the vacuum electromagnetic field. This will be applied to compute the radiative correction to the scattering of an electron by a Coulomb field (Schwinger, 1949b). The second paper will deal with the effects of radiative corrections on energy levels. (Schwinger, *loc. cit.*, p. 790)

However, that second paper was never written, largely because soon Schwinger would begin work on a third reformulation of quantum electrodynamics. Let us concentrate on the results given in this monumental paper.[1139a] After removing a spurious infrared divergence, Schwinger obtained first the additional spin magnetic moment he had first given a year and one-half earlier [Schwinger, *loc. cit.*, p. 802, Eq. (1.121)],

$$\delta\mu = \frac{\alpha}{2\pi}\mu_0. \tag{833}$$

Then, he turned to radiative corrections to electron scattering. He obtained a result for the differential scattering cross section by an electron scattered by a fixed charge Ze (a nucleus) through an angle

$$\frac{d\sigma(\theta, \Delta E)}{d\Omega} = \left(\frac{Z\alpha}{2|\mathbf{p}|\beta}\,\mathrm{cosec}^2\,\frac{\theta}{2}\right)^2\left(1 - \beta^2 \sin^2\frac{\theta}{2}\right)(1 - \delta(\theta, \Delta E)), \tag{834}$$

where $\mathbf{p}$ is the electron momentum, βc is its speed, and the energy loss does not exceed ΔE. A general expression for the radiative correction δ was given; for a slowly moving particle, it takes on the simple form

$$\delta(\theta, \Delta E) \approx \frac{8\alpha}{3\pi}\beta^2 \sin^2\frac{\theta}{2}\left[\log\frac{mc^2}{2\Delta E} + \frac{19}{30}\right], \quad \beta \ll 1,\ \Delta E \ll E - mc^2, \tag{835}$$

where E is the electron energy. Schwinger concluded that these radiative corrections could amount to several percent at the energies then available.

The above result (835) had, in fact, been derived nearly six months earlier. In January 1949, Schwinger had written a Letter to the *Physical Review* in which he discussed 'Radiative Corrections to Electron Scattering' (Schwinger, 1949b). There, he also discussed the Lamb shift, mentioned his earlier error due to the improper magnetic moment contribution, and stated that the result, equal to 1051

[1139a] The actual calculations in Schwinger's quantum electrodynamics turned out to be quite lengthy. For example, he wrote over 100 equations before the second-order correction to the current operator could be written (see Eq. 1.124) of Schwinger (1949c), and even after evaluating further 100 formulae, Eq. (834) would follow.

MHz for the splitting of the $2^2S_{1/2}$ and $2^2P_{1/2}$ levels of hydrogen, was now in agreement with the calculations of French and Weisskopf (1949) and of Kroll and Lamb (1949). As we remarked above, when Schwinger first did the covariant calculation, he made an additional error in matching the high- and low-energy contributions. Feynman made the same mistake, resulting in a significant delay in publication of the French and Weisskopf paper.[1139b]

(i) Gauge Invariance and Vacuum Polarization (1950)

The paper 'On Gauge Invariance and Vacuum Polarization,' submitted by Schwinger to the *Physical Review* near the end of 1950 (Schwinger, 1951a), is almost universally acclaimed as his greatest publication. It is most remarkable because it stands in splendid isolation. It was written over a year after the last of his series of papers on his second, covariant formulation of quantum electrodynamics was completed. And barely two months later, in March 1951, Schwinger would submit the first of the series on his third reformulation of quantum field theory, that was based on the quantum action principle, namely, 'The Theory of Quantized Fields I' (Schwinger, 1951b). But 'Gauge Invariance and Vacuum Polarization' stands on its own, and has endued the rapid changes in tastes and developments in quantum field theory, while the papers in the other series are mostly of historical interest now. As Lowell Brown has pointed out, this paper still has over one hundred citations per year, and is far and away Schwinger's most cited paper (L. S. Brown, 1996, p. 131).

Yet even such a masterpiece was not without its critics. Abraham Klein, who was finishing his doctoral thesis under Schwinger's direction, and would go on to become one of Schwinger's second set of 'assistants' (with Robert Karplus) as, first, an instructor, and then a Junior Fellow, recalled that Schwinger (and, independently, he and Karplus) ran afoul of a temporary editor at the *Physical Review*. That editor thought that Schwinger's original paper repeated too many complicated expressions and that symbols should be introduced to represent expressions that appeared more than once. Schwinger complied, but had his assistants do the dirty work. Harold Levine, who was still sharing Schwinger's office (and whom Schwinger had brought with him as his assistant from the MIT Radiation Laboratory), working on the book on waveguides, typed the revised manuscript, while Klein wrote in many equations. Klein recalled that he took much more care in writing those equations than he did in his own papers. (Abraham Klein, interview by K. A. Milton, 14 December 1998). Schwinger recalled later that he viewed this paper, in part, as a reaction to the:

[1139b] Schweber criticized Schwinger for being less than forthright in acknowledging his error, unlike Feynman; but in Schwinger's defense, it should be noted that he never published his wrong result, giving the incorrect formula only at the Michigan Summer School (Schwinger Archive at UCLA). Schwinger gave a detailed account of his and Feynman's 'goof' in his historical talk 'Renormalization Theory of Quantum Electrodynamics: An Individual View,' in which he concluded, 'And so, although Weisskopf was not the first to find the correct result, he was the first to insist on its correctness.' (Schwinger, 1983a, p. 341)

invariant regularization of Pauli and Villars. (Pauli and Villars, 1949, p. 434) It was this paper, with its mathematical manipulation, without physical insight particularly about such questions as photon mass and so forth, which was the direct inspiration for "Gauge Invariance and Vacuum Polarization." The whole point is that if you have a propagation function, it has a certain singularity when the two points coincide. Suppose you pretend that there are several particles of the same type with different masses and with coupling constants which can suddenly become negative instead of positive. Then, of course, you can cancel them. It's cancellation again, subtraction physics, done in a more sophisticated way, but still, things must be made to add up to zero. Who needs it? (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

An extended synopsis of this work, of course, cannot do justice to its beauty, elegance, and power. The title of this paper is apt, as the first two sentences of the abstract indicate: 'This paper is based on the elementary remark that the extraction of gauge invariant results from a formally gauge invariant theory is ensured if one employs methods of solution that involved only gauge-covariant quantities. We illustrate this statement in connection with the problem of vacuum polarization by a prescribed electromagnetic field.' (Schwinger, 1951a, p. 664) The primary methodology is the use of a proper-time formalism, which is nothing other than the exploitation of the Euler representation of the gamma function, but which allows one to commute dynamical variables by solving proper-time equations of motion, and remains one of the most powerful techniques in quantum field theory. After adopting the gauge-invariant philosophy, Schwinger regarded 'the rest as just technique: I go through the solution of certain problems, some of which turned out to be of some importance in later developments. It is a *tour de force*, let's face it, because it is not easy to find a technique to deal with the electromagnetic field in which the vector potential never enters. The vector potential never appears, there's no gauge ambiguity. I got a great deal of fun out of this.' (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

Schwinger started by describing the motion of an electron, of charge e and mass m, in a prescribed electromagnetic field, A_μ, which motion, of course, is given by the 'second-quantized' Dirac equation,

$$\gamma_\mu(-i\partial_\mu - eA_\mu(x)\psi(x) + m\psi(x) = 0, \tag{836}$$

where the electron field ψ and its adjoint satisfy the equal-time anticommutation relation

$$\{\psi(\mathbf{x}, x_0), \bar{\psi}(\mathbf{x}', x_0)\} = \gamma_0\delta(\mathbf{x} - \mathbf{x}'), \tag{837}$$

and γ^μ are the Dirac matrices such that

$$\{\gamma_\mu, \gamma_\nu\} = -2g_{\mu\nu} \tag{838}$$

in terms of the metric tensor $g_{\mu\nu}$, whose nonzero, diagonal matrix elements are $(-1, 1, 1, 1)$. The electron's Green's function is defined in terms of the vacuum expectation value of the time-ordered product of Dirac fields,

$$G(x, x') = i\langle(\psi(x)\bar{\psi}(x'))_{+}\rangle\varepsilon(x - x'), \tag{839}$$

where for arbitrary field the time-ordering was given by

$$(A(x_0)B(x_0'))_{+} = \begin{cases} A(x_0)B(x_0') & \text{for } x_0 > x_0', \\ B(x_0')A(x_0) & \text{for } x_0 < x_0', \end{cases} \tag{840}$$

and the symbol ε changed the sign depending upon the ordering,

$$\varepsilon(x - x') = \begin{cases} 1 & \text{for } x_0 > x_0', \\ -1 & \text{for } x_0 < x_0'. \end{cases} \tag{841}$$

Now, Schwinger wrote:

> It is useful to regard $G(x, x')$ as a matrix element of an operator G, in which states are labeled by space-time coordinates as well as by the suppressed spinor indices:
>
> $$G(x, x') = (x|G|x'). \qquad [(842)]$$
>
> The defining differential equation for the Green's function is then considered to be a matrix element of the operator equation
>
> $$(\gamma\Pi + m)G = 1, \qquad [(843)]$$
>
> where [the gauge-covariant momentum operator]
>
> $$\Pi_\mu = p_\mu - eA_\mu \qquad [(844)]$$
>
> is then characterized by the operator properties
>
> $$[x_\mu, \Pi_\nu] = i\delta_{\mu\nu}, \quad [\Pi_\mu, \Pi_\nu] = ieF_{\mu\nu}, \qquad [(845)]$$
>
> and
>
> $$F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu \qquad [(846)]$$
>
> is the antisymmetrical field strength tensor.' (Schwinger, 1951a, p. 665)

The proper-time integral appeared on the third page (p. 666), when Schwinger wrote the Green's operator as

$$G = \frac{1}{\gamma\Pi + m} = i\int_0^\infty ds\, \exp\{-i(\gamma\Pi + m)s\}. \tag{847}$$

The vacuum expectation value of the current vector, which he expressed in terms of the Dirac matrix trace of the diagonal elements of the Green's function,

$$\langle j_\mu(x)\rangle = ie\,\mathrm{tr}\,\gamma_\mu(x|G|x), \tag{848}$$

could then be obtained by variation of a certain action integral with respect to A_μ. That action corresponded to the Lagrangian function

$$\mathscr{L}^{(1)}(x) = i\int_\infty^0 \frac{ds}{s}e^{ims}\,\mathrm{tr}(x|\exp\{-i\gamma\Pi s\}|x), \tag{849}$$

and the effective Lagrangian function $\mathscr{L}^{(1)}$ reduced to the evaluation of the matrix element of a proper-time evolution operator,

$$(x'|U(s)|x'') = (x(s)'|x(0)''), \tag{850}$$

where

$$U(s) = \exp(-i\mathscr{H}s), \quad \text{with } \mathscr{H} = -(\gamma\Pi)^2 = \Pi^2 - \frac{1}{2}e\sigma_{\mu\nu}F_{\mu\nu}, \tag{851}$$

and $\sigma_{\mu\nu} = (i/2)[\gamma_\mu, \gamma_\nu]$ denoted the generalization of the Pauli spin matrices. The interpretation of $\mathscr{H}$ was that it is a 'Hamiltonian' that evolves the system in proper-time, so that the matrix element of $U(s)$ is the transformation function from a state in which $x_\mu(s=0)$ has the value x''_μ to a state in which $x_\mu(s)$ has the value x'_μ. This yielded an immediate particle interpretation, involving the equations of motion

$$\frac{dx_\mu}{ds} = -i[x_\mu, \mathscr{H}] = 2\Pi_\mu, \tag{852}$$

$$\frac{d\Pi_\mu}{ds} = -i[\Pi_\mu, \mathscr{H}] = e(F_{\mu\nu}\Pi_\mu + \Pi_\nu F_{\mu\nu}) + \frac{1}{2}e\sigma_\lambda\frac{\partial F_{\lambda\nu}}{\partial x_\mu}. \tag{853}$$

Schwinger then proceeded to solve these equations in three cases. First, he considered the elementary case in which $\boldsymbol{F}_{\mu\nu} = 0$. Of course, this did not mean that the vector potential A_μ vanished, but only that it be a pure gauge. In that case, he found, for example, the following representation for the Green's function,

$$G(x', x'') = \frac{\Phi(x', x'')}{4\pi^2}\int_0^\infty ds\, s^{-2}e^{-im^2 s}\left(-\gamma\frac{(x'-x'')^2}{2s} + m\right)e^{i(x'-x'')^2/4s}. \tag{854}$$

Here, Φ involved a line integral of the vector potential, namely,

$$\Phi(x', x'') = \exp[ie\int_{x''}^{x'} dx_\mu A_\mu(x)]. \tag{855}$$

This was independent of the path, because the potential possessed zero curl.

The third section of the paper contained the central message. There, Schwinger considered the case of constant field strengths. This was an exactly solvable problem because it is equivalent to a harmonic oscillator system. The equations of motion, in matrix form, simply

$$\frac{dx}{ds} = 2\Pi, \quad \frac{d\Pi}{ds} = 2eF\Pi; \tag{856}$$

so, since F was constant, they could be immediately integrated to yield the results

$$\Pi(s) = e^{2eFs}\Pi(0), \tag{857}$$

$$x(s) - x(0) = \left[\frac{e^{2eFs} - 1}{eF}\right]\Pi(0). \tag{858}$$

Schwinger now proceeded inexorably and, after a page and a half of calculation, obtained a proper-time representation for the Lagrange function $\mathscr{L}^{(1)}$:

$$\mathscr{L}^{(1)} = -\frac{1}{8\Pi^2}\int_0^\infty ds\, s^{-3} e^{-m^2 s}\left[(es)^2\mathscr{G}\,\frac{\text{Re cosh } esX}{\text{Im cosh } esX} - 1\right], \tag{859}$$

where the quantity $\mathscr{G}$ denoted

$$\mathscr{G} = \sum_{\mu\nu}\frac{1}{4}F_{\mu\nu}F^*_{\mu\nu} = \mathbf{E}\cdot\mathbf{H}, \tag{860}$$

with F^* representing the dual field strength, or

$$F^* = \frac{1}{2}\sum_{\lambda,\kappa}\varepsilon_{\mu\nu\lambda\kappa}F_{\lambda\kappa}. \tag{861}$$

(A duality transformation interchanges electric and magnetic quantities: $F \to F^*$, or $\mathbf{E} \to \mathbf{B}$, $\mathbf{B} \to -\mathbf{E}$.) Similarly, a quantity $\mathscr{F}$ had been introduced, which was defined as

$$\mathscr{F} = \frac{1}{4}\sum_{\mu\nu}F^2_{\mu\nu} = \frac{1}{2}(\mathbf{H}^2 - \mathbf{E}^2), \tag{862}$$

and then X became

$$X = \sqrt{2(\mathscr{F} + i\mathscr{G})}. \tag{863}$$

Equation (859) turned out to be ultraviolet divergent, because the integrand is singular at $s = 0$; hence, it required renormalization. When expanded in powers of the field strength, the first (logarithmically) divergent term was proportional to F^2 and, thus, amounted to a rescaling of the fields, that is, a corresponding renormalizing of the charge. Therefore, that term should be simply omitted, and Schwinger was left with a finite, gauge-invariant Lagrangian function, exact in the field strength, and of second order in the fine structure constant:

$$\mathscr{L} = -\mathscr{F} - \frac{1}{i\pi^2}\int_0^\infty ds\, s^{-3} e^{-m^2 s}\left[(es)^2 \mathscr{G} \frac{\text{Re cosh } esX}{\text{Im cosh } esX} - 1\frac{2}{3}(es)^2 \mathscr{F}\right] \tag{864}$$

$$= \frac{1}{2}(\mathbf{E}^2 - \mathbf{H}^2) + \frac{2\alpha^2}{45}\frac{(\hbar/mc)^2}{mc^2}[(\mathbf{E}^2 - \mathbf{H}^2)^2 + 7(\mathbf{E}\cdot\mathbf{H})^2] + O(F^6). \tag{865}$$

Here, the coupling was written in terms of the fine structure constant, $\alpha^2 = e^2/4\pi$, and Eq. (865) corresponded to the old Euler–Heisenberg Lagrangian (Heisenberg and Euler, 1936; Weisskopf, 1936, especially Eq. (2) on p. 7). It described, for example, the scattering of light by light, which has never been observed directly (although experiments involving intense laser beams have been proposed), but indirectly it has been seen through its contribution to the anomalous magnetic moment of the electron (S. Laporta and R. Remiddi, 1991; 1993).[1139c] Schwinger concluded this section of his paper with a derivation of the effective Lagrangian for a spin-0 charged particle, which differs from the expansion given above in Eq. (865) by different numerical coefficients in front of the two Lorentz-invariant structures, $\mathscr{F}^2$ and $\mathscr{G}^2$.

In the fourth section of the paper, Schwinger repeated the calculation of a third exactly solvable situation, that of a plane electromagnetic wave. However, in that case, the invariants vanish,

$$\mathscr{F} = \mathscr{G} = 0; \tag{866}$$

hence, Schwinger concluded that 'there are no nonlinear vacuum phenomena for a single plane wave, of arbitrary strength and spectral decomposition.' (Schwinger, 1951a, p. 672)

[1139c] The Euler–Heisenberg Lagrangian represents the interaction of an arbitrary number of photons for the process $\gamma\gamma \to \gamma\gamma$; the total cross section for that process if (e.g., see Lifshitz and Pitayevski, 1974, p. 489)

$$\sigma = \frac{973}{10125\pi}\alpha^4 \frac{\omega^6}{m^6},$$

where ω is the photon frequency in the centre-of-mass frame, which, since it was derived under the assumption that the fields are constant, is valid only for $\omega \ll m$. But, in addition, it describes processes such as $\gamma\gamma \to 4\gamma$, as well as processes such as photon scattering in the presence of Coulomb fields, and the astrophysically important process of photon splitting in strong magnetic fields.

The fifth section is quite remarkable, as Schwinger considered here the decay of scalar and pseudoscalar mesons into photons, a subject which had yielded some difficulty (Steinberger, 1949). The problem was that the coupling of the pseudoscalar fermionic current could be either through a pseudoscalar interaction,

$$g\phi(x)\frac{1}{2}[\bar{\psi}(x), \gamma_5\psi(x)], \tag{867}$$

or through an axial–vector interaction,

$$\frac{g}{2M}\partial_\mu\phi(x)\frac{1}{2i}[\psi(x), \gamma_5\gamma_\mu\psi(x)], \tag{868}$$

where M denoted the mass of the fermion and g denoted the strength of the coupling. Formally, by use of the Dirac equation, these two interactions might be shown to be equivalent. But discrepancies occurred when it was attempted to compute the two-photon decay of the pion (here, we call the pseudoscalar by its modern name),

$$\pi \to \gamma\gamma, \tag{869}$$

where the two photons come from coupling to the fermionic vacuum expectation value or loop. In fact, however, Schwinger showed that if proper care was taken, the axial–vector interaction gave a result that agreed with the pseudoscalar interaction, given by the effective Lagrange function

$$\mathscr{L}' = \frac{\alpha}{\pi}\frac{g}{M}\phi\mathbf{E}\cdot\mathbf{H}. \tag{870}$$

This was an extremely important result, though unappreciated at the time. It was independently rediscovered, and dubbed the axial–vector anomaly, twenty years later (Bell and Jackiw, 1969; Adler, 1969; Jackiw and Johnson, 1969).[1140] Not

[1140] A brief history of this topic was given by K. A. Milton in a letter to *Physics Today* **50**, June 1997, p. 114. A portion of that letter reads:

> [Schwinger was] the true discoverer of the axial-vector anomaly in its original context, the decay of the neutral pion into two photons. Julian Schwinger very explicitly in his classic paper "On Gauge Invariance and Vacuum Polarization" derived the anomaly by showing that pseudoscalar and pseudovector couplings are equivalent. Of course, the language was somewhat different in those days. This result had been apparently completely forgotten by the time of the work of Adler [1969] and Bell and Jackiw [1969], but very shortly thereafter Jackiw and Johnson [1969] recognized that "the first derivation of [the anomaly equation] for external electromagnetic fields was given by Schwinger." (Indeed, Adler in a Note Added in Proof to his paper, (Adler, 1969) acknowledged Jackiw and Johnson's rediscovery of Schwinger' work.) These remarks are not at all meant to disparage in any way the significant contributions made by many people in 1968 and subsequently, but merely to remind us all in physics what a great debt we owe to Julian Schwinger. (Milton, 1997, p. 64)

only is it important for fundamental physical processes such as pion decay, but similar processes occur in gauge theories, where, if they are not suitably cancelled, they will destroy the renormalizability, and hence the consistency of those theories.

The final section of the paper contained another remarkable discovery, namely, that a constant electric field can produce electron–positron pairs; hence, for example, the Coulomb field is unstable. This has an insignificant probability of occurring unless the electric fields are very strong, but such fields might exist in very heavy transuranic elements, and will be sought in heavy-ion accelerators. The probability, per unit time and unit volume, that a pair be created by a constant electric field $\boldsymbol{E}$ should be approximately given by the quantity

$$\frac{\alpha^2}{\pi^2}\sum_{n=1}^{\infty} n^{-2} \exp\left(\frac{-n\pi m}{\mathrm{e}|\boldsymbol{E}|}\right). \tag{871}$$

One last *tour de force* concluded the paper. In a one-page Appendix, using these proper-time methods, Schwinger provided what for the time was the shortest known derivation of the anomalous magnetic moment of the electron, obtaining in the second order

$$\mu' = \frac{\alpha}{2\pi}\frac{e\hbar}{2mc}, \tag{872}$$

which agreed with the known result, Eq. (799). As Schwinger later remarked, ‘this is a very important paper, not for what it discusses, but for what it alludes to.’ (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988) Indeed, it continues even today to be the source of much new research, ranging from applications in astrophysics to searches for magnetic monopoles.[1141]

(j) The Quantum Action Principle (1951)

Schwinger’s wartime work on microwave radiation was largely based on the development of variational methods for computing the modes of microwave cavities and transmission lines. After the war, he immediately applied such techniques to nuclear physics (Schwinger, 1950; Schwinger and Lippmann, 1950); although these papers are dated 1950, they grew out of lectures given by Schwinger at Harvard in 1947. It took somewhat longer for variational methods to take centre stage in his work on quantum electrodynamics.

We recall that by 1950 Julian Schwinger had successfully scaled the peak of quantum electrodynamics (Richard Feynman, on the other hand, had but one ascent). The first approach, which was largely unpublished, led to his first calculation of the electron’s magnetic moment, Eq. (802), in 1947 (reported in

[1141] This subsection and the following one are largely based on Mehra and Milton, 2000, Chapters 8 and 9.

Schwinger, 1948a). Schwinger abandoned this approach quickly, because it was not covariant and, therefore, susceptible to serious errors. At the Washington American Physical Society meeting in April 1948, he announced the covariant approach (Schwinger, 1948b), which was fleshed out in *Quantum Electrodynamics I, II*, and *III* (Schwinger, 1948c; 1949a, c), submitted between the end of July 1948 and May 1949. It was these papers that sealed Schwinger's fame, and largely concluded Schweber's account of Schwinger's work (Schweber, 1994). But the best was yet to come. The monumental 'Gauge Invariance and Vacuum Polarization' (Schwinger, 1951a), described in the preceding subsection, was to be completed a year and a half later. At about the same time Schwinger saw how to obtain a '*Quantum Action Principle*,' extending the stationary principles of Lagrange and Hamilton, to the quantum domain. In this, as with Feynman, his point of departure was the famous paper of Dirac's, 'On the Lagrangian in Quantum Mechanics' (Dirac, 1933), but the response was completely different: Feynman was to give a global 'solution' to the problem of determining the transformation function, the probability amplitude connecting the state of the system at one time to that of the system at a later time, in terms of a sum over classical trajectories, the famous path integral. Schwinger, instead, derived (initially postulated) a differential equation for that transformation function in terms of a quantum action functional. This differential equation possessed Feynman's path integral as a formal solution, which remained poorly defined, but Schwinger believed throughout his life that his approach was 'more general, more elegant, more useful, and more tied to the historical line of development as the quantum transcription of Hamilton's action principle.' (Schwinger, 1973, p. 421)

As Schwinger stated later:

> The idea from the beginning was not, as Feynman would do, to write down the answer, but to continue in the grand tradition of classical mechanics, but only as a historical model, to find a differential, an action principle formulation. What is Hamilton's principle or its generalization in quantum physics? If you want the time transformation function , do not ask what it is but how it infinitesimally changes. The distinction [with the path integral approach] comes [because] this deals with all kinds of quantum variables, on exactly the same footing which means from a field point of view not only do Bose-Einstein fields appear naturally but Fermi-Dirac fields. Whereas with the path-integral approach with its clear connection to the correspondence principle, the anticommuting Fermi system appears out of nowhere, there is no logical reason to have it except that one knows one has to. It does not appear as a logical possibility as it does with the differential. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

At a symposium on the history of particle physics held at Fermilab in May 1980, Schwinger elaborated on this point:

> This development must have begun in late 1949 or early 1950, to judge by a set of notes entitled "Quantum Theory of Fields, A New Formulation." The notes were taken by the now President of the California Institute of Technology, then known as

> Marvin Goldberger [long retired from that position since then]. Dated July, 1950, they refer to a field theory course that was given in the semester between January and June. First for particles, and then for fields, the notes trace how the single quantum action principle leads to operator commutation relations, equations of motion, or field equations, and conservation laws. In the relativistic field context, the postulate of invariance under time reflection (remember, this is 1950) leads to two kinds of fields (two statistics) as a consequence of the more elementary analysis into two kinds of spin, integral and half-integral. This occurs because time reflection is not a canonical, a unitary, transformation, but also requires an inversion in the order of all products. That discloses the fundamental operator nature of the field, distinguishing essential commutativity from essential anticommutativity, as demanded by the spin character of the field. In the subsequent version the existence of two kinds of fields with their characteristic operator properties is recognized at an earlier stage [Schwinger, 1953]. Here also the non-Hermitean fields of charged particles are replaced by Hermitean fields of several components, facilitating the description of the internal degrees of freedom that could proliferate. In this version, time reflection implies a transformation to the complex conjugate algebra, and the postulate of invariance predicts the type of spin to be associated with each statistic. An inspection of the proof shows that what is really used is the hypothesis of invariance under time and space reflection. That invariance and the spin-statistics connection are equivalent. But, with the later discovery of parity non-conservation, the common emphasis as embodied in the so-called TCP (or is it PTC?) theorem, is to regard the spin-statistics relation as primary and the invariance under space-time reflection as a consequence. (Schwinger, 1983a, pp. 345–346)

In order to put these developments in context, we might refer to Schwinger's extended preface to his collection of the most fundamental papers on *Quantum Electrodynamics* (reported on p. 1298 ff). It is revealing of Schwinger's view of the development of the subject that in his collection (Schwinger, ed., 1958) he indeed puts these three papers in the indicated order: Dirac (1933), Feynman (1948a), and Schwinger (1951b). Actually, as Schwinger noted at the beginning of the latter paper, his programme was initiated in Summer 1949 at Brookhaven National Laboratory, and the paper was largely written there the following summer. Again, to quote from the May 1980 Fermilab lecture:

> My retreat began at Brookhaven National Laboratory in the Summer of 1949. It is only human that my first action was a reaction. Like the silicon chip of more recent years, the Feynman diagram was bringing computation to the masses. Yes, one can analyze experience into individual pieces of topology. But eventually one has to put it all together again. And then the piecemeal approach loses some of its attraction. Speaking technically, the summation of some infinite set of diagrams is better and more generally accomplished by solving an integral equation, and those integral equations usually have their origin in a differential equation. And so, the copious notes and scratches labelled "New Opus," that survive from the summer of 1949, are concerned with the compact, operator expression of classes of processes. And slowly, in these pages, the integral equations and the differential equations emerge. There is another collection of scraps that at sometime in the past I put in the folder labelled "New Theory—Old Version (1949–1950)," although I now believe that the reference

to 1950 is erroneous—by then the New Theory in its later manifestation had arrived. There is a way to tell the difference. With the emphasis on the operator field description of realistic, interacting systems, the interaction representation had begun to lose its utility, and fields incorporating the full effects of interaction enter. The unpublished essay of the National Academy of Sciences competition had already taken a step in that direction. If fields of both types, with and without reference to interaction, appear in an equation, the historical period is that of the Old Version. The later version has no sign at all of the interaction representation. On one of these pages there is an Old Version, 1949, equation giving the first steps toward the relativistic equation for two interacting particles now known as the Bethe-Salpeter equation [Salpeter and Bethe, 1951]. Accordingly, it is not surprising to read in a footnote of a 1951 paper, presenting an operator derivation of the two-particle equation, that I had already discussed in my Harvard lectures [Gell-Mann and Low, 1951] (Schwinger, 1983a, p. 343).[1142]

Let us briefly sketch a description of the paper envisaged which Schwinger entitled, 'The Theory of Quantized Fields' and submitted in March 1951 to *Physical Review* (Schwinger, 1951b). As noted, the essential idea was to break away from correspondence-principle arguments and 'develop a self-contained quantum-dynamical principle from which the equations of motion and the commutation relations could be deduced.' The introduction includes the words:

> Quantitative success has been achieved thus far only in the restricted domain of quantum electrodynamics. Furthermore, the existence of divergences, whether cancelled or explicit, serves to emphasize that the present quantum theory of fields must in some respects be incomplete. It is not our purpose to propose a solution of this basic problem, but rather to present a general theory of quantum field dynamics which unifies several independently developed procedures and which may provide a framework capable of admitting fundamentally new physical ideas. (Schwinger, *loc. cit*, p. 914)

As he would remark later:

> I was simply saying this was a synthesis of me, Feynman, Dyson, and so forth. It was going to be a unification in one systematic, self-contained framework, freed from the correspondence principle. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

Schwinger went on to complete his series of articles on 'The Theory of Quantized Fields' later on, by two relatively brief, but extremely important papers to the National Academy, 'On the Green's Functions of Quantized Fields. I' and 'II' (Schwinger, 1951c). After treating this development of Schwinger's scheme, we

[1142] The files entitled 'New Opus 1949' and 'New Theory—Old Version (1949–1950),' as well as 'Quantum Theory of Fields, A New Formulation,' class lecture notes transcribed by Marvin L. Goldberger, MIT, 1950, containing very recent, unpublished work, may be found in the Schwinger Archive—Julian Schwinger papers (Collection 371), Department of Special Collections, University Research Library—at the University of California, Los Angeles.

return to the other main contributors to quantum electrodynamics: Tomonaga, Feynman, and Dyson.

(k) Tomonaga Writes to Oppenheimer (April 1948)

Amid the devastation of the last years of the war in Japan, and in the immediate postwar period, a hard-working group of theoreticians around Sin-itiro Tomonaga in Tokyo made enormous progress in formulating a consistent, Lorentz-invariant quantum electrodynamics, although the participants were completely cut off from developments in America. This approach turned out to be remarkably similar to the covariant approach of Schwinger, although in large measure due to their isolation, they were not able to carry the programme fully through to a theory capable of producing final calculations comparable to the experimental data. The West learned of this work through Tomonaga's communication to Oppenheimer; upon his return from the Pocono Conference in April 1948, J. Robert Oppenheimer found a letter from Sin-itiro Tomonaga, who wrote:

> I have taken the liberty of sending you copies of several papers and notes concerning the reaction of radiation field in scattering processes and related problems, which my collaborators and I have been investigating the last six months. . . .
>
> During the wartimes . . . Dr. S. Sakata . . . . made an attempt to overcome the divergence difficulty of the self-energy of the electron by introducing a neutral scalar field (the so-called *C*-meson field) which interacts with electrons. . . . In view of its promising feature my collaborators and I have applied this theory to the problem of elastic scattering of an electron. . . . We have thus carried out a perturbation calculation following Dr. S. M. Dancoff [1939] and . . . finally arrived at the conclusion that the new field is also capable of eliminating the divergence in the scattering cross section in e^2-approximation (the second note of Ito, Koba, and Tomonaga [1947] and the two papers of the same authors [1948a, b]).
>
> Shortly before we finished this work we received the striking report about the experimental evidence of the level shift of hydrogen atoms [Lamb and Retherford, 1947] and its theoretical explanation by Dr. H. A. Bethe [1947] in terms of radiation interaction. Thereupon I proposed a formalism to express Dr. Bethe's fundamental assumption in a more closed and plausible—as I believe—form, in which the separation of terms to be subtracted is made by a canonical transformation (the note of Tati and Tomonaga [1948]). This formalism which we called "self-consistent subtraction method," was then applied to the scattering problem mentioned above and it was confirmed that the non-diagonal part of the mass correction just cancels the infinity that appeared in the usual formalism (the note of Koba and Tomonaga [1947] and the paper of the same authors [1948]). (Tomonaga to Oppenheimer, 2 April 1948, quoted in Schweber, 1994, pp. 200–201)[1143]

[1143] Tomonaga's communication to Oppenheimer was delivered by hand by the first Japanese students to visit the United States after the war, particularly, Katsumi Tanaka. Tanaka, who later became a professor at The Ohio State University, recalled that he was one of the first group of Japanese students sent to the USA after the war, and served as courier from Tomonaga to Oppenheimer (K. Tanaka, Conversation with K. A. Milton, 28 July 1998). Marshak (October 1987, VPI-HEP-87/7, UR-1041) recalled that Tanaka delivered the Sakata-Inoue paper (1946), which suggested the existence of two mesons, to Oppenheimer in November 1947, and presumably the *QED* papers as well.

Moreover, Tomonaga referred to the fact that the Japanese 'subtraction method' might be brought into 'a relativistically more elegant form according to the formalism' which he had proposed several years previously (Tomonaga, 1943/1946). Oppenheimer immediately cabled back to Tomonaga urging him to summarize the results he had intimated for publication in *Physical Review*. Tomonaga complied, and with the assistance of the American military agencies, his manuscript was quickly brought to the United States, where it appeared in the *Physical Review*, the issue of 15 July 1948 (Tomonaga, 1948).

In this note entitled 'On Infinite Field Reactions in Quantum Field Theory,' Tomonaga began by referring to the American papers of Bethe (1947), Schwinger (1948a), and others on the radiation problem, and then he emphasized the fact that 'almost the same line of attack was taken independently of these American authors also by the Tokyo group' (Tomonaga, 1948, p. 224). He explicitly mentioned the paper of Shoichi Sakata and Osamu Hara on the so-called *C*-meson (1947), and the subsequent work of the Tokyo group (Ito, Koba, and Tomonaga, 1947; 1948a, b; Tati and Tomonaga, 1948). Although Oppenheimer added a critical remark to Tomonaga's report —'insufficiently cautious treatment, and therefore inadequate identification of light-quantum self-energies' (in Tomonaga, 1948, p. 225), Freeman Dyson, then a British graduate student at Cornell University, was 'enormously pleased' with the Japanese competition in quantum electrodynamics, as he wrote to his parents:

> Long-sighted scientists are worried by the growing danger of nationalism in American science, and even more in the minds of politicians and industrialists who finance science. In the public mind, experimental science at least is a thing only Americans know how to do, and the fact that some theorists have to be imported from Europe is rather grudgingly admitted. In this atmosphere the new Schwinger theory tended to be acclaimed as a demonstration that now even in theoretical physics America had nothing to learn [and] now for the first time has produced her own Einstein. (Dyson to his family, 11 April 1948).[1144]

Dyson felt, however, 'that even in this chosen field of physics America was anticipated and indeed by the much despised race of Japanese' and claimed that 'this will be a strong card to play against national politics' (Dyson, *loc. cit.*).

(l) Tomonaga's Papers (1946–1948)

Sin-itiro Tomonaga's 1946 paper in *Progress of Theoretical Physics* (Tomonaga, 1946) was entitled 'On a Relativistically Invariant Formulation of the Quantum Theory of Wave Fields,' and was noted as having first been published in Japanese in 1943 (Tomonaga, 1943). It generalized the Schrödinger equation by proceeding from the many-time formulation of Dirac (1932). That is, there were as many time

[1144] J.M. is grateful to Freeman Dyson for making copies of his letters to his family in England as well as of his scientific correspondence available to him.

variables as there were particle coordinates in the state vector. This suggested the introduction of infinitely many variables, one for each point, t_{xyz}, a local time, an idea which had also been introduced by Stueckelberg (1938). From this perspective, Tomonaga was able to define the state vector as a functional of the space-like surface C, i.e., $\Psi(C)$, which satisfied the functional Schrödinger equation

$$\left\{H_{12}(P) + \frac{\hbar}{i}\frac{\delta}{\delta C_P}\right\}\Psi(C) = 0. \tag{873}$$

Here, H_{12} is the interaction Hamiltonian between the two fields that Tomonaga was considering and C_P is a surface passing through the point P. Indeed, this is the same equation that Schwinger would obtain five years later, in 1948, in his 'Quantum Electrodynamics I' (Schwinger, 1948c). A form equivalent to the integral equation (826) was also given by Tomonaga in this paper, with nearly the same notation. He rounded out the paper by giving generalized probability amplitudes and transformations functions.

To assess this work in context, it may be useful to quote the 'Concluding Remarks' in full:

> We have thus shown that the quantum theory of wave fields can be really brought into a form which reveals directly the invariance of the theory against Lorentz transformations. The reason why the ordinary formalism of the quantum field theory is so unsatisfactory lies in the fact that it has been built up in a way much too analogous to the ordinary non-relativistic mechanics. In this ordinary formalism of the quantum theory of fields the theory is divided into two distinct sections: the section giving the kinematical relations between various quantities at the same instant of time, and the section determining the causal relations between quantities at different instants of time. Thus the commutation relations belong to the first section and the Schrödinger equation to the second.
>
> As stated before, this way of separating the theory into two sections is very unrelativistic, since here the concept "same instant of time" plays a distinct role.
>
> Also in our formalism the theory is divided into two sections. But now the separation is introduced in another place. In our formalism the theory consists of two sections, one of which gives the laws of behaviour of the fields when they are left alone, and the other of which gives the laws determining the deviation from this behaviour due to interactions. This way of separating the theory can be carried out relativistically.
>
> Although in this way the theory can be brought into more satisfactory form, no new contents are added thereby. So, the well-known divergence difficulties are inherited also by our theory. Indeed, our fundamental equations [(873)] admit only catastrophal solutions, as can be seen directly in the fact that the unavoidable infinity due to non-vanishing zero-point amplitudes of the fields inheres in the operator $H_{12}(P)$. Thus, a more profound modification of the theory is required in order to remove this fundamental difficulty.
>
> It is expected that such a modification of the theory would possibly be introduced by some revision of the concept of interaction, because we meet no such difficulty when we deal with the non-interacting fields. This revision would then have the result

> that in the separability of the theory into two sections, one for free fields and one for interactions, some uncertainty would be introduced. This seems to be implied by the very fact that, when we formulate the quantum field theory in a relativistically satisfactory manner, this way of separation has revealed itself as a fundamental element of the theory. (Tomonaga, 1946, p. 41–42)

So, although Tomonaga indeed discovered the Tomonaga–Schwinger equation first, he was still in 1943 far from seeing how to resolve the fundamental problems of the theory. In contrast to Tomonaga's previous call for a 'more profound modificaiton' of quantum field theory, in the special case of quantum electrodynamics Schwinger's conservative bent five years later led to the insistence on retaining the known electromagnetic interaction, with the divergences absorbed by the process of renormalization, and that expressed the new spirit of quantum electrodynamics.

As we have mentioned, in 1948, Oppenheimer arranged to have a brief note published in the *Physical Review*, summarizing the progress in quantum electrodynamics which had occurred in Japan since the end of the war (Tomonaga, 1948). This note expressed the reaction to the news of the experimental discovery of the Lamb shift (Lamb and Retherford, 1947), and Bethe's (1947) and Schwinger's (1948a) theoretical contributions. Tomonaga first reported on his group's unsuccessful attempt (Ito, Koba, and Tomonaga, 1947; 1948) to use the method of compensation (Pais, 1947; Sakata, 1947). Then, after seeing the work of Bethe (1947), the Japanese theoreticians were able to absorb infinities into a reinterpretation of the mass and charge of the electron, i.e., renormalization of these physical quantities. However, they made an error, and found additional divergences in the e^2-correction to the Klein–Nishina formula for Compton scattering. As Oppenheimer remarked in an attached comment, 'From manuscripts kindly sent by Tomonaga, I would conclude that the difficulties referred to in this note result from an insufficiently cautious treatment, and therefore inadequate identification, of light-quantum self-energies.' Tomonaga's letter concluded with a statement that a calculation of the Lamb shift was in progress (by Yoichiro Nambu), which included the anomalous magnetic moment effect found by Schwinger (1948a). By September 1948, Tomonaga's group had reproduced the correct relativistic Lamb shift calculation of French and Weisskopf (1949), albeit using nonrelativistic approximation (Fukuda, Miyamoto, and Tomonaga, 1949); their paper appeared in 1949, as did Schwinger's (1949b) and Feynman's (1949b) papers on the Lamb shift, in which a relativistic treatment was attempted (see above).

(m) Feynman's Preparation up to 1947

Much has been written about Feynman's accomplishments, both at the popular level (Gleick, 1992) and from the scholarly point of view. For the latter, we refer the reader to Mehra's biography (1994) and to Schweber's book (1994). As we will see, Feynman's approach to quantum electrodynamics seemed to be totally different from that of Schwinger or Tomonaga, or indeed from that of any of the field theorists of the 1930s and 1940s. His approach was far more intuitive (to him at least), less mathematical (on the surface anyway), and apparently revolutionary (as opposed to Schwinger's conservative road); yet, remarkably, as both Feynman and Schwinger came to realize in 1948, the two procedures were equivalent; Freeman Dyson proved that in 1949.

Richard Feynman started on his unorthodox path at Princeton, while working on his Ph.D. with John Archibald Wheeler. Already while he was an undergraduate at MIT, he was concerned with the infinities of electrodynamics, in particular, the infinite self-action of the electron on itself. Perhaps, he thought, one could just impose a rule that a given electron does not interact with itself. But that would not be correct, because radiation reaction, which must be present to preserve the energy balance between the electron and the electromagnetic field, would then not occur either. Feynman and Wheeler got the idea that the self-action could be eliminated by making what seemed like an outrageous change in the boundary conditions of ordinary classical electrodynamics. Instead of having only retarded waves, in which the waves reach the observer from the past, they proposed having a classical electrodynamics in which one had half-retarded and half-advanced waves, waves which come from the future. This had the theoretical advantage of being time-symmetric, that is, invariant under the change of the sense of the flow of time, from past to future, to future to past, so that the boundary conditions in time mirror the symmetry to Maxwell's equations. It was not quite so simple as that, in that perfectly absorbing boundaries had to be assumed as well. But then radiation reaction could be accounted for, as Feynman noted later: 'It became clear that there was the possibility that if we assume all actions are via half-advanced and half-retarded solutions of Maxwell's equations and assume that the sources are surrounded by material absorbing all the light which is emitted, then we could account for radiation resistance as direct action of the absorber acting back by advanced waves on the source.' (Feynman, 1966, p. 33)

Early in the collaboration between Wheeler and Feynman, an idea occurred to Wheeler that would be very important for Feynman's later thinking about quantum electrodynamics. The question was why do all electrons possess the same mass and charge? 'Because,' said Wheeler in 1940, 'they are all one and the same electron.' (Feynman Interviews and Conversations with Jagdish Mehra, January 1988) By this, Wheeler meant that there was only one worldline of an electron, which zigzagged, sometimes going forward in time, in which case it was an electron, and sometimes going backwards in time, in which case, it was a positron, with the same mass as the electron but with opposite charge. Feynman doubted there was but one such electron in the world (if so, the number of electrons and positrons would seem to have to be the same, manifestly in contradiction to experience), but he very much liked the idea that a positron was merely an electron going backward in time. It seemed to be a much more attractive idea than

Dirac's holes in a filled electron sea. This notion would play a special role in Feynman's diagrammatic interpretation of quantum electrodynamics at the end of the decade. (See Fig. 1 below.)

The next step in Feynman's journey was the principle of least action. The action, for a single particle with coordinate $q(t)$, is given by the integral

$$S[q(t)] = \int_{t_1}^{t_2} \mathscr{L}(\dot{q}, q, t)\, dt, \tag{874}$$

where t_1 and t_2 are the initial and final times and $\mathscr{L}$ is the Lagrangian of the system. The classical stationary action principle states that the trajectory of the particle is such that the action S is an extremum, which yields the Lagrange equation,

$$\frac{\partial \mathscr{L}}{\partial q} - \frac{d}{dt}\frac{\partial \mathscr{L}}{\partial \dot{q}} = 0. \tag{875}$$

These equations could be immediately extended to a system described by an arbitrary number of generalized coordinates, q_a.

Feynman's inspiration for the quantum theory, as had Schwinger's, came from Dirac. In this case, it was his paper 'The Lagrangian in Quantum Mechanics' (Dirac, 1933), a paper (which we have repeatedly mentioned) which would be the springboard for Schwinger's later action-principle based field theory.[1145] In that paper, Dirac stated that the transformation function in the coordinate representation between two different times, $(x_{t_2}|x_{t_1})$, 'is analogous to' $\exp[(i/\hbar)S(x_2, t_2; x_1, t_1)]$, the exponential factor being the action carrying a particle from an initial position x_1 at time t_1 to a final position x_2 at time t_2. No one, including Dirac, seemed to know what 'analogous to' meant in this case. Perhaps, Feynman thought in 1941, that it meant 'equal or [rather] proportional to.' Thus was born Feynman's famous path integral.

In fact the transformation function and $e^{iS/\hbar}$ were proportional, if the time interval were short, $t_2 - t_1 \ll t_1$. To calculate the transformation function

[1145] Schwinger would later remark:

> Dirac was central to this in the connection between quantum mechanics and classical mechanics, shall we say. Action in general. There are two different ways of looking at it. Feynman picked up the integral part of it in which you combine little steps in time into an integral formulation. I picked up another remark in that very same paper, namely the differential aspect, the quantum aspects and analogies with Hamilton-Jacobi and so forth. So ultimately to the extent that we finally diverged with attitudes about reformulations of quantum mechanics—which is what I think this is all really about—we were both inspired by Dirac, but took two different avenues, which are equivalent in limited contexts. I like to think that the differential aspect is more fundamental, because it is not based on mimicking of a classical situation. If everything is classical, then what do you do about non-classical degrees of freedom, like Fermi-Dirac fields and spins and such things. Whereas the differential aspect allows both possibilities, it is not so confining in the nature of the system to which it refers. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

$K(X,T;x,t)$ that carries one from a wavefunction $\psi(x,t)$ to a wavefunction $\psi(X,T)$ required breaking up the interval into a great many steps, say, N, and integrating over each intermediate position (see Eq. (793)):

$$K(X,T;x,t) = \int \exp\left[\frac{i}{\hbar}\sum_{i=0}^{N-1}\mathscr{L}\left(\frac{x_{i+1}-x_i}{t_{i+1}-t_i}, x_{i+1}\right)(t_{i+1}-t_i)\right]\frac{dx_N}{A_N}\ldots\frac{dx_i}{A_i}, \tag{876}$$

where A's are some constants, which can be easily worked out in simple cases, but which are usually irrelevant. One is supposed to take the limit as the number of intervals N goes to infinity, at the same time as the size of all time intervals goes to zero; in that sense, it resembles the definition of a Riemann integral.

In 1942, Feynman wrote his Ph.D. thesis which consisted of the work on the new approach to quantum mechanics and the action-at-a-distance electrodynamics; these were only fully described after the war, the first in the joint paper with Wheeler (Wheeler and Feynman, 1945), and the second in an article by himself, entitled 'Space-Time Approach to Nonrelativistic Quantum Mechanics' (Feynman, 1948a)—the contents of which we have outlined earlier in Subsection I.1(b). He then spent full time on war work, and soon, after his marriage to Arline Greenbaum, departed for Los Alamos where he became in charge of the Theoretical Computation Group under Hans Bethe. It would not be until he was well settled as a professor at Cornell, in 1946, that he would again resume fundamental research. But his debt to his thesis advisor, John Wheeler, with his tremendous geometrical way of thinking, was incalculable, for it would lead to Feynman's space-time view of quantum electrodynamics.

(n) Richard Feynman after the Shelter Island Conference (1947–1950)

Like Schwinger, Feynman was excited by the experimental results announced at the Shelter Island Conference in June 1947. He set to work, and by the time of the Pocono Conference the following March, he, like Schwinger, had a relativistically invariant computational scheme. We have already described Feynman's presentation at the Pocono Conference. But that conference belonged to Schwinger, and Feynman's unconventional approach was not received with much favour. He realized that only through the publication of his work could he hope to convince the community that he was on the right track.

As we have mentioned, at the January 1948 American Physical Society meeting in New York, after Schwinger's famous repeated lecture on the anomalous magnetic moment and the preliminary unsatisfactory situation with the relativistic Lamb shift calculation, Feynman got up and stated that he agreed with Schwinger's results, but he, unlike Schwinger, had the correct value of the anomalous magnetic moment for an electron in the atom. (Actually, the discrepancy was with the corresponding electrical coupling obtained from the magnetic one by a relativistic transformation.) He was at that time feeling a tremendous sense of competition with Schwinger, who had got a head start on him, but now Feynman felt, probably overconfidently, that he had caught up (Mehra, 1994, Chapter 12; Schweber, 1994, Chapter 8).

Feynman published two relatively short papers bearing on this subject in the summer of 1948. The first was entitled 'A Relativistic Cut-Off for Classical Electrodynamics' (Feynman, 1948b), which was an extended version of a manuscript he had written in 1941 (see Mehra, 1994, Chapter 13; Schweber, 1994, Chapter 8). This paper dealt largely with the action-at-a-distance formulation he had worked on before getting involved in the war effort, but now with a density of field quanta playing the role of a regulator, so that the energy of a particle was made finite. A similar idea was present in the second paper, 'Relativistic Cut-Off for Quantum Electrodynamics' (Feynman, 1948c). He used this to calculate the self-energy of the electron,

$$\delta\mu = \mu\frac{e^2}{\pi}\left[\frac{3}{2}\ln\frac{\lambda_0}{\mu} + \frac{3}{8}\right], \tag{877}$$

where μ is the electron mass and λ_0 is a cutoff, which, in conventional electrodynamics, would tend to infinity. This result had first been obtained by Weisskopf (1939), with Furry's help in the hole theory; it was actually given a covariant derivation by Feynman, as given above in Eq. (877), and was published five months later by Schwinger (1949a). In fact, Feynman's paper directly preceded Schwinger's 'Quantum Electrodynamics I' in the *Physical Review* (Schwinger, 1948c), which was received by the journal just about two weeks after it. Since Feynman used old-fashioned methods, which he employed in part to make it acceptable to other physicists, this paper is mainly remembered for its incorrect discussion of the relativistic Lamb shift.

The moment when Feynman achieved confidence in the power of his methods came at the January 1949 American Physical Society meeting in New York. This is the famous story of Murray Slotnick, who had spent six months in calculating a certain interaction between electrons and neutrons using either a pseudoscalar or a pseudovector interaction. The first form gave a finite result, while the second was divergent. After his talk, Oppenheimer challenged Slotnick: 'What about Case's theorem?' Ken Case, a former student of Schwinger's, who was then a postdoc at the Institute for Advanced Study, had a proof that the pseudovector and pseudoscalar theories were equivalent. Feynman was intrigued, so he talked to Slotnick, and that very evening he worked out the general result for an arbitrary momentum transfer. When he talked to Slotnick the next day, Feynman found that Slotnick only had the result for zero momentum transfer. But in that limit they agreed. Feynman was ecstatic: 'That was the moment when I got my Nobel Prize, when Slotnick told me he had been working [on the problem] for two years. When I got the real prize, it was really nothing, because I already knew I was a success.' (See Mehra, 1994, pp. 268–269; Schweber, 1994, pp. 454–456) Later, after he learned the meaning of creation and annihilation operators, Feynman found the error in Case's theorem (Case, 1949a, b).

Feynman's substantial papers on quantum electrodynamics appeared in 1949. These were 'The Theory of Positrons' (Feynman, 1949a), received by *Physical Review* on 8 April 1949, and 'Space-Time Approach to Quantum Electrodynamics' (Feynman, 1949b), received a month later. The validity of the rules given in these two papers was demonstrated in a third paper, 'Mathematical Formulation of the Quantum Theory of Electromagnetic Interactions' (Feynman, 1950), which arrived at *Physical Review* over a year later, on 8 June 1950. (All three of these papers are reprinted in Schwinger's collection (Schwinger, 1958).)

'The Theory of Positrons' is summarized in the abstract:

> The problem of the behavior of positrons and electrons in given external potentials, neglecting their mutual interaction, is analyzed by replacing the theory of holes by a reinterpretation of the solution of the Dirac equation. It is possible to write down a complete solution of the problem in terms of boundary conditions on the wave function, and this solution contains automatically all the possibilities of virtual (and real) pair formation and annihilation together with the ordinary scattering processes, including the correct relative signs of the various terms.
>
> In this solution, the "negative energy states" appear in a form which may be pictured (as by Stueckelberg [1942]) in space-time as waves traveling away from the external potential backwards in time. Experimentally, such a wave corresponds to a positron approaching the potential and annihilating the electron. A particle moving forward in time (electron) in a potential may be scattered forward in time (ordinary scattering) or backward (pair annihilation). When moving backward (positron) it may be scattered backward in time (positron scattering) or forward (pair production). For such a particle the amplitude for transition from an initial to a final state is analyzed to any order in the potential by considering it to undergo a sequence of such scatterings.
>
> The amplitude for a process involving many such particles is the product of transition amplitudes for each particle. The exclusion principle requires that antisymmetric combinations of amplitudes be chosen for those complete processes which differ only by exchange of particles. It seems that a consistent interpretation is only possible if the exclusion principle is adopted. The exclusion principle need not be taken into account in intermediate states. Vacuum problems do not arise for charges which do not interact with one another, but these are analyzed nevertheless in anticipation of application to quantum electrodynamics.
>
> The results are also expressed in momentum-energy variables. Equivalence to the second quantization theory of holes is proved in an appendix. (Feynman, 1949a, p. 749)

Feynman began by considering a classical picture of pair production at time t_1, followed by positron annihilation. An electron–positron pair is produced at time t_1, after which two world lines, corresponding to the electron and positron, advanced forward in time. At some later time t_2, the positron is annihilated by another electron. The picture might be as sketched in Fig. 1. As he says, 'Following the charge rather than the particles corresponds to considering this continuous

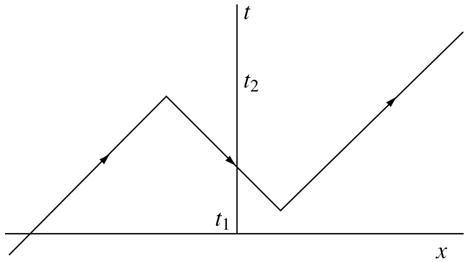

Figure 1: Space-time diagram of electron-positron pair production, followed by annihilation of the positron by another electron. The arrows pointing in an upward sense denote electrons moving forward in time, while arrows pointing in a downward sense denote electrons moving backward in time, or positrons moving forward in time.

world line as a whole rather than breaking it up into pieces. It is as though a bombardier flying low over a road suddenly sees three roads and it is only when the two of them come together and disappear that he realizes that he has simply passed over a long switchback in a single road.' (Feynman, *loc. cit.*)[1146]

Feynman went on to consider the Green's function for Schrödinger's equation, which he defined as relating the wave function at two different space-time points:

$$\psi(\mathbf{x}_2, t_2) = \int K(\mathbf{x}_2, t_2; \mathbf{x}_1, t_1)\psi(\mathbf{x}_1, t_1)\, d^3\mathbf{x}_1. \tag{878}$$

He proceeded to solve the Dirac equation for a particle of mass m in an external potential A_μ (here, he used the notation $\mathbf{A} = \gamma_\mu A_\mu$, $\mathbf{\nabla} = \gamma_\mu \partial_\mu$)

$$(i\mathbf{\nabla} - m)\psi = \mathbf{A}\psi \tag{879}$$

in terms of the Green's function, which satisfies

$$(i\mathbf{\nabla}_2 - \mathbf{A}(2) - m)K_+^{(A)}(2,1) = \mathrm{i}\delta(2,1). \tag{880}$$

Here, Feynman had adopted a compressed notation, in which the numbers 2 and 1 stand for the space-time coordinates with the respective index. It is clear that this differential equation is equivalent to the integral equation,

$$K_+^A(2,1) = K_+(2,1) - i\int K_+(2,3)\mathbf{A}(3)K_+^{(A)}(3,1)\, d\tau_3, \tag{881}$$

[1146] In an interview with Schweber, Feynman stated that this metaphor 'was suggested to me by some student at Cornell (who had actually been a bombardier during the war) when I was writing up the paper and was asking for opinions of how to explain it and only had poor or awkward metaphors.' Feynman, in Schweber, 1994, p. 656) where the Green's function without the superscript is a solution to Eq. (880) with $A = 0$. The subscript here refers to the appropriate boundary conditions in time. In order that Feynman's theory be equivalent to the hole theory, he had to choose the free Green's function so that it involved a sum over positive energy states for positive time differences, and a sum over negative energy states for negative time differences:

$$K_+(2,1) = \sum_{\text{pos } E_n} \phi_n(2)\bar{\phi}_n(1) \exp[-iE_n(t_2 - t_1)], \quad t_2 > t_1,$$

$$= - \sum_{\text{neg } E_n} \phi_n(2)\bar{\phi}_n(1) \exp[-iE_n(t_2 - t_1)], \quad t_2 < t_1. \tag{882}$$

Here, ϕ_n is an eigenfunction of the free Dirac Hamiltonian, with energy E_n, and $\bar{\phi}_n = \phi_n^*\beta$ is the Dirac conjugate.

Quantum electrodynamics proper was the subject of the second paper, 'Space-Time Approach to Quantum Electrodynamics' (Feynman, 1949b).[1147] The first paragraph of the abstract gives a good summary:

> In this paper two things are done. (1) It is shown that a considerable simplification can be attained by writing down matrix elements for complex processes in electrodynamics. Further a physical point of view is available which permits them to be written down for any specific problem. Being simply a restatement of conventional electrodynamics, however, the matrix elements diverge for complex processes. (2) Electrodynamics is modified by altering the interaction of electrons at short distances. All matrix elements are now finite, with the exception of those relating to problems of vacuum polarization. The latter are evaluated in a manner suggested by Pauli and Bethe, which gives finite results for these matrices also. The only effects sensitive to the modification are changes in mass and charge of the electrons. Such changes could not be directly observed. Phenomena directly observable, are insensitive to the details of the modification used (except at extreme energies). For such phenomena, a limit can be taken as the range of the modification goes to zero. The results then agree with those of Schwinger. A complete, unambiguous, and presumably consistent, method is therefore available for the calculation of all processes involving electrons and photons. (Feynman, *loc. cit.*, p. 769)

In this paper, Feynman then gave the famous Feynman rules and Feynman diagrams. These may well be illustrated in the momentum-space diagram, representing the 'interaction of an electron with itself,' shown in Fig. 2. This diagram has a precise mathematical correspondence with a quantum-mechanical amplitude,

[1147] In a remarkable demonstration of how close the competition was between Feynman and Schwinger, this paper appeared in the *Physical Review* directly before Schwinger's 'QED. III,' which was received 17 days later, on 26 May 1949. We recall that Feynman's 'Relativistic Cut-Off in Quantum Electrodynamics' had also appeared directly before Schwinger's 'QED. I,' which again was received by the journal exactly 17 days after Feynman's paper, on 29 July 1948.

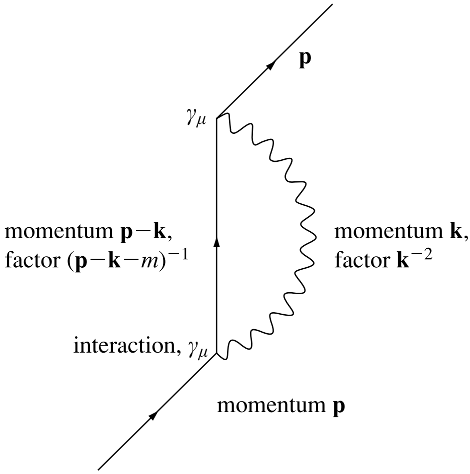

Figure 2: Feynman diagram representing the electron self-energy, in momentum space.

in this case, the divergent integral,

$$\frac{e^2}{\pi i}\int \gamma_\mu(\mathbf{p}-\mathbf{k}-m)^{-1}\gamma_\mu \mathbf{k}^{-2}\, d^4k. \tag{883}$$

Indicated in the figure are the various factors that are assembled in order to construct the amplitude (883). As he stated, Feynman's second step was a modification of electrodynamics so that these integrals would be rendered convergent. He did this, in effect, by modifying the photon propagator $1/k^2$ by multiplying it with a convergence factor $C(k^2)$ which falls off at least as fast as $1/k^2$, so now integrals such as (883) converge. For instance, we could take $C(k^2) = \lambda^2/(\lambda^2 - k^2)$, which tends to unity as $\lambda \to \infty$. (Actually, Feynman proposed averaging over a weight function $G(\lambda)$, with the property over a weight $\int_0^\infty \lambda^2 G(\lambda)\, d\lambda = 0$.) Doing so for the case of the process here, given by Eq. (883), gave a result for the electron mass shift exactly of the form (877) as given first by Feynman and then by Schwinger the year before.

Feynman next considered radiative corrections to scattering, in particular, the Lamb shift. There, he admitted the error he had previously published in the 'Relativistic Cut-Off for Quantum Electrodynamics' (Feynman, 1948c). He recounted the story in the famous footnote 13:

> That the result given in *B* [Feynman, 1948c, Eq. (19)] was in error was repeatedly point out to the author, in private communication, by V. F. Weisskopf and J. B. French, as their calculation, completed simultaneously with the author's in 1948, gave a different result. French has finally shown that although the expression for the radiationless scattering … is correct, it is incorrectly joined onto Bethe's non-relativistic result. He shows that the relation $\ln 2k_{\max} - 1 = \ln \lambda_{\min}$ used by the

author should have been $\ln 2k_{\max} - 5/6 = \lambda_{\min}$. This results in adding a $-1/6$ to the logarithm B, (Eq. (19), so that the result now agrees with that of J. B. French and V. F. Weisskopf (1949) and N. M. Kroll and W. E. Lamb (1949). The author feels unhappily responsible for the very considerable delay in the publication of French's result occasioned by this error. This footnote is appropriately numbered. (Feynman, 1949a, pp. 777–778)[1148]

However, Feynman also faced a real difficulty with vacuum polarization. His 'regularization' scheme did nothing to remove the divergence associated with a closed electron loop, as given by the amplitude

$$J_{\mu\nu} = -\frac{e^2}{\pi i} \int \mathrm{Sp}[(\mathbf{p} + \mathbf{q} - m)^{-1} \gamma_\nu (\mathbf{p} - m)^{-1} \gamma_\mu] \, d^4 p, \tag{884}$$

where $\mathrm{Sp} = \mathrm{Spur}$ is the old notation for trace. He continued to suggest that perhaps such closed loops did not exist, harking back to his collaboration with Wheeler, and the suggestion that there be but one electron in the universe. That view made the idea of closed loops 'unnatural.' Of course, Schwinger knew better,[1149] as did Feynman. He realized that in the hole theory they were necessary for probability conservation. He suggested that the Lamb shift measurement be sufficiently improved so that the vacuum polarization contribution, which amounted to -27 MHz compared to a total splitting of 1050 MHz, could be experimentally confirmed. Finally, he did discuss a method of regularizing vacuum polarization which he attributed (without reference) to Bethe and Pauli. This evidently was the Pauli–Villars technique (1949), which Feynman called 'the superposition of the effects of quanta of various masses (some contributing negatively)' (Feynman, *loc. cit.*, p. 780). This gives rise to a renormalization of the charge, again depending logarithmically on a cutoff λ,

$$\frac{\Delta e^2}{e^2} = -\frac{2e^2}{3\pi} \ln \frac{\lambda}{m}. \tag{885}$$

Feynman closed the paper by discussing spin-0 particles and meson theory in this language. This was a payoff from the Slotnick episode. He was able to repro-

[1148] According to footnote 8 in an earlier published paper of Dyson (1949b, p. 1744), it was Schwinger who detected the incorrect use of the insertion of a 'photon mass' to match the high-energy with the low-energy contributions to the Lamb shift.

[1149] Schwinger remarked:

Vacuum polarization means no more than that an electron-positron combination is coupled to the electromagnetic field and it may show itself really or virtually as you like. Schwinger knew that vacuum polarization was real from his work with Oppenheimer at Berkeley (1939). And his work on classical electrodynamics was invaluable: As with the resistive and reactive parts in synchrotron radiation, "the overtly physical and the implicitly physical parts are all connected together, you don't keep one and throw the other away. In other words, I had lots of preparation in other areas of physics, I'm not sure Feynman did. He was too abstract." (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988) duce all sorts of meson-theoretic calculations using his rules to order g^2 very easily, much to his delight. But comparison with experiment was not very fruitful, because of the largeness of the coupling.

The next paper on the 'Mathematical Formulation of Quantum Theory of Electromagnetic Interaction' (Feynman, 1950) was designed to justify the space-time procedure given in the previous papers, and supply the 'proof of the equivalence of these rules to the conventional electrodynamics.' (Feynman, *loc. cit.*, p. 440) In fact, the first four sections of this paper were written in 1947, much of which duplicated the work of Feynman's thesis, Chapter 8. It was followed a year later by 'An Operator Calculus Having Applications in Quantum Electrodynamics' (1951), which was completed while Feynman was on leave of absence in Brazil, before taking up his new appointment as a Professor of Theoretical Physics at Caltech. As Feynman said:

> Dates don't mean anything. It was published in 1951, but it had all been invented by 1948. (Feynman, Interviews and Conversations with Jagdish Mehra, January 1988)

This last paper remains of some interest.[1150] Feynman began by discussing the ordering of operators, in particular, the meaning of e^{A+B} when A and B are non-commuting. The question is, how is this 'disentangled' into its dependence on the individual operators, for only if A and B commute is it equal to $e^A e^B$. This is a subject for which the work of Schwinger is justly famous.[1151] Feynman went on to apply his calculus to quantum mechanics, in particular, to a system coupled to a harmonic oscillator, and to field theory, quantum electrodynamics in particular. He supplied his own derivation of the Tomonaga–Schwinger equation (821). He used his procedure to supply 'an independent deduction of all the main formal results in quantum electrodynamics, by use of the operator notation.' (Feynman, 1951, p. 119) He then rederived the quantum-mechanical amplitudes for processes he had computed by his intuitive technique (Feynman, 1949b). As he remarked later: 'With this paper I had completed the project on quantum electrodynamics. I didn't have anything remaining that required publishing. In these two papers (Feynman, 1950; 1951), I put everything I had done and thought should be published on the subject. And that was the end of my published work in the field.' (Feynman, Interviews and Conversations with Jagdish Mehra, January 1988)

Feynman left the field of quantum electrodynamics in triumph, but personally he was dissatisfied. He thought that he would solve the problem of the divergences in the theory, that he would 'fix' the problem, but he didn't. 'I invented a better way to figure, but I hadn't fixed what I wanted to fix. I had kept the relativistic invariance under control and everything was nice ... but I hadn't fixed any-

[1150] According to the *Science Citation Index 1997* (ISI, Philadelphia, 1998), this paper still had a very respectable 19 citations in 1997 alone.

[1151] The general problem was discussed in an appendix to a paper Schwinger wrote with Robert Karplus (1948), with the unlikely title of 'A Note on Saturation in Microwave Spectroscopy,' received by *Physical Review* on 9 January 1948 (Karplus and Schwinger, 1948).

thing. . . . I wasn't satisfied at all' (Feynman, in Schweber, 1994, p. 457) In fact, in 'Space-Time Approach to Quantum Electrodynamics,' Feynman apologized for not having solved the problem: 'The desire to make the methods of simplifying the calculation of quantum electrodynamic processes more widely available has prompted this publication before an analysis of the correct form for the [cutoff function] f_+ is complete.' (Feynman, 1949b, p. 778) He was also disappointed that his space-time picture of electrodynamics was not really new, that it was, in fact, equivalent to the conventional field theory of Schwinger and Tomonaga. He had hoped to eliminate fields entirely as fundamental entities in favour of particles, but field theory had triumphed in the end.

(o) Freeman Dyson and the Equivalence of the Radiation Theories of Schwinger, Tomonaga, and Feynman (1949–1952)

As we have mentioned, already in 1948—although the proof was only published in 1950 (Feynman, 1950)—Feynman had shown, to his own satisfaction, the equivalence of his space-time approach to quantum electrodynamics, and the more conventional, yet equally brilliant, canonical approach of Schwinger. But Feynman never received the credit for this demonstration, largely because of his slow publication schedule. In fact, it is invariably Freeman Dyson who is credited with proving the equivalence of the two, seemingly very different, approaches to quantum field theory.

We have earlier recounted Dyson's interactions with Schwinger and Feynman. When Bethe showed Dyson the letter Tomonaga had written to Oppenheimer, Dyson was delighted, for he found Tomonaga's exposition transparent, whereas the notes from Schwinger's lectures at Pocono seemed complicated, and penetrable only by the master himself (Schweber, 1994, pp. 501–502). Dyson attended Schwinger's lectures at the Michigan Summer School in 1948, and found them 'unbelievably complicated.' Dyson felt that Schwinger' approach 'couldn't be the way to do it,' for it was 'something that needed such skills that nobody besides Schwinger could do it. If you listened to the lectures you couldn't see the motivation; it was all hidden in this wonderful apparatus.' (Dyson, quoted in Schweber, 1994, p. 504) In contrast, by this time, he was already on very friendly terms with Feynman, with whom he had driven across the country. So before he took up his new residence at the Institute for Advanced Study in Princeton, he had already established his allegiance.

Dyson saw early on, perhaps more explicitly than did either Feynman or Schwinger, the connection between the two methodologies. What is remarkable is that he published his paper, 'The Radiation Theories of Tomonaga, Schwinger, and Feynman' (Dyson, 1949a) and 'The *S* Matrix in Quantum Electrodynamics' (Dyson, 1949b), received by *Physical Review* on 6 October 1948, and 14 February 1949, respectively, well before Feynman's central paper, 'The Theory of Positrons' (Feynman, 1949a), was received on 8 April 1949. Moreover, the first appeared before Schwinger's '*QED*.II' (1949a), which established the divergence structure of the theory, and both before '*QED*.III,' (1949c), Schwinger's definitive paper in the triad. It could be argued that Dyson's alacrity in publication ensured his place in history, whereas had he published after the principals had completed their expositions, his contributions would have appeared relatively minor.

In his first paper, Dyson started from the Tomonaga–Schwinger equation (821), which makes reference to the interaction representation. He then gave a perturbative solution to that equation for the time-evolution operator in powers of the interaction Hamiltonian.[1152] This expansion is, in general, only possible for that part of the interaction referring to the coupling of matter to the radiation field, given by Eq. (822). He then went on to contrast, and relate, the approaches of Schwinger and Feynman. The former is characterized by an operator which 'represents the interaction of a physical particle with an external field, including radiative corrections' (Dyson, 1949a, p. 489), which may be expressed in terms of 'characteristic' repeated commutators:

$$H_T(x_0) = \sum_{n=0}^{\infty} \left(\frac{i}{\hbar c}\right)^n \int_{-\infty}^{\sigma(x_0)} dx_1 \int_{-\infty}^{\sigma(x_1)} dx_2 \ldots$$

$$\int_{-\infty}^{\sigma(x_{\mathrm{n}-1})} dx_n \times [H^I(x_n), [\ldots, [[H^I(x_2), [H^I(x_1, H^e(x_0)]] \ldots]]. \tag{886}$$

Here, H^I denoted the interaction Hamiltonian (822) with a mass-shift term removed,

$$H^I(x) = -\frac{1}{c} j_\mu(x) A_\mu(x) - \delta m c^2 \bar{\psi}(x) \psi(x), \tag{887}$$

and H^e constituted the remaining part of the interaction Hamiltonian, for example, the interaction to the Coulomb field of the nucleus. In Dyson's perhaps critical words, 'The repeated commutators in this formula are characteristic of the Schwinger theory, and their evaluation gives rise to long and rather difficult analysis.' (Dyson, *loc. cit.*, p. 491)[1153] (In a note 'To Section V' added in proof, Dyson noted on p. 502 that he had given an incorrect interpretation of Schwinger's formulation, and in fact Schwinger's approach, like Feynman's, is symmetric be-

[1152] For the details of Dyson's elaboration of the theories of Feynman, Schwinger, and Tomonaga, see Mehra (1994, Chapter 15, Section 15.5, pp. 314–318).

[1153] To which Schwinger responded:

> Well, it wasn't so long and it wasn't so difficult, but nevertheless it was not the most economical way of going on to higher-order effects. That I not only grant, but I insist on.... Dyson did recognize that, as I think Feynman probably didn't, that the Feynman theory does operate with a statement about initial and final states, which is a concentration on the overall evolution of the system. And that was a useful thing. No question about it. And as soon as I understood that, I immediately incorporated it into my own next version as well. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

tween past and future.) But Dyson's main point in his investigations was not an explication of Schwinger's methods, but of Feynman's.

As a key innovation, he introduced a time-ordering operator P: 'If

$$F(x_1), \ldots, F_n(x_n) \qquad [(888)]$$

are any operators defined, respectively, at the points $(x_1), \ldots, (x_n)$ of space-time, then

$$P(F_1(x_1), \ldots, F_n(x_n)) \qquad [(889)]$$

will denote the product of these operators, taken in the order, reading from right to left, in which the surfaces $\sigma(x_1), \ldots, \sigma(x_n)$ occur in time.' (Dyson, *loc. cit.*, p. 492) The Feynman theory was then given in terms of a time-ordered product of interaction operators:

$$H_F(x) = \sum_{n=0}^{\infty} \left(\frac{-i}{\hbar c}\right)^n \frac{1}{n!} \int_{-\infty}^{\infty} dx_1 \ldots \int_{-\infty}^{\infty} dx_n P(H^e(x_0), H^I(x_1), \ldots H^I(x_n)). \qquad (890)$$

Dyson went on to calculate matrix elements. In so doing, he used his time-ordering notation to define the 'Feynman' propagators for the photon [he called D_F 'the type of D function introduced by Feynman' (Dyson, *loc. cit.*, p. 494)], and similarly S_F for the electron, i.e.,

$$\langle P(A_\mu(x), A_\nu(y))\rangle_0 = \frac{1}{2}\hbar c \delta_{\mu\nu} D_F(x-y),$$

$$\langle P(\psi_\alpha(x), \psi_\beta(y))\rangle_0 = \frac{1}{2}\eta(x,y) S_{F\alpha\beta}(x-y), \qquad (891)$$

where $\eta(x,y)$ is ± 1 depending on whether $\sigma(x)$ is later than or earlier than $\sigma(y)$, and the subscripts on fermion fields are Dirac indices. In terms of these, Dyson was able to derive Feynman's diagrammatic rules.

Note that in fact Dyson had made a major break with Feynman, who insisted on the particle nature of electrons, while Dyson, like Schwinger and Tomonaga, saw everything as fields. However: 'Nobody at Cornell understood that the electron field was a field like the Maxwell field. That was something in Wentzel (1943/1949) but was nowhere else. That was what was lacking in the old-fashioned way of calculating. The electron was a particle, the photon was a field, and the two were just totally different. This notion of just two interacting fields with the simple interaction term $\bar{\psi}\gamma_\mu A^\mu \psi$ was essentially what I brought to Cornell with me from England out of Wentzel's book.' (Dyson, in Schweber, 1994, p. 508)

This paper appeared shortly after Dyson assumed his visiting fellowship at the Institute for Advanced Study, whose director was J. Robert Oppenheimer. Dyson was invited to present several seminars on his work. Oppenheimer, although initially expressing interest, was very hostile until Hans Bethe intervened; then Oppenheimer capitulated and became a believer. But Dyson was not happy with him: ‘Oppenheimer was a great disappointment. He hadn’t time for details. As compared to Hans Bethe, Oppenheimer was completely superficial. To talk to Oppenheimer was interesting. It was like meeting some very famous person who had interesting things to say but I just never got anything that you could call guidance. I wasn’t needing much guidance.... He had a bad effect on other people who needed guidance more than I did.’ (Dyson, *loc. cit.*, p. 526) These remarks are not dissimilar to those of Schwinger concerning his interactions with Oppenheimer in Berkeley a decade earlier.

It was Dyson’s second paper, ‘The *S* Matrix in Quantum Electrodynamics’ (Dyson, 1949b) that assured his fame. In this paper, he recast Schwinger’s and Feynman’s electrodynamics into what has become the standard form. As Dyson stated in the introduction:

> The present paper deals with the relation between the Schwinger and Feynman theories when the restriction to one-electron problems is removed. In these more general circumstances, the two theories appear as complementary rather than identical. The Feynman method is essentially a set of rules for the calculation of elements of the Heisenberg *S* matrix corresponding to any physical process, and can be applied with directness to all kinds of scattering problems. The Schwinger method evaluates radiative corrections by exhibiting them as extra terms appearing in the Schrödinger equation of a system of particles and is suited especially to bound-state problems. In spite of the difference of principle, the two methods in practice involve the calculation of closely related expressions; moreover, the theory underlying them is in all cases the same. The systematic technique of Feynman, the exposition of which occupied the second half of I [Dyson, 1949a] and occupies the major part of the present paper, is therefore now available for the evaluation not only of the *S*-matrix, but also of most of the operators occurring in the Schwinger theory. (Dyson, 1949b, p. 1736)

Dyson gave a systematic exposition of the perturbation theory of quantum electrodynamics. He did so by giving the so-called Schwinger–Dyson equations. These consisted of an infinite set of coupled integral equations for the Green’s functions of the theory. For example, the full electron and photon propagators, satisfied

$$S_F'(p) = S_F(p) + S_F(p)\Sigma^*(p)S_F'(p), \tag{892}$$

$$D_F'(p) = D_F(p) + D_F(p)\Pi^*(p)D_F'(p), \tag{893}$$

where Σ^* and Π^* denoted ‘proper electron (photon) self-energy parts,’ respectively. Although these equations are algebraic in momentum space, the self-energy parts are given by integral equations (which were not stated explicitly in Dyson’s paper, though they were given by a graphical description). For example, vacuum polarization is in general given by[1154]

$$\Pi_{\mu\nu}(q) = ie^2 \int \frac{d^4k}{(2\pi)^4} \operatorname{Tr} \gamma_\mu S'_F \Gamma_\nu(k, k+q) S'_F(k+q), \tag{894}$$

where $\Gamma_\nu(k, k+q)$ is a vertex amplitude coupling a vacuum polarization $A_\nu(q)$, corresponding to a photon with momentum q to incoming and outgoing electrons with momenta k and $k+q$, respectively, which in turn is determined by still further integral equations. The perturbative solution to this system of equations, where in the lowest order $\Gamma_\nu = \gamma_\nu$, leads to Feynman's rules for the construction of all quantum-mechanical amplitudes for computing scattering processes in *QED*.[1155]

Dyson concluded his paper by discussing renormalization. He showed that the 'true' vertex Γ_μ and the 'true' propagators S'_F and D'_F were of the form

$$\Gamma_\mu = Z_1^{-1} \Gamma_{\mu_1}(e_1),$$

$$S'_F = Z_2 S'_{F_1}(e_1) \quad \text{and} \quad D'_F = Z_3 D'_{F_1}(e_1), \tag{895}$$

where $\Gamma_{\mu_1}(e_1) S'_{F_1}(e_1)$ and $D'_F(e_2)$ denoted 'the operators obtained by the process of substitution dropping divergent terms,' and the (infinite) constants Z_1, Z_2, Z_3 were thus determined so as to give the true electronic charge $e_1 = Z_1^{-1} Z_2 Z_3^{1/2} e$ in terms of the original (bare) electronic charge e (Dyson, 1949b, p. 1750). The resulting Green's function of the theory then became entirely finite. Later, John Ward proved Dyson's conjecture (Dyson, *loc. cit.*, p. 1753) that two renormalization constants, Z_1 and Z_3, sufficed by demonstrating the 'Ward identity' $Z_1 = Z_2$, suggested by Dyon (*loc. cit.*) (Ward, 1950).

Dyson continued his contributions to field theory with a series of major papers published in 1951, dealing with what he called 'Heisenberg operators.' This was somewhat in the spirit of Schwinger's canonical transformation designed to isolate the renormalization effects, but unlike Schwinger, Dyson did not use the adiabatic (slowly varying) approximation. He gave an exposition of this programme at the Michigan Summer School in 1950. When the papers were published the following

[1154] This equation appeared explicitly in Schwinger's paper, 'On the Green's Function of Quantized Fields' (Schwinger, 1951c).

[1155] Roy Glauber recounted an embarrassing error that Schwinger made in this connection. In fall 1949, Schwinger gave a long sequence of lectures at the joint theoretical seminar hosted by Harvard and MIT on the Green's functions of quantum electrodynamics; in effect, he claimed to have found a closed integral expression for the vertex function Γ_μ. John Blatt took notes of these seminars, and they reached Normal Kroll at Columbia, who discovered a crucial error, the scattering of light by light had been inadvertently omitted. Shortly thereafter, Pauli visited Harvard from the Institute for Advanced Study, having heard of this error from Kroll, and visited Schwinger in his office. Sometime later, Schwinger emerged, 'badly shaken: Pauli was delighted to be the bearer of bad news.' Of course, in those early days, the structure of field theory was poorly glimpsed, so it is understandable that such an error could escape even the master. (Glauber, Interview with K. A. Milton, 8 June 1999) year (Dyson, 1951a, b, c, d), Dyson felt that he had made a major contribution that would 'get radiation theory moving forward again' and would allow the application of field-theory methods to meson problems also. Unfortunately for Dyson, more effective methods rapidly became available, including Schwinger's Green's function techniques (Schwinger, 1951c), so these papers had negligible impact at the time (Schweber, 1994, p. 563).[1156]

A concluding paper published by Dyson in 1952 had significant repercussions on the view of the meaning of perturbation theory in quantum field theory. This was 'Divergence of Perturbation Theory in Quantum Electrodynamics' (Dyson, 1952). There, he gave a simple argument that perturbation theory could not result in a convergent series. The argument went as follows: Suppose one computed a Green's function as a series in powers of e^2. (Apart from an overall factor, any Green's function has an expansion in powers of e^2 or α.) If the series were convergent for sufficiently small values of e^2, it would have to converge even if e^2 were small but negative. But this cannot be, for if e^2 were negative, like charges would attract, and the vacuum would be unstable to decay into an arbitrarily large number of electron–positron pairs. At best, then, perturbation theory must result in an asymptotic series, which nowhere converges, but for which a finite number of terms gives an optimal approximation to the true Green's function. This is not an obstacle in practice for quantum electrodynamics, since the coupling constant, $\alpha = 1/137$, is so small. But the proof was damaging to Dyson: 'That was, of course, a terrible blow to all my hopes. It really meant that this whole programme [of perturbative quantum field theory] made no sense.' (Dyson, in Schweber, 1994, p. 565) Nowadays, no one is seriously disturbed about the asymptotic nature of perturbation theory, although it does raise the unresolved issue of the importance of nonperturbative effects in field theories, be they quantum electrodynamics or quantum chromodynamics, the theory of strong interactions. There is also the beginning of a recognition that Dyson's argument may be wrong, because it fails to take into account boundary conditions (Schwinger, 1951c).

(p) The Impact of Dyson's Work

The predominant view of the impact of Dyson's work was beautifully stated by C. N. Yang:

> The papers of Tomonaga, Schwinger and Feynman, did not complete the renormalization program. Since they confined themselves to low-order calculations. It was Dyson who dared to face the problem of high orders and brought the program to completion. In two magnificently penetrating papers, he pointed out and resolved the main problems of this very difficult analysis. Renormalization is a program that converts additive subtractions into multiplicative renormalization. That it works required a highly nontrivial proof. That proof Dyson supplied. He defined the con-

[1156] These four papers of Dyson's had no citations in 1997 according to the *Science Citation Index 1997* (ISI, Philadelphia, 1998).

cepts of primitive divergences, skeleton graphs, and overlapping divergences. Using these concepts, he pushed through an incisive analysis and completed the proof of renormalizability of quantum electrodynamics. His perception and power were dazzling. (Yang, 1983, p. 65; quoted in Schweber, 1994, p. 529)

But the inventors of renormalized quantum electrodynamics were less than impressed. In a later interview, Schwinger expressed his view of Dyson's contributions to quantum electrodynamics. He began by referring to Feynman:

"Of course, neither you nor I needed to be told that our theories were equivalent and we didn't need Dyson," [Schwinger paraphrased Feynman]. And, of course, that was true. Dyson was writing not for us, but for the rest of the world. What Dyson contributed was ... the utility of a formal construction of that unitary operator in terms of time-ordering. There is the point that Dyson recognized that Feynman throughout was always dealing with scattering problems, that his theory in principle was incapable of dealing with bound states. Dyson recognized that I had a more complete theory. It was a Hamiltonian theory; you could deal with energy eigenvalues and so forth. Dyson did contribute something in his recognition of the importance of the time-ordering formulation. And that is what underlay the particular propagation function that Feynman and, as we know, Stückelberg before him, had introduced. From a practical point of view, I think he was simply translating his understanding of what Feynman was trying to do—and it is not clear that Feynman would necessarily have agreed with that—into the ordinary language of operators and so forth. And pointing out that the different handling of the operators would produce the Feynman results. Valuable. Not world-shaking, but valuable. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

In fact, as we have noted, Schwinger reacted positively to Dyson's introduction of time-ordering, recognizing its superiority. 'If you look at my own work you will see not time-ordering, but a concern with symmetrical and antisymmetrical products. Therefore, two functions. Whereas the complex time-ordered [propagation] function ultimately turns out to be the more convenient thing.' (Schwinger, *loc. cit.*) In Schwinger's view, it was fortunate that Dyson's paper (1949a) was published before Feynman's (1949a):

Feynman's paper published by itself would probably never have communicated very well. Dyson recognized what quantum-mechanical formulation Feynman was implicitly using, which was very valuable because nobody else could possibly have understood it without that recognition. Dyson's papers were useful as one of the gospels, the interpretation of the mystical words to the masses. (Schwinger, *loc. cit.*)

But Schwinger was unhappy at the success of the Feynman–Dyson approach: 'I confess it utterly astonished me that his method became so popular. That, of course, was not Feynman's doing but Dyson's. Without Dyson using my language to translate Feynman it never would have been understood.' (Schwinger, *loc. cit.*)

Feynman, perhaps, had more cause for unhappiness, because Dyson's papers appeared before his own. For a while, people talked about 'Dyson graphs.' Yet Feynman was not too concerned. In later remarks, he rather commented:

> He [Dyson] wasn't trying to steal anything from me; he hadn't claimed that they were his. All he was trying to do was tell everyone that there was something good in my theory, that he had discovered the connection with the work of Tomonaga and Schwinger, and that all these different approaches were equivalent. This greatly helped people to understand the different theories. His paper had some crazy language which I couldn't understand, but others could understand it. It was like a translation of my theory, my language for other people; of course, it's a mistake to translate something for the author. I was bothered only slightly, and I would be more concerned today if they were still called "Dyson graphs." That would not make me miserable, but I would complain a little bit about it.
>
> A little later, the diagrams came to be called "Dyson-Feynman graphs," with some others calling them the "Feynman graphs" through a number of people who knew about their origins a little better. Now, of course, it is as it should be. "We write down *the* diagram for this or that process." And that's the best, because it's anonymous, it's *the* diagram. It makes me feel better than the "Feynman diagram," because it is *the* rule for something, and that's just fine. (Feynman, Interviews and Conversations with Jagdish Mehra, April 1970, and January 1988)

(q) Feynman and Schwinger: Cross Fertilization

Although Schwinger and Feynman never collaborated, and talked together only rarely, it is clear that there was a certain synergism between these two innovators who nearly simultaneously scaled the peak of electrodynamics.

They had of course rather different goals: Schwinger was interested in understanding the experimental situation, namely:

> I was concentrating on understanding these electromagnetic phenomena. I developed a formalism adequate enough to account for it, period. Feynman had something more grandiose in mind from the beginning, a reconstruction of quantum mechanics using more intuitive ideas, and these same electromagnetic problems were for him simply a way of understanding what he was trying to do. These particular problems were not the center of his interest as they were for me. They were just another bit of experimental data in order to evolve his ideas. So Feynman was aiming at a more general method to begin with, but he could not have gotten there without the concrete answers, shall I say, that I provided and which he could then adapt and on the basis of which put forward his more general method. I don't know quite how to say it except that his aim was ultimately more far-reaching, but he needed—we were complementing each other. We were not in competition. Our ambitions were different. I got to these answers very quickly, which rather contradicts the general opinion that I need very complicated incomprehensible methods. They went fast and I don't ascribe it to any particular talents that I have. The machinery was perfectly okay for the purpose that it was being invented for. Whereas Feynman was looking for something more general. (Schwinger, Interviews and Conversations with Jagdish Mehra, March 1988)

Feynman indeed influenced Schwinger to find a better method to work out higher order effects:

> Let's face it, the method that I had got clumsier and clumsier. Any method does at higher order. Perhaps a little more rapidly, which is why, when I finally realized what Feynman was trying to do, I took a look at it and went back and found a more general method myself. Which is perfectly reasonable. I'm emphasizing the point that what I did was more than adequate for the limited questions being asked, it explained the Lamb shift and the magnetic moment to the accuracy at which they were measured. When the accuracy increased, the theory had to go to higher orders. Then came the question of which way of formulating it was more efficient and something along Feynman's line was no question [more efficient] and I adapted myself to it [albeit with a differential rather than integral attitude]. [In the year 1948–1949] I was certainly deeply involved in trying to look for more general formulations and seeing what there was in the Feynman-Dyson things that I should adopt, to find a synthesis. These were clearly not so different paths, but variations on each other. [The question was] what ultimately was the best version. I spent a lot of time on that. Particularly looking at all kinds of higher-order effects, for example, the two-particle differential equation, which became known as the Bethe-Salpeter equation, which I was talking about a year earlier and describing in lectures at Harvard. That was certainly the future not the past. (Schwinger, *loc. cit.*)

But Schwinger also expressed regret that his interactions with Feynman had not been stronger:

> We were kind of moving in similar directions. It's too bad we couldn't have interacted earlier. We could have saved the world a lot of time. If he had gone to Columbia, we would have worked together at a much earlier stage. The reformulation of quantum mechanics might have occurred earlier and then that would have vastly simplified the application to electrodynamics. (Schwinger, *loc. cit.*)

1.3 New Elementary Particles and Their Interactions (1947–1964)

With quantum electrodynamics (*QED*), one finally had available the first relativistic quantum field theory, which allowed one to calculate in detail with arbitrary accuracy the various radiative processes and, in particular, accounted for the refined experimental results obtained in that field after World War II. Elated with this breakthrough—and in spite of the unusual and disturbing feature that the theory involved obviously infinite renormalization constants—the physicists immediately checked whether *QED* might also serve as a model for the theoretical description of other elementary particles, especially the nonelectromagnetic processes in which they were involved. Thus, Paul T. Matthews and Abdus Salam in England investigated the renormalizability of meson–nuclear interactions (Matthews, 1949; Salam, 1951a, b; Matthews and Salam, 1951). The Nagoya

(Japan) theoreticians around Shoichi Sakata regarded renormalization theory 'as an abstract formalism behind which the concrete structure of elementary particles lies hidden' (Sakata, Umezawa, and Kamefuchi, 1951, p. 154). They suggested a 'formal' classification of the known interactions into two groups: '(a) those interactions which can be renormalized by assuming the coexistence of a finite number of interactions of the same group and (b) those interactions which require the further introduction of infinitely many interaction terms having successively higher derivatives of the field quantities' (Sakata *et al.*, *loc. cit.*). In the first cases, such as *QED*, the renormalization procedure would give a consistent closed theory (in 1936, Heisenberg described this as 'an interaction of the first kind'); in the other cases (which Heisenberg had called 'interactions of the second kind'), the renormalization procedure actually failed. The renormalizable theories possessed dimensionless coupling parameters, and the non-renormalizable ones those having the dimension (length)$^\eta$ with $\eta > 0$ (Sakata *et al.*, 1952). Now, in the case of scalar and pseudoscalar mesons interacting with nucleons, Salam had demonstrated their renormalizability; however, this did not suffice to establish in these cases a consistent theory similar to *QED*, as Robert E. Marshak pointed out in the preface of his book on *Meson Physics*:

> The theoretical situation in meson physics is ... less encouraging for book writing: no genuine meson theory exists but only plausible conjectures which occasionally illuminate the complexities of the experimental material. Despite these formidable obstacles, the task has been undertaken for two reasons. First, many indisputable facts concerning π- and μ-mesons have been established and these seem worth recording.... Secondly, by restricting ourselves to real meson processes and omitting considerations of all nuclear phenomena which involve mesons only as virtual transitions (e.g., nuclear force), we have eliminated the most speculative and least satisfactory predictions of meson theory. (Marshak, 1952, p. V)

Even this cautious approach was criticized in another book on *Mesons and Fields* by Hans Bethe and Frederic De Hoffmann, because Marshak had made use of the so-called 'weak-coupling theory,' but:

> Since we know the coupling constant g^2 to be of the order of magnitude ten, it is clear that an expansion in powers of g is not warranted. Hence the calculations on π-mesons reported in Marshak's book should be used only to give qualitative ideas on the orders of magnitude to be expected; even in this respect caution is indicated. (Bethe and De Hoffmann, 1955, p. xii)

Now, between 1947 and 1962, the number of elementary particles increased from four to more than one hundred and even exploded further afterward. The first of these objects was announced in two notes submitted to *Nature*, dated 4 October and 20 December 1947, respectively, the so-called 'π-mesons' and the 'V-particles.' While the latter were observed with P. M. S. Blackett's cloud chamber (plus a magnetic field) in Manchester, and exhibited a mass of about 850–970 m_e

(Rochester and Butler, 1947), the former were detected with the help of the newly developed, highly sensitive, photographic emulsions and possessed a mass of about 300 m_e; the π-meson also evidently decayed into another particle of mass about 200 m_e. 'We assume that the π-mesons are, respectively, positively and negatively charged particles of the same type, which are produced in processes associated with explosive disintegration of nuclei,' as Cecil F. Powell and his collaborators in Bristol explained, and added:

> The positive π-mesons suffer μ-decay and give rise to μ-mesons.... On the other hand, the negative π-mesons ... are captured by nuclei to produce disintegrations with the emission of heavy particles. (Lattes, Occhialini, and Powell, 1947b, p. 490)

With these observations, deciphered from photographic emulsions when exposed at 5500-m height in Bolivia, the Bristol group solved two outstanding riddles of the past: They established the existence of two types of mesons, of which the heavier π's had to be associated with Yukawa's mesons that mediated the nuclear force; the lighter ones, emerging from the π-decay and thus observed essentially at lower altitudes (as the 'mesotrons' of Anderson and Neddermeyer), were found to exhibit no strong interactions with nuclei (in agreement with the experiment of Conversi *et al.*, 1947, in Rome, mentioned earlier).[1157] While the two mesons and the *V*-particles occurred in the 'natural' laboratory of cosmic rays, in 1950, Jack Steinberger, Wolfgang K. H. Panofsky, and J. Stellar produced, beside the already known charged ones, also neutral pions by using the electron–synchrotron at Berkeley; in particular, they observed two photons among the decay products of π^0, and concluded:

> Since spin-$\frac{1}{2}$ and spin-1 mesons are forbidden to decay in two mesons, the spin [of π^0] must be zero, excluding the possibility of very high intrinsic angular momenta. It seems reasonable, and is in very good agreement with all observations, to assume that both charged and neutral mesons are of the same [spin] type. It then follows from the angular distribution of the X-ray produced π^+-mesons, and the high cross sections for making neutral mesons by X-rays [from the Berkeley cyclotron] that the π-meson is a pseudoscalar. (Steinberger *et al.*, 1950, p. 805)

These initial discoveries, made until spring 1950, opened the new era of elementary particles in physics: In 1951, cloud-chamber observations yielded a new neutral lambda (Λ)-particle, which decayed weakly into a proton and a negative π-meson; between 1952 and 1953, the discovery of the negatively charged Ξ-particle and the charged Σ-particles followed, all heavier than the nucleons and detected in cosmic radiation at high altitudes. These objects (called hyperons) appeared, like the *V*-particles of Rochester and Butler, not only unexpectedly, but

[1157] Evidently, the existence of the two different mesons also solved the problem of the lifetime noticed in the late 1930s; the π-meson indeed decayed much faster than did the μ-meson.

they also exhibited an unusual feature: They were produced relatively copiously in nuclear collisions, on the one hand, and decayed with relatively long mean lifetimes of 10^{-10} to 10^{-8} s (leaving observable tracks in emulsions and cloud chambers), on the other. Hence, these so-called 'strange particles' were produced—in fact, always in pairs ('associated production')—by strong nuclear forces but decayed by weak nuclear forces (Pais, 1952). In 1953, a theoretical reason would be proposed to explain the above-mentioned behaviour of strange particles, independently by Tadao Nakano and Kazuhiko Nishijima in Japan and by Murray Gell-Mann in the United States: They introduced a quantum number S which was conserved in the associated, strong production processes and violated in the weak decays of single strange particles (Gell-Mann, 1953; Nakano and Nishijima, 1953). The quantum number S followed from a generalization of the known relation for the electric charge Q (in units of the unit charge e) of a particle, having I_3 as the third isospin component, and B as the baryon number (which was $+1$ for nucleons and hyperons and -1 for the corresponding antiparticles, and 0 for π-mesons and the V-particles), via the equation

$$Q = I_3 + \frac{1}{2}B + \frac{1}{2}S. \tag{896}$$

This additive quantum number, called 'strangeness,' assumed the values ± 1, 0, as well as other integral values; besides the strong interactions, S remained unaltered also in electromagnetic processes, whereas it was violated in weak interactions, especially in the spontaneous decay of strange particles.

By the end of the year 1953, nearly all—especially the heavier—new particles had been detected in the cosmic radiation, in general with the help of photographic-emulsion equipment (and less frequently with cloud chambers), transported into high-altitude mountains (e.g., the Pic-du-Midi in Southern France) or taken up into the atmosphere by balloons; the physicists, in their laboratories, then measured under the microscope the registered tracks created by charged particles. However, after that period devoted to cosmic-ray investigations, the particle accelerators took over.[1158] The Chicago cyclotron, built by a group of Enrico Fermi's collaborators, reached proton energies up to 450 MeV in 1951, the intense beams having a well-defined energy; hence, the investigation of production cross sections of charged and neutral π-mesons became possible, as well as the observation of the scattering of charged π-mesons by nuclear targets. Now, the theoretical predictions of Walter Heitler (1946), who had concluded the existence of a compound system of a meson (with isospin $I = 1$) and a nucleon (with $I = \frac{1}{2}$)—having a total isospin of either $\frac{3}{2}$ or $\frac{1}{2}$—and calculated the relationships between the

[1158] Earlier, we mentioned the Berkeley synchrotron used by Steinberger *et al.* to produce the neutral π-meson; even before that, Eugene Gardner and Cesar Lattes (1948) had obtained charged π^+-mesons with the 184-in cyclotron at Berkeley.

isotopic amplitudes, were tested by the Chicago group during the course of analyzing their π-meson–nucleon states.[1159] Indeed, they found such a resonance, a state having spin-$\frac{3}{2}$ and isospin-$\frac{3}{2}$, the so-called (3–3) nucleon isobar (Anderson, Fermi, Martin, and Nagle, 1953). Later on, further nucleon resonances, e.g., those with spin-$\frac{5}{2}$ (and larger) and isospin-$\frac{1}{2}$, were obtained with various accelerators. Since the completion of the Brookhaven 'Cosmotron' (in 1952), and the Berkeley 'Bevatron' (in 1954), a new generation of high-energy machines all over the world—e.g., the Dubna (since 1957), the CERN (since 1959) and the Brookhaven (since 1960) proton synchrotrons—not only copiously produced associated strange particles and the anti-proton (i.e., anti-particle to the proton, in Dirac's sense) but soon revealed an almost explosively increasing number of further strong-interaction resonances under easily reproducible conditions. The laboratories housing these large accelerators therefore assumed a privileged, dominant status in elementary-particle research, attracting numerous visiting experimental groups.[1160]

In organizing and describing the properties of ever-increasing numbers of elementary particles and their interactions, symmetry principles began to play a vital role. For instance, in the discussion of the results of the Chicago π-meson–nucleon scattering experiments, the isospin invariance of nuclear forces, discovered already in the 1930s, entered crucially. Another symmetry operation—still recognized before the war—the charge-conjugation C, would be discussed in a more general context and lead toward the mid-1950s to an important theorem that helped to characterize the fundamental forces between elementary particles. It began with Julian Schwinger's paper, 'The Theory of Quantized Fields. I,' received by the *Physical Review* on 2 March 1951, in which Schwinger proposed a general relativistic theory of localizable fields and demonstrated that 'the requirement of invariance under time reflection imposes a restriction upon the operator properties of the fields, which is simply the connection between the spin and statistics of particles' (Schwinger, 1951b, p. 914). Wolfgang Pauli, for decades the expert in the correlation of spin and statistics, knew that all his previous efforts had been restricted essentially to the case of free particles (see Pauli, 1941). He now carefully followed the further work on this problem and summarized the situation in a critical review article written for Niels Bohr's seventieth-birthday *Festschrift*, which he edited (Pauli, 1955). At many places, Pauli referred to an article by a young Göttingen theoretical physicist, Gerhart Lüders, who had analyzed systematically the concepts of time reversal (T), parity (P), and charge conjugation (C) and formulated what was later called the 'TCP-theorem' (Lüders,

[1159] It should be pointed out that Gregor Wentzel, on the basis of his strong-coupling theory (1941), had claimed the existence of excited nucleons even earlier than Heitler.

[1160] For the establishment of new accelerator centres in the 1950s, we refer to the articles of Ernest D. Courant, Donald W. Kerst, Gerson Goldhaber, Edoardo Amaldi, and Armin Hermann, in Brown, Dresden, and Hoddeson, 1989; a report on the foundation of the Institute of Nuclear Research in Dubna has been given by Birjukow *et al.*, 1960.

1954).[1161] Lüders provided a detailed proof of this theorem, stating 'that a wide class of quantized field theories which are invariant under the proper Lorentz group is also invariant with respect to the product of time reversal (T), charge conjugation (C) and parity (P)' three years later (Lüders, 1957, p. 1). This theorem not only established the spin-statistics relations for Bose and Fermi fields and their local interactions, but also served to restrict the possible forms for the interaction terms, notably, in the case of weak interactions.[1162]

In the summer of 1955, it was in particular the so-called '$\theta - \tau$ puzzle' that bothered the particle physicists; it was the existence of two heavy K-mesons (as one called the V-particles of Rochester and Butler at this time)—one, the θ, decaying into two π-mesons, the other, τ, into three π-mesons—which possessed the same mass and lifetime (Lee and Orear, 1955). Tsung Dao Lee and Chen Ning Yang then assumed that 'θ and τ have the same spin but opposite parity,' and introduced a 'parity conjugation' symmetry leading to further such parity doublets among the strange particles (Lee and Yang, 1956a). However, half a year later, Lee and Yang proposed a way out of the puzzle by claiming: 'Parity is not conserved, so that θ^+ and τ^+ are two different decay modes of the same particle, which necessarily has a single mass value and a single lifetime' (Lee and Yang, 1956b, p. 254).[1163] They therefore suggested possible tests of parity conservation

[1161] The concept of time-reversal symmetry in quantum mechanics had been formulated and first used by Eugene Wigner in atomic physics (Wigner, 1932).

Weyl's and Wigner's work immediately after the birth of quantum mechanics began the application of group theory to atomic spectroscopy. Within a year or two, Wigner had explicitly given the construction of Wigner coefficients, what we now usually call $3j$ symbols, in terms of the reduction of the product of representations $\boldsymbol{D}^{(j)} \times \boldsymbol{D}^{(j')}$ into irreducible representations. The $3j$ symbol is actually merely a symmetric way of writing the usual Clebsch–Gordan coefficient. This was all explained very transparently in his book published in 1931. The Wigner coefficients thus allow one to couple two angular momenta to form a third. However, although this is sufficient in principle, it becomes impractical in complex spectra.

It was in fact Giulio Racah's work that made the theory of complex spectra amenable to analysis. In a series of four papers he wrote in Jerusalem from 1942 to 1949, he showed how to construct matrix elements of scalar products of tensor operators in terms of new coefficients, the Racah coefficients, $W(j_1, j_2, j_3; J, J_{12}, J_{13})$. The Racah coefficients are thus essential for the application of the Wigner–Eckart theorem. As the notation suggests, it is also the transformation function of two different ways of coupling three angular momenta, j_1, j_2, j_3. Here, $\mathbf{j}_1 + \mathbf{j}_2 = \mathbf{J}_{12}$, $\mathbf{j}_1 + \mathbf{J}_{13}$, and $\mathbf{J}_{12} + \mathbf{j}_3 = \mathbf{j}_2 + \mathbf{J}_{13} = \mathbf{J}$. Thus, this is a $6j$ symbol. Modern theoretical nuclear and atomic physics is inconceivable without its introduction. Although these papers dealt exhaustively with the theory and application of these coefficients, they did not close the subject, for Regge [1958] nearly a decade later observed that the symmetry of the Wigner coefficients was much larger than expected, being that of the $9j$ symbol, which described the coupling of four angular momenta. It is also interesting to note that the work on Casimir operators of Lie groups was brought to a conclusion by Racah.

We close this brief summary by noting that Schwinger in his 1952 report on angular momentum provided a compact, physical derivation of all of the theory, including the Wigner and Racah coefficients.

[1162] For the early attempts made in this direction, see Biedenharn and Rose (1951) and Tolhoek and de Groot (1951).

[1163] In the following investigation with Reinhard Oehme (who was familiar with the details of the former work of Lüders and others on the interrelation between T, C, and P, and corrected certain misconceptions of Lee and Yang), they clarified the situation with the help of the TCP-theorem (Lee, Oehme, and Yang, 1957).

in nuclear γ-decays and meson- and hyperon-decays. Already on 15 January 1957—i.e., two-and-a-half months after the publication of the suggestion of Lee and Yang—the *Physical Review* received a report by Chien-Shiung Wu and her collaborators, namely:

> In β-decay, one could measure the angular distribution of the electrons coming from β-decays of polarized nuclei. If an asymmetry in the distribution between θ and $180° - \theta$ (where θ is the angle between the orientation of the parent nuclei and the momentum of the electrons) is observed, it provides unequivocal proof that parity is not observed in β-decay. (Wu, *et al.*, 1957, p. 1413)

By observing the decay of the Co^{60}-nuclei, and upon establishing their polarization via a subtle method available at the National Bureau of Standards (where Madame Wu's collaborators came from), they found 'a large asymmetry [which] does not change sign with the reversal of the direction of the demagnetization field, indicating that it is not caused by remnant magnetization in the sample [containing Co^{60}-nuclei embedded in paramagnetic cerium-magnesium nitrate]' (Wu, *et al.*, *loc. cit.*, p. 1414). At the same time, Jerome I. Friedman and Valentine L. Telegdi of the University of Chicago tested parity in the weak-decay chain $\pi^+ \to \mu^+ \to e^+$, also obtaining an asymmetry and thus proving parity violation (Friedman and Telegdi, 1957). Already on 10 December 1957, the theoreticians Lee and Yang received the Nobel Prize in Physics for the important prediction of the breakdown of parity in weak interactions.

In their 1957 Nobel lectures, Lee and Yang mentioned the fact that the parity-violation result implied the breakdown of another symmetry connected with the charge conjugation C (due to the TCP-theorem and assumed T-invariance, as shown in their joint paper with Oehme cited above), and that the neutrino should be described by the two-component wave equation of Hermann Weyl (1929b). This implied that 'the mass of the neutrino and the antineutrino must be zero' (Lee, 1964, p. 416). Moreover, 'it is best if we assume the existence of a conservation law for leptons,' and: 'We assign to each lepton a leptonic number L equal to $+1$ or -1 and to any other particle the leptonic number zero.' (Lee, *loc. cit.*, p. 415) Like the baryonic number B, the leptonic number L had to remain fixed in all elementary-particle processes. With these assignments, the observed weak decays could be satisfactorily described, e.g.,

$$\text{neutron decay:} \quad n \to p + e^- + \bar{\nu}, \tag{897}$$

$$\pi\text{-meson decay:} \quad \pi^+ \to \mu^+ + \nu, \tag{898a}$$

$$\mu\text{-meson decay:} \quad \mu^+ \to e^+ + \nu + \bar{\nu}. \tag{898b}$$

And they accounted also for the reaction ($\bar{\nu} + p \to e^+ + n$), by means of which Frederick Reines and Clyde L. Cowan and collaborators finally established the existence of the neutrino (Cowan *et al.*, 1956). However, the story of the neutrino went on: Several years later, a team around Leon Lederman, Melvin Schwartz, and Jack Steinberger observed two kinds of neutrinos at the Brookhaven proton accelerator (*AGS*) (Danby *et al.*, 1962); hence, one had to accept the existence of two families of leptons, $e^{\pm}$ and $\mu^{\pm}$, each associated with its own neutrino ν_e and ν_μ. Therefore, Eqs. (897–898a, b) had to be rewritten as

$$n \to p + e^- + \nu_e, \tag{899}$$

$$\pi^+ \to \mu^+ + \nu_\mu, \tag{899a}$$

$$\mu^+ \to e^+ + \nu_e + \nu_\mu. \tag{899b}$$

(Here, the lepton numbers $L_e(e^-) = L(\nu_e) = +1$, $L_\mu(\mu^-) = L_\mu(\nu_\mu) = 1$ were assigned.)

In the physics of weak interactions, still another surprise awaited the physicists in 1964, when a Princeton team (also working at the Brookhaven *AGS*) observed 'Evidence for the 2π-Decay of the K_2^0 Meson' (Christenson *et al.*, 1964). According to the then standard theory, the neutral *K*-meson formed two mixed (*CP*)-eigenstates, K_1^0 and K_2^0, with

$$K_2^0 = \frac{1}{\sqrt{2}}(K^0 + \bar{K}^0) \quad \text{and} \quad K_2 = \frac{1}{\sqrt{2}}(K^0 - \bar{K}^0), \tag{900}$$

such that K_1^0 would decay into two π's and K_2^0 into three π's. The discovery of the 2π-decay of the K_2^0 therefore had to be interpreted, as Lincoln Wolfenstein pointed out immediately, as 'evidence that the weak interactions are not invariant with respect to the operation *CP*,' and he proposed a model of 'superweak interactions which described the observed ratios—of the order of 10^{-3} for the *CP*-violating to the *CP*-conserving decays of K_2^0' (Wolfenstein, 1964, p. 562).[1164]

Apart from these refinements, which the knowledge of weak interactions faced in the first half of the 1960's, the parity revolution of 1956/1957 paved the way for a more general description of all weak interactions. 'The near equality of the effective coupling constants in the processes of beta decay, muon decay and muon capture has led to the postulation of a Universal Fermi Interaction,' E. C G. Sudarshan and Robert E. Marshak of the University of Rochester stated in the abstract of a report prepared for the 'International Conference on Mesons and Recently Discovered Particles,' held at Padua and Venice from 22 to 28 September 1957 (Sudarshan and Marshak, 1958a, p. V-14). For his Ph.D. thesis, Sudarshan had analyzed the available empirical data (including the π-, *K*-, and hyperon-decays), taking into account the *C*- and *P*- violations, and used the two-component

[1164] The development from the discovery of the K_L^0 to *CP*-violation has been sketched in Cahn and Goldhaber, 1989, Chapter 7.

neutrino field to conclude 'that the only possible universal four-fermion interaction is an equal admixture of vector plus axial vector interaction' (Sudarshan and Marshak, *loc. cit.*). After some of the previous discrepancies with experiments had been removed, Sudarshan and Marshak submitted a paper on 'Chirality Invariance and the Universal Fermi Interaction' (1958b) to *Physical Review* in January 1958. Nearly four months earlier, the same journal had also received a paper by Richard P. Feynman and Murray Gell-Mann on the 'Theory of the Fermi Interaction' (1958); they arrived at an interaction Hamiltonian that was completely identical to the one of Sudarshan and Marshak's, i.e.,

$$H = \mathscr{G}\{[\bar{A}\gamma_\mu(1+\gamma_5)B]^+[\bar{C}\gamma_\mu(1+\gamma_5)D] + \text{Hermitean conjugate}\}, \tag{901}$$

with A, B, C, and D (and the corresponding conjugate expressions) representing the four Dirac particles involved (e.g., n, p, e^-, and $\bar{\nu}$ in the neutron decay) and $\mathscr{G}$ representing a universal coupling constant (see Sudarshan and Marshak, 1958b, p. 1860, Eq. I). Feynman and Gell-Mann provided a quite different derivation of what later came to be called the 'V–A theory,' which they based on a consideration of weak currents. However, it is also true that Gell-Mann learned from Marshak and Sudarshan about the results of their analysis in July 1957.[1165] Feynman and Gell-Mann went beyond Eq. (901) by investigating the possibility that the coupling constant for the axial-vector current, in contrast to the vector current, might have to be renormalized due to the spreading-out of the meson field (suggesting a multiplication factor of $\sqrt{1.3}$; see Feynman and Gell-Mann, 1958, p. 197). With this slight generalization, the universal (V–A)-theory of the weak interaction became—in the following years—an extremely successful description of the data; it even gave rise to a new theoretical approach in particle physics, the 'current algebras,' which were especially advocated by Gell-Mann.

Besides helping to establish an efficient theory of weak interactions, the discussion of the violation of the discrete symmetries P and C clarified—toward the end of 1950s—the general classification of elementary-particle interactions. Apart from gravitation (which was neglected at that time), essentially three different classes of interactions existed, these being:

1. *Strong Interactions*. This group is responsible for the production and scattering of nucleons, pions [i.e., π-mesons], hyperons [i.e., Λ^0, Σ^-, etc.] and K-mesons [all of these particles were called "hadrons"]. It is characterized by a coupling constant $f^2/\hbar c \cong 1$.
2. *Electromagnetic Interactions*. The electromagnetic coupling constant [for all single-charged particles] is $e^2/\hbar c = (1/137)$.
3. *Weak Interactions*. This group includes all known non-electromagnetic decay interactions of those elementary particles and the recently discovered absorption

[1165] See the acknowledgement of Feynman and Gell-Mann (1958, p. 198) and the historical account of Sudarshan (1989, especially, p. 488). For a full historical account of the V–A theory of weak interactions, see Mehra, 1994, Chapter 21.

> process of neutrinos by nucleons (Cowan, *et al.*, 1956). These interactions are characterized by coupling constants $g^2/\hbar c \approx 10^{-14}$.
> The law of conservation of parity is valid for both strong and electromagnetic interactions but is not valid for the weak interactions. (Lee, 1964, pp. 407–408)

Actually, even more was known about the symmetry properties distinguishing the various classes of interactions. Thus, full isospin symmetry held sway in strong-interaction processes, while the electromagnetic ones only conserved the third component I_3, and in weak interactions, even I_3 could be changed. Similarly, strong and electromagnetic interactions would keep the strangeness quantum number S constant, but in general, not the weak ones. Finally, the three charge-like quantum numbers existed, the (electric) Q, the (baryonic) B, and the (leptonic) L (which, in 1962, was split into L_e and L_μ, as mentioned earlier), which always seemed to be conserved in nature. Symmetry considerations therefore advanced to a principal tool for determining the structure of the fundamental interactions between elementary particles, and toward the end of the 1950s, they were increasingly applied to detect the still unknown laws governing the strong interactions.

In a report on 'Simple Groups and Strong Interaction Symmetries' in the *Reviews of Modern Physics*, the authors analyzed the consequences which could be derived from several symmetry groups, especially SU_3, C_2, B_2, and G_2 (so-to-say, the next higher—'second rank'—symmetry groups after the isospin group) for the particle spectrum of the hadrons (Berends *et al.*, 1962). These symmetries shared, as the main ingredient from physics, the property of having as a characteristic representation an octet of baryons, n, p, Λ, Σ^-, Σ^0, Σ^+, Ξ^-, and Ξ^0. Meanwhile, an Israeli research student of Abdus Salam's at the Imperial College in London and Murray Gell-Mann had gone further: Guided by Jun Sakurai's thorough study on the 'Theory of Strong Interactions' (1960)—who insisted 'that, instead of postulating artificial "higher" symmetries which must be broken anyway within the realm of strong interactions, we take the *existing exact* symmetries of strong interactions more seriously than before and exploit them to the utmost limit' (Sakurai, *loc. cit.*, p. 1)—they proposed what Gell-Mann called the '*Eightfold Way*' (Ne'eman, 1961; Gell-Mann, 1962).[1166] They used the $SU(3)$ symmetry—a generalization of the isospin group $SU(2)$, with a rank-two algebra—so that the only additive quantum numbers for hadron states were I_3 and Y, as had actually been observed. 'It also picked out the eight-dimensional adjoint representation—not only for the vector particles and currents, but also for baryons and pseudoscalar mesons as well,' as Ne'eman described their scheme (in Gell-Mann and Ne'eman, 1964, p. 5), and Gell-Mann summarized the essential features as:

[1166] This was the title of a preprint by Murray Gell-Mann, dated 15 March 1961. Independently, Yuval Ne'eman discussed the same symmetry-approach in his paper on the 'Derivation of Strong Interactions From a Gauge Invariance'—by the way, Sakurai, in his programmatic study, mentioned the use of gauge invariance as a guide also to investigate strong interactions. His paper was received on 13 February 1961, by the European journal *Nuclear Physics*.

1. It is suggested that the strong interactions, besides conserving the three components of isotopic spin (I_1, I_2, I_3) [which he renamed $F_1 = I_1$, $F_2 = I_2$ and $F_3 = I_3$] and the hypercharge Y $[= \frac{2}{\sqrt{3}} F_8]$ also appropriately conserve four more operations $F_4, F_5, F_6, F_7 \ldots$ [Thus] we have an algebra of eight operators $F_1, \ldots F_8$, and it is proposed that these have the right commutation rules to form the eight generators of the algebra of $SU(3)$....
2. All known strongly interacting particles are to be assigned to "tensor representations" [i.e., to direct products of the fundamental three-dimensional representation of $SU(3)$!], the smallest of which are: (a) the trivial one-dimensional representation, denoted by **1** and consisting of a neutral singlet with $I = 0$, $Y = 0$, and with $I = \frac{1}{2}$, $Y = 1$, a doublet, with $I = \frac{1}{2}$, $Y = -1$, a singlet; (b) the 'adjoint' eight dimensional representation, denoted by **8** consisting of a doublet with $I = 0$, $Y = 0$, and a triplet with $I = 1$, $Y = 0$ [this gave rise to the famous hexagonal figure with a double core in the $I_3 - Y$ plane]....
3. It is proposed that the part of strong interactions that violates the higher symmetry transforms like ... the eighth component of an octet, in order that the isospin and strangeness may be conserved.... For an octet, we obtain a sum rule for the masses of the four multiplets involved.

(Gell-Mann, in Gell-Mann and Ne'eman, 1964, pp. 8–9)

From the assumptions of the Eightfold Way, Susumo Okubo derived a general mass-formula,

$$m = a + bY + c\left[\frac{1}{4}Y^2 - I(I+1)\right], \tag{902}$$

for any $S(U)3$ multiplet, with a, b, and c denoting the characteristic parameters for a given supermultiplet (Okubo, 1962, p. 959, Eq. (27)). For the baryon-octet, he obtained the relation

$$\frac{1}{2}[m(n) + m(\Xi)] = \frac{3}{4}m(\Lambda) + \frac{1}{2}m(\Sigma) \tag{903}$$

(Okubo, *loc. cit.*, p. 960, Eq. (30)), which had also been given by Gell-Mann (1962, p. 1080, Eq. (8.1)), and which fitted the data to within a few MeV. Further quantitative consequences, e.g., for the β-decay of octet members (Cabibbo, 1963), would also be derived in good agreement with experiment. Evidently, the Gell-Mann–Ne'eman scheme represented the essential features of hadrons and their behaviour. As a particular triumph, there appeared the observation of a hyperon with strangeness $Y = -3$, the Ω^-, which followed as the last missing member of the $SU(3)$-decuplet of baryon resonances, consisting of the $(3,3)$-nucleon isobar $N^*_{3/2}$ and the known hyperon resonances Y^*_1 and $\Xi^*_{1/2}$; in particular, it had the right mass, $m = (1680 \pm 12)$ MeV/c^2, as compared to the predicted value 1680 MeV/c^2 from the $SU(3)$-mass formula (Barnes *et al.*, 1964).

1.4 The Problems of Strong-Interaction Theory: Fields, *S*-Matrix, Currents, and the Quark Model (1952–1969)

In 1949, when the problems of quantum electrodynamics had been overcome by the renormalized *QED* of Feynman, Schwinger, and Tomonaga, Hideki Yukawa claimed that the description of strong interactions required the use of nonlocal fields (in contrast to *QED*), which would also take into account the finite size of the particles (i.e., the hadrons) involved (Yukawa, 1950a, b). During the following years, quite a few theoreticians—including Werner Heisenberg, Walter Heitler, Christian Møller, and Rudolf Peierls—devoted considerable interest and effort to nonlocal field theories. On the other hand, the advocates of maintaining quantum field theories strictly local did not give up so easily, especially since the proposed nonlocal descriptions exhibited obvious difficulties. Based on the Hamiltonian formulation and standard quantization (i.e., the canonical commutation relations for Bose and Fermi fields), the mathematical theory of distributions—formulated by Laurent Schwartz (1950) and others—and finally the renormalization concept (so successful in *QED*), they worked on the so-called 'axiomatic quantum field theory.'[1167] One of the papers that founded this approach was contributed by Ernst C. G. Stueckelberg and his student A. Petermann, who put the renormalization procedure on a systematic basis by developing a 'renormalization group' (Stueckelberg and Petermann, 1953). In Göttingen, Harry Lehmann (1954) classified the singularities of the propagator in local quantum field theories and proceeded, together with Kurt Symanzik and Wolfhart Zimmermann, to write the conditions for strictly local theories describing elementary particles. They derived three important results: first, the asymptotic condition to express the asymptotic 'out' and 'in' operators directly in terms of fields; secondly, the reduction formula for *S*-matrix elements in terms of Green's functions; thirdly, systems of nonlinear equations for the Green's functions, which could be solved in formal power series involving a coupling parameter but not implying any of the ambiguities and infinities of the previous quantum field theories (Lehmann, Symanzik, and Zimmermann, 1955, 1957).[1168]

The programme of local quantum field theory was promoted in the following decade by the work of Arthur Wightman in the United States, Hans Borchers, Rudolf Haag, Daniel Kastler, and David Ruelle in Europe, and Huzihiko Araki in Japan. In particular, it helped to prove rigorously the so-called dispersion relations, derived for the scattering amplitudes of pions and nucleons by Geoffrey Chew, Murray Gell-Mann, Marvin Goldberger, Francis Low, and Yoichiro Nambu, from 1953 onward.[1169] A typical dispersion relation read, for

[1167] For a condensed history of this approach, see Wightman, 1989.

[1168] The investigations of Lüders (1954) on the *TCP*-theorem, mentioned in the previous subsection, might also be counted in the context of general, strictly local quantum field theories.

[1169] For a historical overview of the dispersion-relation approach to strong interactions, see Pickering, 1989.

example,

$$D_{\pm}(k)-\frac{1}{2}\left(1+\frac{\omega}{\mu}\right)D_{\pm}(0)-\frac{1}{2}\left(1-\frac{\omega}{\mu}\right)D_{\mp}(0)$$
$$=\pm\frac{2f^2}{\mu^2}\frac{k^2}{\omega-\mu/2M}+\frac{k^2}{4\pi^2}\int_{\mu}^{\infty}\frac{d\omega'}{\omega'-\omega}\cdot\left[\frac{\sigma_{\pm}(\omega')}{\omega'-\omega}+\frac{\sigma_{\mp}(\omega')}{\omega'+\omega}\right], \qquad (904)$$

where D_+ and D_- denoted the two independent amplitudes, ω denoted the energy of the incident pion (with k its momentum; hence, $k^2=\omega^2-\mu^2$), and μ and M denoted the pion and nucleon masses (see Goldberger, Miyazawa, and Oehme, 1955, p. 987).[1170] The first term on the right-hand side of Eq. (904) then represented the contribution of the single-nucleon pole; it contained, in particular, the (pseudovector) pion–nucleon coupling constant f, which could then be determined from the scattering data and assumed the value 0.08—the corresponding pseudoscalar pion–nucleon coupling constant (which enters into the principal renormalizable interaction) would be $g=\left(\frac{2M}{\mu}\right)f$, hence, become of the order of 10. Since the validity of the dispersion relations followed from the causal behaviour of the scattering amplitudes, the adherents of the strictly local quantum field theories—in which also local causality ruled—succeeded in deriving relations like Eq. (904) directly from their schemes (see, e.g., Symanzik, 1957; Bogoliubov, Medvedev, and Polivanov, 1958; Bremermann, Oehme, and Taylor, 1958; Lehmann, 1958).

In contrast to the above-mentioned younger members of the Theory Division at the *Max Planck-Institut für Physik* in Göttingen, Werner Heisenberg felt that they had restricted the mathematical scheme of their local quantum field theory too much; hence, it would not allow one to describe the observed elementary particles and their interactions. After 1951, he preferred to investigate a different kind of quantum field theory by pursuing a much more ambitious goal, namely, to describe *all* elementary particles and their reactions with the help of a *single* nonlinear equation involving a single spinor field ψ—the real elementary particles then had to be composed of suitable products of ψ (though they were not composites of only a few ψ's).[1171] In particular, Heisenberg renounced two requirements of the axiomatic local quantum field theory: first, that the Hilbert space can be spanned already by asymptotic operators; and secondly, that it possesses a positive metric; instead, he made use of Paul Dirac's negative-norm states (1942),

[1170] The dispersion relations of the type of Eq. (904) reminded one, of course, of the Kramers–Kronig dispersion relations derived in the mid-1920s for the scattering of X-rays by atoms (Kramers, 1927; Kronig, 1926c). Indeed, it had been Kronig who submitted the first dispersion relation for the scattering amplitudes of elementary particles in Heisenberg's *S*-matrix scheme (Kronig, 1946) and thus pioneered the new approach.

[1171] Heisenberg's collaborator since 1958, Hans-Peter Dürr, has outlined the motivations and steps leading to the 'nonlinear spinor theory' in an annotation to Heisenberg's *Collected Works* (Dürr, 1993).

which now helped to improve the short-distance behaviour of the propagator function of the fundamental ψ (Heisenberg, 1957, pp. 272–273). The universal nonlinear spinor-field equation of Werner Heisenberg and Wolfgang Pauli—the latter had eagerly participated in this enterprize from 1957 to January 1958—seemed to fulfill their dream of the 1930s, namely, to describe all the elementary particles and to determine their properties and behaviour (including, in particular, the calculation of the numerical values of the coupling constants involved) from one general theory, or field equation, without running into any problem with the infinities. However, the unresolved difficulties soon destroyed Pauli's initially great optimism, and he severely criticized both the mathematical and computational methods (e.g., the use of the indefinite metric and the peculiar 'Tamm–Dancoff' approximation employed in the calculation).[1172] Later on, the physical evidence also contradicted certain predictions of the theory, such as the coupling constant of the η-meson. Still, however, several features of the attempt have remained fruitful in the further development of elementary-particle theory, such as having more regular field operators (than those occurring in the canonical free-field theories), or the idea of a degenerate vacuum state connected with a spontaneous breakdown of certain original symmetries.

According to Heisenberg, the theories of elementary particles might be established in three different ways: first, as a pure *S*-matrix scheme, in which a unitary characteristic matrix satisfying the condition of causality (because of its analytic structure) accounts for all existing elementary particles and their interactions; secondly, as a quantum field theory of the type considered by Heisenberg himself in the late 1950s; and thirdly, as the strictly local quantum field theory of the axiomatic type. Although the *S*-matrix theory of the 1940s should, at least in principle, allow one to describe the behaviour of all strongly interacting elementary particles, the physicists—in the 1950s—exploited only the relation between causality and analyticity of the *S*-matrix (via the dispersion relations). 'How could the *S*-matrix, which deals with the asymptotic states, where particles are outside the region of interaction, incorporate the equivalent of a Yukawa force?' then asked Geoffrey F. Chew—previously himself a participant in a collaboration on the dispersion approach to meson-nucleon scattering—and answered:

> That 1956 collaboration by Chew, Goldberger, Low and Nambu (CGLN), which led to two papers, in my recollection yielded the first clear statement that force—in the sense of Yukawa—resides in the singularities of an analytic *S*-matrix (Chew *et al.*, 1957a, b). From that point on I never believed that the description of interhadronic forces demanded a Lagrangian. Stanley Mandelstam's paper of 1958 gave powerful reinforcement of this belief (Mandelstam, 1958). Although I failed to recognize until

[1172] Although Pauli drafted the first preprint, entitled 'On the Isospin Group in the Theory of the Elementary Particles,' he withdrew from further collaboration in January 1958, after he encountered severe criticism and opposition to the theory from the U.S. physicists at the American Physical Society meeting in New York; thus, Heisenberg was left to work out the details of the theory with younger collaborators (Dürr *et al.*, 1959).

1960 that the CGLN papers and that of Mandelstam were dealing with the concept identified by Heisenberg in the early [1940s], my thinking for two decades, starting in 1956, was based on the analytic S matrix. (Chew, 1989, p. 604)

The observation that in the process $\pi + \pi \rightarrow N + \overline{N}$ (with N denoting the nucleon), which is related to the pion–nucleon scattering—it actually corresponds to the crossed reaction channel in which the variables s (energy squared) and t (momentum transfer squared) are interchanged—suggested indeed that for a scattering amplitude dispersion relations can be written both in the s- and t-variables, i.e., as the so-called 'double-dispersion relations' (Mandelstam, 1958). Based on Mandelstam's programme—Lev Landau in Moscow investigated a similar approach (1959)—Chew expanded on a 'Unified Dynamical Approach to High- and Low-Energy Strong Interactions' at the Berkeley Conference of December 1960. 'Steve Frautschi and I got started in this direction because of frustration with attempts to use the Mandelstam representation [involved in the double-dispersion relations (1958)] to make a self-consistent low-energy dynamical theory that includes P resonances [of the $\pi\pi$-system],' Chew told about the first step and continued:

Frautschi and I are proposing [in 1960] the simplest extension of the original Mandelstam program that we feel can conceivably accommodate low-energy P resonances. We have arrived at a set of equations that may or may not be self-consistent and complete. (Chew, 1961, p. 467)

In carrying out their ambitious programme, namely, to obtain self-consistent solutions of the strong-interaction problems, which would allow one to determine both the forces between the hadrons and their masses, spin, and other properties, Chew and his collaborators in Berkeley received support from a different source. In Munich, the Italian visitor Tullio Regge investigated the asymptotic behaviour of (nonrelativistic) potential scattering; he found that for large invariant momentum transfer t, the scattering amplitude behaves like

$$\text{scattering amplitude} \sim s^{\alpha(t)}, \tag{905}$$

with $\mathrm{Re}\,\alpha$ being positive for an attractive potential and increasing when the strength of the potential increases (Regge, 1959). He further showed that potential waves for angular momentum $l \leqslant (\mathrm{Re}\,\alpha)_{\max}$ may have bound states or resonances, while those for $l > [\mathrm{Re}\,\alpha]_{\max}$ necessarily have small phase shifts (Regge, 1960). Chew believed 'Regge's arguments to be of general validity' (Chew, 1961, p. 469), and stated, in a discussion at the 1961 (twelfth) Solvay Conference in Brussels, the following conclusions:

Regge has shown for elastic scattering and Froissart [1961b] for any amplitude satisfying the Mandelstam representation [1958] that the S-matrix can be simultaneously continued into the complex energy (E) and angular momentum [J] planes; for scattering by a superposition of Yukawa potentials, all poles are associated with bound

> states and resonances and may be viewed either in the E-plane for fixed J or in the J-plane for fixed E. A corollary to the latter viewpoint is that the position, α_i, of a particular pole in the J-plane is an analytic function of E, and $\alpha_i(E) =$ constant turns out not to be allowed. If at the same energy the value Re $\alpha_i(E)$ passes through a positive integer or zero (with d Re $\alpha_i/dE > 0$) one has here a physical resonance or bound state for J equal to this interger, so in general the trajectory of a single pole in the J-plane as E changes corresponds to a family of "particles"—some stable and some unstable—of different J and different mass.... We may satisfy Feynman's principle [i.e., the correct theory should not allow a decision as to which particles are elementary or not] by postulating that *all* poles of the S-matrix are of this type ([i.e.] Regge poles). (Chew, in Mandelstam, 1962, p. 230)

With the above-mentioned set of ideas, the so-called 'Regge theory' of strong interaction was launched, which dominated the description of hadronic collision data—the main result from the then-possible high-energy experiments, at least with proton accelerators—through the 1960's. The partial wave amplitudes observed appeared to be determined especially by the 'Regge trajectories' $\alpha(t)$—with the hadrons lying on linear trajectories in the (Re $\alpha - t$)-plane, and all members of each hadron family occupying as 'Regge poles' the position at integral values of Re $\alpha = J$, with a spacing $\Delta J = 2$. Thus, the strong forces found their proper place in the S-matrix formalism as being created by the poles in the crossed t-channel, while in the production and annihilation processes the poles (i.e., hadrons) appear in the s-channel. The unitarity condition for the S-matrix added a bound, the 'Froissart bound,' to the position of the Regge trajectories (Froissart, 1961b). Still, the role of other discontinuities that could occur in the complex J-plane, such as 'Regge cuts,' had to be debated, and in the late 1960's, the Regge theory reached a state of very high technical and conceptual sophistication: In addition to the Regge trajectories, 'daughter' and 'parent' trajectories and a 'conspiracy' of trajectories were taken into account (besides, of course, the cuts).[1173] On the other hand, 'duality considerations' somewhat simplified the situation.[1174]

Although the S-matrix theory provided the majority of publications dealing with hadrons and strong interactions in the 1960's, it did not exhaust the literature in the field. The enormous amount of data produced by numerous particle-accelerator laboratories stimulated the theorists to apply a variety of other approaches as well. For example, 'current algebras' which Gell-Mann (1962) introduced as a dynamical basis for the $SU(3)$-symmetry—he quite distrusted the quantum field theory at that time—connected with the hypothesis of the partially conserved axial current (Gell-Mann and Lévy, 1960), resulted in remarkable conclusions such as the Adler–Weisberger relation. The latter expressed G_A, the renormalization factor of the weak axial current entering Eq. (901) and stemming from strong interaction, in

[1173] For a review, see Collins and Squires, 1967.

[1174] In particular, it was found that the Regge (in the t-channel) and the resonance (in the s-channel) descriptions of pion–nucleon amplitudes did not have to be added, since one is basically complementary to the other; i.e., the Regge amplitude provided an *average description* of the resonances in the observed amplitudes (Dolen, Horn, and Schmid, 1967).

terms of the pion-decay constant f_π and strong-interaction quantities such as

$$G_A^2 = 1 - 2\frac{f_\pi^2}{\pi}\int_0^\infty \frac{d\nu}{\nu}[\sigma^{\pi^- p}(\nu) - \sigma^{\pi^+ p}(\nu)] \tag{906}$$

(Adler, 1965; Weisberger, 1965). The abnormally small mass of the lightest hadron π (or pion) suggested the almost perfect validity of 'chiral symmetry' in strong interactions, from which several consequences, notably, the relations between the production amplitudes of pions in hadronic collisions (soft-pion approach), were derived in good agreement with the observations. In connection with chiral symmetries and the partially conserved axial current, effective Lagrangians were introduced (see the review by Stephen Gasiorowicz and D. A. Geffen, 1969). The effective Lagrangians in general turned out to be non-renormalizable; they often exhibited a non-polynomial structure, for which refined mathematical rules (like Borel sums) had to be used (e.g., Salam, 1970).

Before the mid-1960s, a model in the literature appeared, which, despite its modest entrance, increasingly caught the attention of the particle physics community; thus, Peter Freund, at the Boulder Conference on High Energy Physics in August 1969, stated:

> The quark model has been a major source of meaning for numerous questions that have been raised in hadron physics over the last five years. Satisfactory answers to old questions are also provided by the quark model. This is really quite remarkable, as we know very little about the quarks themselves. (Freund, 1970, p. 565)

Indeed, in January 1964, Murray Gell-Mann introduced new objects into particle physics and baptized them 'quarks': They arose as the fundamental three-dimensional representation of the $SU(3)$ group to which, in contrast to 1962, Gell-Mann now assigned some significance for building up the known hadrons: For example, the baryons would consist of three such quarks, each having the spin-$\frac{1}{2}$, while the mesons would be made of quark–antiquark pairs (Gell-Mann, 1964).[1175] A particular and unusual feature characterized these quarks: The three fundamental objects, distinguished as u ('up'), d ('down'), and s ('strange') quarks, had to possess fractional charges, namely,

$$Q(u) = \frac{2}{3}, \quad Q(d) = Q(s) = -\frac{1}{3}. \tag{907}$$

Incidentally, Gell-Mann's idea was duplicated (evidently independently) by the former Feynman graduate student at Caltech and later a visitor to CERN, George

[1175] Actually, the idea of composing hadrons from a set of fundamental entities had been discussed by Shoichi Sakata since the 1940s; in 1956, he proposed that proton, neutron, and Λ represented a fundamental triplet, from which all other baryons could be constructed, while the mesons consisted of a particle-antiparticle pair (Sakata, 1956).

Zweig, in an unpublished preprint; he called his fractionally charged fundamental objects 'aces' (1964).[1176] The quark (or ace) picture now immediately explained the existence of the observed $SU(3)$-hadron multiplets; for example, from a quark-triplet only singlets, octets, and decuplets would emerge for the baryons, and from a quark–antiquark pair only, singlets and octets for mesons.

The quark model, because of its simplicity, soon attracted the interest of both theoretical and experimental physicists. 'A great variety of observations concerning strong, electromagnetic and weak interactions, can be understood if we suppose that quarks are the basic constituents of hadronic matter,' wrote Kokkedee, the author of the book *The Quark Model*, in 1969, and he also stated perhaps the decisive reason for this success:

> Moreover, the simplest dynamical assumption one can make, namely, that of *additivity*, in which some hadronic property is described as the sum of the corresponding quark properties, has been amazingly successful in providing simple relations between different facts. In this way the model not only reproduces the results of $SU(3)$ and $SU(6)$ symmetry, but leads to many experimentally correct predictions that do not follow directly from either symmetry. This fact is most strikingly demonstrated by its application to high-energy scattering. (Kokkedee, 1969, p. ix)

Indeed, toward the end of the 1960s, the quark model not only served to describe 'low-energy properties' of elementary particles—e.g., masses magnetic moments, and decay processes—but, in addition, the high-energy elastic and inelastic hadron–hadron processes, including annihilation and multiparticle production. If one assumed the quarks to be heavy objects, they would move practically always with nonrelativistic velocities, and the assumption of additivity might be justified. However, as Freund remarked at the Boulder Conference referred to above, 'we know very little about quarks,' and especially:

> We don't know whether there should be three or nine quarks, whether the electrical charge should be integral or fractional, whether they carry magnetic charge, whether they are fermions or parafermions, whether they are light or heavy, and ultimately whether quarks exist. (Freund, 1970, p. 565)

Here, Peter Freund addressed some of the most serious problems of the 'naive quark model' (as one would call it later). The problem of quark statistics arose from an analysis of Fermi's $(3,3)$ nuclear resonance, which seemed to be an S-wave state of three quarks and should be symmetric in spin and isospin, but then the constituent quarks would not obey the Fermi statistics. Hence, O. W. Greenberg (1964) argued that quarks might follow a generalization of the usual statistics, the parastatistics introduced by Herbert S. Green (1953) earlier; indeed,

[1176] Zweig (who did not share Gell-Mann's knowledge of literature—"quarks" came from James Joyce's *Finnegan's Wake*)—invented his 'aces' in order to understand the reason why certain strong decays seem to be forbidden, especially the decay of the ϕ-meson (having spin 1) into $\rho\pi$. With the help of the 'ace'-composition, he could then formulate the 'Zweig rule' for such suppressed decays.

they had to behave like parafermions of order 3 in Green's language. A year later, M. Y. Han and Yoichiro Nambu (1965) offered a different proposal to solve both problems of fractional charge and statistics: They assumed that each quark existed, however, in three different species having integral charges.[1177] The question of the real quark mass depended theoretically on the unknown quark dynamics, and especially on the discovery of free quarks. Already Gell-Mann had commented about this point in his pioneering paper, when he wrote:

> It would be fun to speculate about the way quarks would behave if they were physical particles of finite mass (instead of purely mathematical entities as they would be in the limit of infinite mass). Since charge and baryon number are exactly conserved, one of the quarks ($u^{2/3}$ or $d^{-1/3}$) would be absolutely stable.... Ordinary matter near each other's surface would be contaminated by stable quarks as a result of high-energy cosmic-ray events throughout the earth's history, but the contamination is estimated to be small. A search for stable quarks $-1/3$ or $+2/3$ and/or stable di-quarks of charge $-2/3$ or $+1/3$ or $+4/3$ at the highest energy accelerators would help to reassure us of the non-existence of real quarks. (Gell-Mann, 1964, p. 215)

Gell-Mann's negative expectation was indeed substantiated by the experiments until 1969—a positive claim in that year and a later one in 1977 would not survive experimental checks. Hence, Freund's conclusion, 'It may well happen that quarks are but a useful technical device to treat hadrons' (Freund, 1970, p. 565)—in agreement with Gell-Mann's opinion—remained the last word at the end of the 1960s. However, the question of the quark's existence and the other ideas mentioned in this connection would soon be answered in a different sense by the new 'Standard Model' of elementary particles.

1.5 The 'Standard Model' and Beyond (1964–1999)

In the introductory essay to the proceedings of the 'Third International Symposium on the History of Particle Physics,' which was devoted to particle physics in the 1960s and 1970s, especially to 'The Rise of the Standard Model,' the editors concluded:

> To characterize the 1964–1979 period as a "scientific revolution," however, strikes us as inexact and misleading. It was indeed a time of real upheaval in particle physics, but the field lacked a crucial ingredient at the outset of this period—a single dominant theory of the subatomic world agreed upon by the entire community of practitioners. Rather particle physics in the early 1960s could be broken into a number of fiefdoms, none of which could claim the unswerving allegiance of every single knight. There was a surfeit of nobles ready and eager to fight amongst themselves, but no powerful king to overthrow.

[1177] The fractional charges of Gell-Mann and Zweig's constituents then arose from the averaging of the three species: e.g., if two u-quarks had charge 1 and one charge 0, the new averaging yielded $Q = \frac{2}{3}$, etc.

> The rise of the Standard Model changed this chaotic situation dramatically. A cacophony of competing ideas was replaced by a single theory upon which essentially all practicing physicists agree—a single reference point against which all work is now compared. In a telling phrase, ... particle physicists sometimes refer to this period not as a revolution but as a "phase change." (Hoddeson *et al.*, 1997, p. 30)

This phase change or phase transition came about not via the input of completely new ideas but rested rather on assembling and developing further already known theoretical concepts—such as renormalized quantum field theory, especially in the gauge formulation, the quark model, and spontaneous symmetry-breaking—in the light of new experimental discoveries—e.g., the narrow resonances, the τ- or Υ, particles, or even the quarks in the deep-inelastic scattering. We shall attempt to list below in some systematic way the main theoretical or empirical ingredients of the Standard Model. Since the latter actually consists of two well-separated models, the so-called 'electroweak theory,' on the one hand, and the 'quantum chromodynamics (QCD),' on the other, the following section will be split into these two domains covering: (a) the unified electromagnetic and weak interactions and (b) strong interactions. Of course, links also showed up between these two domains; indeed, the main features of both theories, like the gauge structure and renormalizability as well as the number of fundamental families of constituents—there being three lepton families corresponding to three quark families—document a nearly complete harmony. Hence, one would like to suspect that in the final effort both parts may be joined into one uniform scheme or theory. However, nature has so often presented surprises, and one should not expect to arrive at a smooth unification of parts (a) and (b), not to think about the possible incorporation of the still missing fundamental interaction of gravitation into the unified scheme. The new ideas so far expressed go beyond the standard model, some of which are touched upon at the end of this subsection, and are conceptually and methodically quite diverse, often incomplete and mostly speculative, and sometimes they open entirely new territories [see the last Subsection (c)].

(a) The 'Electroweak Theory' (1964–1983)

The so-called electroweak theory contains as the crucial input a new particle and a special symmetry-breaking mechanism; it was formulated finally as a particular model of a renormalizable gauge field theory.

(a1) The 'Intermediate Weak Boson': 'One of the recurrent dreams in elementary particle physics is that of a possible fundamental synthesis between electromagnetism and weak interaction,' wrote Abdus Salam and John C. Ward in a letter, received on 24 September 1964 by *Physics Letters* (Salam and Ward, 1964). They argued that both forces equally affect all forms of matter, share the character of vector form and possess universal strengths (Salam and Ward, *loc. cit.*, p. 168). Still, profound differences in strength, space-time behaviour, and the ΔS- and ΔI-behaviour existed. However, Salam and Ward showed how to remove the differ-

ence by assuming a particular Lagrangian involving three new particles, the positively and negatively charged and the neutral 'intermediate (weak) boson,' which should have masses of about 137 m_p, in order that the new dimensionless weak coupling constant $g_W^2/4\pi$ equals $e^2/4\pi$, the fine structure constant. A similar theory, though not in explicit detail, had been proposed by Sheldon Lee Glashow (1960) earlier; both publications referred to earlier made attempts in this direction.

(a2) Spontaneous Symmetry-Breaking and the Higgs Mechanism: The idea of spontaneous symmetry-breaking went back to the 19th-century ideas on ferromagnetism (Pierre Curie) and was first adapted by Heisenberg and Pauli to elementary particle physics for explaining certain features of the weak and electromagnetic forces in the nonlinear spinor theory: 'When it appears impossible to construct a fully symmetric state called "vacuum," it must be interpreted visually only in the way that the unsymmetric ground state does not really represent the vacuum but rather the state "world," forming the substrate for the existence of elementary particles,' argued Heisenberg (Dürr *et al.*, 1959, p. 446), and derived several consequences from the degenerate 'world' state like the occasional breakdown of isospin symmetry. Yoichiro Nambu and Giovanni Jona-Lasinio (1961) then considered a simple dynamical model for the nucleon–meson interaction (in analogy to the situation in superconductivity) which also exhibited some vacuum degeneracy, and finally, Jeffrey Goldstone treated a renormalizable theory with a quartic self-interaction exhibiting degenerate vacuum states connected with a broken continuous symmetry; he found the solutions not only to exhibit a lower symmetry than the Lagrangian, but also to 'contain mass zero bosons' (Goldstone, 1961, p. 154). Together with Abdus Salam and Steven Weinberg, Goldstone provided 'some proof of Goldstone's conjecture [afterwards called "Goldstone's theorem"], that if there is continuous symmetry transformation under which the Lagrangian is invariant, then either the vacuum state is also invariant under the transformation, or there must exist spinless particles of zero mass' (Goldstone *et al.*, 1962, p. 965). Two years later, Peter Higgs of Edinburgh demonstrated that 'the theorem fails if and only if the conserved currents associated with the internal group are coupled to gauge fields,' and:

> As a consequence of this coupling, the spin-one quanta of some of the gauge fields acquires mass; the longitudinal degrees of freedom of these particles (which would be absent if their mass were zero) go over into the Goldstone bosons when the coupling tends to zero. (Higgs, 1964b, p. 508)[1178]

(a3) The Weinberg–Salam Model and Its Renormalization: The possibility of escaping from the zero-mass particles, arising due to the Goldstone theorem, encouraged Weinberg to unite the electromagnetic and weak interactions in 'a model, in which the symmetry between the electromagnetic and weak interactions is spontaneously broken, but in which the Goldstone bosons are avoided by intro-

[1178] For more details, see also Higgs, 1964a, 1966.

ducing the photon and the intermediate boson fields as gauge fields' (Weinberg, 1967, p. 1264). The fields of the originally renormalizable Lagrangian of Salam and Weinberg could indeed be rearranged into a charged pair of massive spin-1 fields $W_\mu^\pm$, a neutral massive spin-1 field Z_μ, and the massless photon field A_μ; but the question whether the rearranged field theory (i.e., rearranged by the 'Goldstone mechanism') was renormalizable remained open. Half a year later, Salam attacked this question in his talk at the Nobel Symposium in Sweden (1968). He also started from the Higgs mechanism, particularly from the fact that the symmetry-breaking term $\langle \rho_0 \rangle$ occurring in the gauge transformation of the term $(\partial_\mu - ieA_\mu)\phi^* \cdot (\partial_\mu + ieA_\mu)\phi$ in the Lagrangian gave rise to a term $e^2\langle \rho_0 \rangle^2 A_\mu^2$, i.e., a massive vector boson, and wrote the new Lagrangian for the weak interaction as (with the suffixes L and R for left- and right-handed)

$$\mathscr{L}_{\text{weak}} = [(e^- \nu)_L + (\bar{\nu}\mu^+)_R] W^- + \text{Hermitean conjugate}, \tag{908}$$

where the e^-, etc., denoted the corresponding fields. The Lagrangian of the neutral boson field W_μ^0 (or Z_μ of Weinberg) had to be added to the expression (908). Again, three years later, in two papers on the 'Renormalization of Massless Yang-Mills Fields' and 'Renormalizable Lagrangians for Massive Yang-Mills Fields,' Gerard 't Hooft of Utrecht—based on preliminary work by his thesis supervisor Martinus Veltman (1968, 1970)—completed the electroweak theory ('t Hooft, 1971a, b).[1179] While Benjamin W. Lee removed certain spurious singularities appearing in the proof of renormalizability (Lee, 1972), Weinberg formulated the Feynman rules in the new theory and began to calculate the physical processes explicitly, especially the expressions for the production amplitudes of the weak bosons $W^\pm$ and Z^0 (Weinberg, 1971).

(a4) Neutral Currents and the Discovery of the Weak Bosons: In an investigation on the 'Effects of a Neutral Intermediate Boson in Semileptonic Processes,' Weinberg developed the description of these processes in the electroweak theory and calculated for the ratio of the cross sections of neutral-current processes involving leptons over the cross sections of the charge-exchange processes (Weinberg, 1972). For not too large momentum transfers, he obtained the result

$$0.15 \leq \frac{\sigma(\nu' + p \to \nu' + p)}{\sigma(\nu' + p \to \mu^- + p)} \leq 0.25 \tag{909}$$

as compared to the experimental upper limit 0.12 ± 0.06 [see Weinberg, *loc. cit.*, p. 1412, Eq. (1.1)].[1180] However, the 'Observation of Neutrino-Like Interaction

[1179] Just as we write these lines, the award of the Nobel Prize for Physics for 1999 went to Gerard 't Hooft and his teacher, Martinus Veltman, for their pioneering work. The pioneering papers of 't Hooft and Veltman are included in G. 't Hooft's *Under the Spell of the Gauge Principle* (Singapore: World Scientific, 1994).

[1180] The absence of strangeness-changing weak currents of the neutral type could be explained by a suggestion of Glashow and collaborators (1970) proposing the existence of a fourth quark; see below.

Without Muon or Electron in the Gargamelle Neutrino Experiment’ at CERN provided ‘events induced by neutral particles and producing hadrons, but no muon or electron’ which ‘behave as expected if they arise from neutral current-induced processes’ (Hasert *et al.*, 1973, p. 138). Although the result was at first doubted, especially by the experimental particle physicists in the United States, who saw no such events, it was eventually confirmed, and in 1979, both Salam and Weinberg—together with Sheldon Glashow—received the Nobel Prize in Physics.

Still, the greatest triumph of the electroweak theory had to wait for another three years, until the UA1-Collaboration at CERN announced events ‘which have the signature of a two-body decay of a particle of mass ~80 GeV/c^2,’ and:

> The topology as well as the number of events fits well with the hypothesis that they are produced by the $p + \bar{p} \rightarrow W^{\pm} + X$, with $W^{\pm} \rightarrow e^{\pm} + \nu$, where $W^{\pm}$ is the Intermediate Boson postulated by the unified theory of weak and electromagnetic interactions. (Arnison *et al.*, 1983, p. 103)

Actually, the experimentalists at CERN knew pretty well, in which energy region of their colliding proton–antiproton beams they had to search, at least if the minimal electroweak coupling of the Standard Model was to be realized. Indeed, two coupling constants g and g' existed (to the $W^{\pm}$ and the Z^0, respectively), whose ratio was given by the ‘Weinberg angle’ θ_W as

$$\tan \theta_W = g'/g. \tag{910}$$

Then, the charged weak currents would couple to the electromagnetic field with the coupling constant $e = g \sin \theta_W = g' \cos \theta_W$, while the weak neutral current coupled to Z^0 with the strength $g/\cos \theta_W$. Finally, the Higgs mechanism provided the vector mesons (with unequal) masses $m_W = m_Z \cos \theta_W$, and in tree-approximation, the relation (with G_F denoting the Fermi constant),

$$m_W \sin \theta_W = m_Z \sin \theta_W \cos \theta_W = \frac{e}{Z}(\sqrt{2} G_F)^{-1/2} = 37.3 \text{ GeV/c}^2, \tag{911}$$

determined the individual masses of the $W^{\pm}$- and Z^0-bosons, provided the Weinberg angle θ was given. Now, the neutral current data fixed the angle as

$$\sin^2 \theta_W = 0.230 \pm 0.09; \tag{912}$$

hence,

$$m_W \approx 77\text{–}84 \text{ GeV/c}^2, \tag{913a}$$

$$m_Z \approx 89\text{–}95 \text{ GeV/c}^2, \tag{913b}$$

(See Salam's Nobel lecture, 1992, p. 524). Consequently, the results of UA1 and UA2 at CERN for the W-mass,

$$m_W = (81^{+5}_{-5})\ \mathrm{GeV/c^2} \quad \text{and} \quad (80^{+10}_{-6})\ \mathrm{GeV/c^2}. \tag{913'}$$

were indeed 'in excellent agreement' with the electroweak theory of Salam and Weinberg (Arnison *et al.*, 1983, p. 115, and Banner *et al.*, 1983, p. 485).[1181] The observation of Z^0 and the experimental determination of mass and width would have to wait for several years until the Stanford Linear Collider and the CERN Large Electron–Positron Collider were completed, and again the result would agree with the prediction of Eq. (913b).

(b) Quantum Chromodynamics (*QCD*) (1965–1995)

The renormalizable gauge theory of strong interactions, which represents the other domain of the Standard Model, also received an initial, crucial experimental verification, notably the demonstration of the peculiar existence of quarks, which, in turn, gave rise to the mathematical requirement of 'asymptotic freedom.'

(b1) The Discovery of Physical Quarks: Although Gell-Mann had not considered quarks to be real physical objects—though Zweig thought differently—the physicists continued to look for them or, in any case, at the most elementary constituents of hadrons. In his Varenna lectures of 1967, James D. Bjorken conceived a new possibility for an experimental search; he analyzed deep-inelastic electron scattering with the help of the predictions of the sum-rule (derived from current algebra) and noticed: 'We find these relations so perspicuous that, by an appeal to history, an interpretation in terms of elementary constituents is suggested.' (Bjorken, 1968, p. 56) Especially, a sum-rule given by Stephen Adler led to the following inequality for the inelastic electron-nucleon scattering,

$$\int_{q^2/2M}^{\infty} d\nu[W_2^p(\nu, q^2) + W_2^n(\nu, q)] \geq \frac{1}{2}, \tag{914}$$

with W_2^p and W_2^n the proton and neutron structure functions and q^2 the momentum-transfer squared. In the limit of very high energy, the inequality (914) could be written as (with the fine structure constant α)

$$\frac{d\sigma_{ep}}{dq^2} + \frac{d\sigma_{en}}{dq^2} \geq \frac{2\pi\alpha^2}{q^2}. \tag{914'}$$

[1181] Already in the following year, 1984, Carlo Rubbia, the leader of the UA1 collaboration, received the Nobel Prize in Physics, together with Simon Van der Meer, the construction design engineer of the proton–antiproton collider 'for their decisive contribution to the large project, which led to the discovery of the field particles W and Z communicators of weak interaction.' (Citation, in *Nobel Foundation*, ed., 1993, p. 231)

In other words, the sum of (elastic and inelastic) plus electron-neutron total cross sections at fixed q^2 came out to be greater than half the cross section of elastic scattering of electrons from a point-like particle. A couple of years later, Richard Feynman (1969) analyzed the data from the recent deep-inelastic scattering at the Stanford Linear Accelerator Center (SLAC). 'Theoretical interest at SLAC in the implications of the inelastic scattering increased substantially after an August visit by R. P. Feynman,' recalled Henry W. Kendall, one of the experimentalists involved, and further:

> He had been trying to understand hadron-hadron interactions at high energy assuming constituents he referred to as *partons*. On becoming aware of the inelastic scattering data, he immediately saw in partons an explanation both of scaling and the weak q^2 dependence. In his initial formulation (Feynman, 1969) ... he assumed the proton was composed of point-like partons, from which the electrons scattered incoherently.... The partons were assumed not to interact with one another while the virtual photon was exchanged.... Thus, in this theory, electrons scattered from constituents that were "free".... Feynman came to Stanford again, in October 1968, and gave the first public talk on his parton model stimulating much of the theoretical work that ultimately led to the identification of his partons with quarks. (Kendall, 1991, pp. 611–612)[1181a]

The evaluation of the SLAC data extended over years, and the interpretation of the form factor rules finally approached the prediction of the quark model (Friedman and Kendall, 1972). Almost simultaneously, Donald H. Perkins of Oxford reported about measurements with the large heavy-liquid bubble chamber '*Gargamelle,*' and stated that 'the preliminary data on the cross sections provide an astonishing verification of Gell-Mann-Zweig quark model of hadrons' (Perkins, 1972, p. 189).[1182]

(b2) Asymptotic Freedom of Strong Interaction Forces: In contrast to the 'low-energy' experiments—which did not reveal any free quarks but showed them to be strongly-bound as triplets $(q\ q\ q)$ in baryons and as pairs $(q\ \bar{q})$ in mesons—the very-high energy experiments on deep-inelastic electron scattering or neutrino and anti-neutrino scattering on hadrons demonstrated that quarks became free objects at high energies and indeed exhibited their fractional charges. Theoretically, this fact followed from an investigation of David J. Gross and Frank Wilczek of Princeton, who summarized their note on 'Ultraviolet Behavior of Non-Abelian Gauge Theories' to *Physical Review Letters* (received on 27 April 1973) by stating:

> It is shown that a wide class of non-Abelian gauge theories have, up to calculable logarithmic corrections, free-field-theory asymptotic behavior. It is suggested that Bjorken scaling may be obtained by strong-interaction dynamics based on non-Abelian gauge symmetry. (Gross and Wilczek, 1973, p. 1343)

[1181a] For the details of Feynman's visits to SLAC and his work on partons, see Mehra, 1994, Chapter 23, especially, pp. 507–515.

[1182] The experimentalists at the Stanford-MIT collaboration—Richard Taylor, Henry W. Kendall, and Jerome I. Friedman—received the 1990 Nobel Prize in Physics for their fundamental investigations.

'Asymptotic freedom,' i.e., the diminishing of strong interaction to zero interaction at the highest energies, was independently proved by David Politzer of Harvard University: 'The renormalization group technique shows [that] in a Yang-Mills theory ... the effective coupling goes to zero for large momenta' (Politzer, 1973, p. 1346). However, the result depended, as both groups showed, on the number of fermions, i.e., quarks which contribute to the gluon self-energy: 'For example, in the case of $SU(3)$ ... one could accommodate as much as 16 triplets.' (Gross and Wilczek, 1973, p. 1344) On the other hand, for small momenta or large distances between the quarks, higher-order corrections play a role, and semi-empirical models for observed bound quark-antiquark states exhibit linearly increasing forces (see below).

(b3) Quantum Chromodynamics: In a review on 'Current Algebra: Quarks and What Else?' presented at the large Chicago–Batavia High-Energy Physics Conference in September 1972, Harald Fritzsch and Murray Gell-Mann discussed the 'fictitious quarks and "gluons" and their statistics' (Fritzsch and Gell-Mann, 1972, especially, pp. 138–141). 'We assume here that quarks do not have real counterparts, that are detectable in isolation in the laboratory—they are supposed to be permanently bound inside the mesons and baryons,' they argued, and added:

> In particular, we assume that they obey the special quark statistics, equivalent to "para-Fermi statistics of rank three".... The simplest description of quark statistics involves starting with three triplets of quarks, called red [R], white [W] and blue [B], distinguished only by the parameters referred to as color. These nine mathematical entities all obey Fermi-Dirac statistics, but real particles are required to be singlets with respect to the $SU(3)$ of color, that is to say combinations acting like $\bar{q}_R q_R + \bar{q}_B q_B + \bar{q}_W q_W$ or $q_R q_B q_W - q_B q_R q_W - q_R q_W q_B - q_W q_B q_R + q_W q_R q_B + q_B q_W q_R$. (Fritzsch and Gell-Mann, *loc. cit.*, p. 138)

Fritzsch and Gell-Mann claimed that this peculiar way of describing quarks was not common, forgetting about the proposal of Nambu made seven years earlier.[1183] In the fall of 1973, Fritzsch, Gell-Mann, and Heinrich Leutwyler went on to collect the essential ingredients of a non-Abelian gauge-field theory of strong interaction that had meanwhile been assembled in a note on the 'Advantages of the Color Octet Gluon Picture' (Fritzsch *et al.*, 1973). They now sketched the particular Lagrangian

$$\mathscr{L} = -\bar{q}[\gamma_\alpha(\partial_\alpha - iB_{A\alpha}\chi_A) + M]q + \mathscr{L}_B \text{ (Yang–Mills)}, \tag{915}$$

with $B_{A\alpha}$ denoting a neutral vector field represented by a colour-octet ($A = 1, \ldots, 8$) —it describes the 'gluon'-field establishing the strong force between quarks—χ_α denoting a parameter of the colour $SU(3)$ group, and M denoting the quark-mass matrix. Fritzsch *et al.* showed that this *Ansatz* solved all problems that had arisen in the quark model since 1964. The main task, namely, the renormalization of the

[1183] See the investigations of Nambu (1966) and Han and Nambu (1965).

theory had essentially just been accomplished by Gross, Wilczek, and Politzer, based on the renormalization-group techniques pioneered by Ernst C. G. Stueckelberg and A. Petermann, Nikolai Bogoliubov and Dimitri V. Shirkov, and Kurt Symanzik and Kenneth G. Wilson. As a consequence, the coupling constant at the quark-gluon vertex (appearing, say, in the e^+–e^- pair annihilation) would depend on the momentum transfer t as

$$\alpha_s(t) = \frac{4\pi}{\left[11 - \frac{2}{3}N_f(\mu^2)\right]\,\log\left(\frac{t}{\Lambda}\right)}, \tag{916}$$

with N_f being the number of quarks having a mass below Λ; that is, $\alpha_s(t)$ represented a 'running coupling constant.'[1184] It remained to Gell-Mann to baptize the theory properly as 'quantum chromodynamics (QCD).'

(b4) The Completion of QCD: The original quark model of 1964 contained three fundamental objects, u-, d-, and s-quarks (each of which now appeared in three colours). Actually, already in the same year a fourth quark had been predicted by James D. Bjorken and Sheldon L. Glashow on the basis of $SU(3)$-symmetry and the 'fundamental similarity between the weak and electromagnetic interactions of the leptons and the [quark fields] ψ_i' (Bjorken and Glashow, 1964, p. 256). Several years later, Glashow returned to this idea with his collaborators John Illiapoulos and L. Maiani when they proposed 'a model of weak interactions in which the currents are constructed of four basic quark fields and interact with a charged massive vector boson' (Glashow *et al.*, 1970, p. 1285). The model with the c-quark (Bjorken and Glashow, 1964, p. 255, had introduced the extra property 'charm'!) allowed, as desired, to suppress the strangeness-changing neutral current effects; since it had not yet been detected directly, Glashow and Bjorken speculated that it may have a mass as high as 2 GeV/c^2. While investigating the mass of di-lepton events obtained from protons (at the Brookhaven AGS) scattered by a beryllium target with a high-resolution two-arm spectrometer (borrowed from $DESY$), Samuel Ting and collaborators discovered an unexpectedly narrow resonance of about 3100-MeV energy, which they called 'J' (Aubert *et al.*, 1974). 'The most striking feature of J is the possibility that it might be one of the theoretically-suggested charmed particles (see Glashow, private communication)' or another predicted object, they concluded (Aubert *et al.*, *loc. cit.*, p. 1406). The same resonance was also nearly simultaneously obtained with the SLAC electron–positron storage ring SPEAR by Burton Richter and his collaborators (Augustin *et al.*, 1974). The identification with a charmed particle became substantiated; as the narrow resonance (called ψ by the experimentalists in Stanford) represented an electrodynamically decaying object consisting of a charm-quark and a charm-antiquark. Soon an entire family of higher charmed mesons (consisting of a

[1184] From the e^+–e^- pair annihilation, it followed that $\alpha_s(-100\ \mathrm{GeV}^2) \approx 0.2$ and $\Lambda = 112$ MeV (with $N_f = 6$).

pair of c- and u-, d- or s-antiquarks) was found, as well as baryons (containing a c- or a $\bar{c}$-quark). Moreover, the investigation of higher-mass resonance states ψ' and ψ'' allowed one to derive information about the quark–antiquark potential ('charmonium spectroscopy'), confirming the increase of the *QCD*-forces at larger distances.

Even with Glashow's charmed objects having been discovered, the family of quarks had not been completed. In early summer 1977, a study of di-muon states obtained in the 400-GeV proton-nucleus collisions at the Fermilab accelerator in Batavia yielded 'a strong enhancement observed at 9.5 GeV [/c^2] mass' (Herb *et al.*, 1977, p. 252). The enhancement, subsequently baptized 'Upsilon (Υ),' resembled in certain aspects the charmonium and could be interpreted as a 'beauty meson' consisting of a new type of quark, the *b*-quark (with *b* denoting 'beauty' or 'bottom') and the $\bar{b}$-quark (with proper binding forces also giving rise to the excited states Υ', Υ'', etc., and the corresponding spectroscopy). The discovery of the fifth quark may have been expected if one believed in a general lepton–hadron symmetry stating that for each lepton family, a hadron family, i.e., a quark family, also had to exist, i.e., a quark family. Now, in summer 1975, a third type of lepton, the 'τ-lepton' had been announced, having a mass of 1.8 GeV/c^2 and forming with its neutrino ν_τ a third lepton family (Perl *et al.*, 1975). Evidently, to keep up with the third quark family, the *b*-quark had to be completed by '*t*-' or 'top-quark' to another, fourth quark family.' The eager search for the suspected heaviest quark at the largest available accelerators went on for 18 years; then, the Tevatron at Fermilab had the highest collision energies available to produce the $t\bar{t}$-pair, and the detailed, complicated analysis confirmed the existence of the missing top quark, whose mass was given as 199^{+19}_{-21} (statistical error) ± 22 (systematic error) GeV/c^2 (Abachi *et al.*, 1995, especially, p. 2636).

(c) Beyond the Standard Model (1970–1999)

After three decades, the Standard Model had finally been established—even including the direct proof of the intermediate boson of strong interactions, the gluon, in high-energy processes.[1185] To turn the satisfactory picture into a perfect model, only the detection of the Higgs particle in the electroweak theory is missing, which one hopes to achieve very soon—perhaps one has already obtained indications of the missing particle at CERN. The standard model indeed accounts for most of the phenomena exhibited by the innermost structure of matter, and has thus far been checked by hundreds of critical tests during the past 25 years. It rests on the basic theoretical principles known and substantiated since the 1930s, quantum mechanics, special relativity, and locality, and on old and more recently discovered symmetries. Both schemes of electroweak theory and *QCD* share re-

[1185] For instance, at the PETRA ring at *DESY*, one isolated several planar three-jet events, including a noncolinear gluon fragmenting into hadrons (Brandelik *et al.*, 1979); for a study of quark jets, see Mehra, 1994, Chapter 23, pp. 514–519.

normalizability and have minimal couplings. Their predictions turned out to be precise and leave no room for substantial deviations. The special feature of *QCD*, namely, asymptotic freedom, indicates—if one thinks of further unification of the two schemes—that the theory might become simpler for larger energy and shorter distances, with all of the fundamental interactions becoming weak.

Nevertheless the Standard Model also exhibits several defects: Besides involving possibly small violations of parity and time-inversion in strong interactions, it especially does not answer the question of why there are three families in each sector and not more or less—this fact has (so far) been just imposed by observation (or perhaps by the requirement of renormalizability?). In addition, it does not explain the masses of the elementary constituents or explain the other parameters that enter into the theory. Finally, while evidence accumulates for a nonzero mass of the neutrino, the Standard Model leaves no room for it. On the purely theoretical side, although much progress has been achieved in improving the solidity of the mathematical formulation, no strict proof has thus far been given for the problem of quark confinement; there are some indications derived from lattice-group calculations, but the situation still remains controversial. Finally, the theory cannot include the gravitational interaction.

Various attempts have been proposed in the past decades to obtain progress in the questions mentioned above, and a considerable number of mathematical and conceptually intriguing ideas have been suggested. In contrast to the theories which we have sketched in the foregoing, efforts to go beyond the Standard Model have not yet resulted in predictions that make contact with observations; hence, their discussion appears to be a task for prophets rather than for historians. Still, we must be prepared, of course, that even the best and perfectly safe theoretical description of physical phenomena today will not stand the test of future discoveries. To indicate possible trends, let us therefore refer to two opinions expressed recently by two eminent members of the community of quantum theorists at a symposium on the 'Critical Problems in Physics,' held to celebrate the 250th anniversary of the founding of Princeton University, which—since the 1930s—has been one of the leading centres of research in quantum theory.

Edward Witten especially drew attention to the vistas opened by the concepts of 'string theory,' which had emerged from a dual resonance model of the late 1960s. Gabriele Veneziano had then found a remarkable formula satisfying the strong-interaction duality for the scattering amplitude with both poles and Regge behaviour (1968). Attempts to construct a theory from which the Veneziano formula could be derived led to the idea of string theory (see, e.g., Susskind, 1969), which was investigated by a growing number of theorists since the early 1970s. If strings or vibrating membranes acting in many-dimensional spaces (up to 26 dimensions; some of them related to the usual space-time dimensions, and the others perhaps confined or 'compactified,' by quantum conditions to smallest dimensions: cf., the old idea on the fifth dimension of Oskar Klein in 1926) replaced the current concepts of particles and fields; their behaviour might explain not only the known, partly *ad hoc* (because of the empirical evidence) introduced features of the Standard Model, but also even establish a more general description of the innermost constitution of matter (e.g., by bringing in a larger 'supersymmetry') and, thus, allow the incorporation of the gravitational interaction—so Witten argued, and concluded:

> In string theory, we have peeled layers off the onion for 28 years now, and it is still hard to guess how many more layers and surprises there are. In past years, when asked how long the process would take, that has always been my answer. It may still be the correct answer.
>
> However, I have mentioned the black hole state counting problem not merely for its intrinsic beauty and fame. The fact that earlier we could not count the quantum states—or elementary components—of a black hole, and now we can, may be a hint that we are close to closure, that we are now at least close to knowing the basic ingredients in which the theory should be described.
>
> If so, the noncommutative "position" matrices of the *D*-brane may be a good clue about the answer to the "big" questions. A proposal in that direction has been made (by Banks, Fischler, Shenker and Susskind). If their proposal, or something like it, is right, we will probably be doing a completely new kind of physics, with noncommutativity built in at a much more fundamental level, long before the 300th anniversary of Princeton University. (Witten, 1997, p. 278)

These few lines may suffice to give a taste of the direction taken by the thoughts of the 'string' theorists.[1186]

Probably, the advanced proposals of the other Princeton representative of elementary particle theory will sound a bit more familiar. Frank Wilczek, in particular, advocated a unification of all fundamental forces in nature along the path of Howard Georgi and Sheldon Glashow, who had conjectured 'strong, electromag-

[1186] For more details, see, e.g., a new two-volume monograph of Joseph Polchinsky (1998). In the first chapter (of Volume 1), Polchinsky pointed out the advantages of the string approach:

1. *Gravity*. Every consistent string theory must contain a massless spin-2 state, whose interactions reduce at low energy to general relativity.
2. *A consistency theory of quantum gravity, at least in perturbation theory*. As we have noted, this is in contrast to all known quantum *field* theories of gravity.
3. *Grand unification*. String theories lead to gauge groups large enough to include the Standard Model. Some of the simplest string theories lead to the same gauge groups and fermion representations that arise in the unification of the Standard Model.
4. *Extra time dimensions*. String theory requires a definite number of space dimensions, ten. The field equations have solutions with four large flat and six small curves dimensions, with four-dimensional physics that resembles the Standard Model.
5. *Supersymmetry*. Consistent string theories require spacetime supersymmetry, as either a manifest or a spontaneously broken symmetry.
6. *Chiral gauge couplings*. The gauge interactions in nature are parity-asymmetric (chiral). This has been a stumbling block for a number of previous unifying ideas: they required parity-symmetric gauge couplings. String theory allow chiral group couplings.
7. *No free parameters*. String theory has no adjustable constants.
8. *Uniqueness*. Not only are there no continuous parameters, but there is no discrete freedom analogous to the choice of gauge group and representations in field theory: there is a unique string theory. (Polchinsky, 1998, pp. 5–6)

netic, and weak forces to arise from a single fundamental interaction based on the group $SU(5)$' (Georgi and Glashow, 1974, p. 438). Now, both elementary constituents of matter, the quarks and the leptons, appeared to be organized in multiplets; hence, two questions arose: first, how to unify the different couplings in nature; second, transitions between quarks and leptons would occur, in contrast to observations. The first question might be answered with the help of 'running' coupling constants (as in *QCD*). With respect to the second problem, Wilczek pointed to 'a theoretical idea which is attractive in many other ways and seems to point a way out of this impasse,' namely:

> That is the idea of supersymmetry. Supersymmetry is a symmetry that extends the Poincaré symmetry of special relativity (there is also a general relativistic version). In a supersymmetric theory one has not only transformations among particle states with different energy momentum but also between particles of different spin. Thus spin-0 particles can be put into multiplets together with spin-$\frac{1}{2}$ particles, or spin-$\frac{1}{2}$ particles with spin-1, and so forth.
>
> Supersymmetry is certainly not a symmetry in nature: for example, there is certainly no bosonic particle with the mass and charge of the electron.... Nevertheless there are many reasons to be interested in supersymmetry, and especially in the hypothesis that supersymmetry is effectively broken at a relatively low sale, say ≈ 1 TeV. (Wilczek, 1997, pp. 294–295)

Wilczek then discussed the effect of supersymmetry—which had been introduced in the beginning of the 1970s (especially, by Julius Wess and Bruno Zumino, 1974a, b)—on the running coupling constant, finding that it did not change the conclusions obtained, say, in the Standard Model. He argued further that the 'supersymmetric particles ... cannot be much heavier than the $SU(2) \times U(1)$ electroweak breaking scale, i.e., they should not be beyond the expected reach of [the new CERN machine] LHC' (Wilczek, *loc. cit.*, 297). This prospect, at least, connected the speculations with the possibility of an experimental check in the near future.

Altogether, Wilczek advocated supersymmetry as a 'good thing,' because: (i) it united the gauge bosons with the various quarks and leptons; (ii) it served as a basis of future consistent string theories; (iii) it helped to understand the vast disparity between weak and unified breaking scales. He forgot to mention that motivation which had guided the pioneers, namely: 'This symmetry between bosons and fermions gives rise to special (sometimes called miraculous) ultraviolet behavior of the supersymmetric field theories. The simplest theory, $N = 1$ supersymmetry (where N counts the number of supersymmetry generators) is essentially free of quadratic divergences. Theories of higher N, for example $N = 4$, are known to be finite.' (Nilles, 1984, p. 3) There were indeed many reasons which made the study of supersymmetric theories a promising task for the future. However, we repeat our above statement: We are not prophets but historians; hence, we have to bring to a close here the exciting story of the development that elementary particle theory has taken in the most recent times.

2 Quantum Effects in the Physical Laboratory and in the Universe

In a talk on 'High Energy Physics and Big Science,' given in March 1971 at the *Universitätswoche für Kernphysik* in Schladming, Walter Thirring described the value of elementary particle physics and its relation to other branches of science. In particular, he emphasized the advantage of using large equipment to solve certain physical problems, which, although being 'a costly enterprize, it is relatively cheaper than Small Science' (Thirring, 1971, p. 14). Concerning funding, Thirring—both a representative of CERN and a Professor of Physics at the University of Vienna—reported that the industrialized countries, like Austria, spent about 2 to 3 per mille of the governmental budget on pure research, of which high energy physics got a little more than 10%—half of which was allotted to large laboratories and the other half to universities. In comparison with that, certain large industrial companies—like Philips in The Netherlands—invested of the order of 1% of its turnover into pure research, expecting of course benefits from it for the development of their products. Thirring's account in the middle of the period considered here characterized the situation of the financial support of research during the previous 50 years as well. Big Science existed, especially in connection with elementary particle physics or plasma physics, from which the universities profited to a large extent, and besides Small Science, mainly carried out at universities on many problems of physics. The research in industrial laboratories might be characterized as standing halfway between the two extremes. Thirring did not mention at all another important branch of physical research—the military one. It was often carried out as Big Science, devoted to achieving certain military objectives, like missile technology, but the same rockets and satellites, developed and constructed to obtain military superiority over the adversaries, also could be perfectly used for solving astrophysical problems. The appropriate American and European Big Science institutions, the *NASA* and the *ESA*, have sent numerous observational instruments beyond the perturbing terrestrial atmosphere, providing the astronomers and physicists profoundly rich data on old and new celestial phenomena.

Certainly, the progress in quantum theory and its applications owed a great deal to the research in industrial laboratories and the observation of the sky, the beginnings can be traced to the late 1940s and the 1950s (Subsection 2.1). Certain quantum concepts from the first quarter of the 20th century were revived and became particularly fruitful in theoretical, experimental, and technical applications: Planck's 'zero-point energy' via the Casimir effect, Einstein's 'stimulated emission' of radiation via masers and lasers, and Einstein's 'condensation of ideal (Bose) gases' (Subsection 2.2). The solution of the most important problems left over in nonrelativistic quantum mechanics, those of superconductivity and, to a far lesser extent, superfluidity, highlighted the essential progress in low-temperature physics (Subsection 2.3), while a series of new, essentially unexpected effects, from the Mössbauer effect to the fractional quantum Hall effect (discovered by Klaus von Klitzing), revealed and illustrated specific consequences from quantum theory

(Subsection 2.4). Finally, we return to the physics of the universe, where new phenomena like pulsars and especially the 3K blackbody background radiation established relativistic quantum mechanics as providing the deepest insights into the fundamental questions of the origin of the universe and its evolution (Subsection 2.5).

In selecting the above topics, we apologize for not describing in detail the results achieved in such large fields of quantum physics, as listed for instance in the latest 1997 edition of the McGraw-Hill *Encyclopedia of Science and Technology*: *Quantum Acoustics* (dealing with the behaviour of quantized waves or photons); *Quantum Chemistry* (whose practical effectivity has been immensely increased since 1960 by the use of computers); *Quantum Electronics* (including nonlinear optics and working with masers and lasers); *Quantum Gravitation*; *Quantum Mineralogy*; and *Quantum Solids*. With every item on this list, a large community of research workers can be connected, who present their results regularly in topical conferences. However, the few special examples assembled below may suffice to illustrate the power of quantum theory as *the* dominant scheme of 20th-century physics.

2.1 The Industrial and Celestial Laboratories (1947–1957)

(a) The Transistor in the Industrial Laboratory (1947–1952)

For the progress of electronics after World War II, the invention of the transistor in 1947 by John Bardeen, Walter H. Brattain, and William B. Shockley of the Bell Telephone Research Laboratories (Bell Labs) designated the point of departure which started a veritable 'solid state revolution.'[1187] This discovery had certainly been prepared by the quantum-mechanical theory of metal electrons, developed in the late 1920s by Felix Bloch, Alan H. Wilson, and others, on the one hand, and some pioneering work by Walter Schottky in the 1930s on the rectification of electric currents by semiconducting devices (such as copper-cuprous oxide system, selenium), carried out at the Berlin Siemens & Halske Laboratories, on the other hand. Further steps on the path to the transistor provided the development of microwave technology (radar) during World War II, notably, in Great Britain and the United States (where at MIT, the Radiation Laboratory was set up in Cambridge, Massachusetts).[1188] The detailed investigation of the properties of silicon and especially germanium, carried out by an associated research group at Purdue University, led to the following results:

[1187] See the article on 'Electronics' in the recent (15th) edition of *Encyclopeadia Britannica*, Vol. 18 (1977), especially, pp. 212–213.

[1188] For further details on the radar research and the prehistory of the transistor, see Hoddeson *et al.*, 1992, Chapter 7. For the work done at the MIT Radiation Laboratory during World War II, see, e.g., Mehra and Milton, 2000, Chapter 4.

1. Germanium of high purity has been prepared by reduction from pure oxide. By using various impurities in a varying range of concentration (0.001 to 17%), it has been shown that both N-type (excess conductor) and P-type (defect conductor) semiconductors can be produced. B, Al, Ga, In, all produce P-type germanium semiconductors. N, P, As, Sb, Bi, and Sn and other elements produce N-type semiconductors.

...

7. Germanium semiconductors using P or Sb can be used in microwave mixer crystals comparing well in performance to silicon crystals. (Lark-Horowitz, 1946, quoted in Hoddeson *et al.*, 1992, pp. 459–460)

The Purdue results interested people at Bell Labs (also a contributor to the MIT radar project), where, e.g., Walter Brattain as early as 1933/1934 had worked on copper oxide rectifiers. After the war, the scientific administration at Bell Labs decided to get into research on semiconducting materials and their possible applications, with William Shockley—co-head of the Solid State Division—directing a group involving Bardeen as theoretical and Brattain as technical experts. Bardeen's theoretical paper on 'Surface States and Rectification at a Metal Semi-Conductor Contact,' which was received in February 1947 by *Physical Review*, altered the emphasis of their investigations to the consideration of processes occurring on the surfaces. In particular, Bardeen concluded:

> If the density of surface levels with energies in the "forbidden" band is sufficiently high ($> \sim 10^{12}/\mathrm{cm}^2$), there will be a double layer at the free surface of a semiconductor formed from a surface state charge and a space charge of opposite sign. The space charge region is similar to that which exists at a rectifying contact. This double layer tends to make the work function independent of the height of the Fermi level in the interior, and so independent of the impurity content (Bardeen, 1947, p. 725).

After some experimentation on the basis of this idea, Brattain and Bardeen prepared a germanium surface by anodizing it and evaporating gold spots. On 15 December 1947, 'it was found that the current flowing in the forward direction from one contact influenced the current flowing in the reverse direction in a neighboring contact in such a way as to produce voltage amplification' (Report of W. S. Gordon on 'Genesis of the Transistor,' quoted in Hoddeson *et al.*, 1992, p. 469). On the same day, Brattain wrote in his notebook how he looked at a gold spot, cut into three sections, using the area A of the spot as the plate and area B as the grid (in complete analogy to the electron tube notation), and noticed that when the points were 'very close together' he 'got voltage amplification about [a factor of] 2 but not power amplification ... independent of frequency [from] 10 to 10,000 cycles' (quoted in Hoddeson *et al.*, *loc. cit.*, p. 470). By further experiments—the gold spots were replaced by other metal points—especially putting the electrodes closely together, Brattain demonstrated on 23 December 1947, at the Bell Labs, also a power amplification of a factor of 18. The name 'transistor' was given to the new device.[1189] In his monograph, entitled *Electrons and Holes in Semiconductors*, Shockley explained the functioning of an 'idealized transistor structure, using two p–n junctions, which separated the p-type regions P_ε and P_c from the n-type region N_b' (Shockley, 1950, p. 92), as follows:

> In the transistor structure the application of the voltage between emitter and base has an effect similar to the application of voltage between grid and cathode in the vacuum tube. The transition region corresponds to the grid cathode spacing and has a capacitance analogous to the grid cathode capacitance. For example, if a negative potential is applied to N_b, the flow of electrons will change this capacitance (and the collector junction capacitance as well) and will reduce the height of the potential hill over which the holes must drift to reach the collector junction. Thus for the transistor there is a region in which electron flow by one means, that is, excess electrons, controls flow by another means, that is, holes. However, in this case there will always be some recombination, and separation of the current to the same degree as in a vacuum tube will be difficult to achieve. (Shockley, *loc. cit.*, pp. 94–95)

Still, by certain modifications, the p–n–p structure could be made to operate effectively.

The management of the Bell Labs soon became convinced that the transistor represented a major breakthrough in solid-state electronics that offered great technological potential; they invested a great amount of activity in improving the materials (e.g., getting germanium of high purity) and the possibilities of its application. Thus, Ralph Brown, the Director of Research at the Bell Labs, and immediate superior of William Shockley, summarized in a foreword to the latter's book the advantages of the transistor as compared to the vacuum tube:

> Solid state electronics, or transistor electronics, as Dr. Shockley calls it, preceded and in one aspect has always excelled vacuum electronics. This is true in communication engineering at least, for the crystal wireless detector preceded the audion and is still the best detector when going gets tough, as in microwaves. Nevertheless in the past forty years the vacuum tube is the tool which has shaped the whole electrical transmission art. It is an art traditionally based upon highly stable amplifiers in which distortion is precisely tailored to a useful purpose. Evidence is already strong that the transistor devices will be developed having characteristics suitable for such exacting uses.
>
> The newer method of transmission by quantizing and time splitting, together with related fields such as electric computing, requires a large number of power amplifiers and gating and flip-flop circuits to handle pulses and stepwise current changes. Transistor devices are being found to have unique advantages in this type of circuitry. They are tiny, fast and efficient. Here the science of electronics which Dr. Shockley and his colleagues have so efficiently launched promises to lead into new areas of technology. (Brown, in Shockley, *loc. cit.*, pp. vii–viii)

[1189] For details of the dramatic events leading to the discovery of transistor effect, see Bardeen's Nobel lecture (Bardeen, 1964, pp. 333–337).

However, years—in which the Bell Labs got considerable military funding (between 1949 and 1958 about \$8.5 million, representing 25% of the total expenditures on device and material development)—would have to pass before Brown's prophesy became actually true. Even before this, the transistor era in technology began, and on 10 December 1956, Bardeen, Brattain, and Shockley received the Nobel Prize in Physics for 'their investigations on semiconductors and the discovery of the transistor effect.' The report on this event in *Physics Today* still stated cautiously: 'The experimental importance of early transistor research in terms of clarifying the physical picture of electronic conduction considerably outweighed the immediate practical gains from even so sensational a discovery as a miniature electronic device that might be used as an amplifier or to satisfy many of the other functions usually associated with the conventional and far more bulky vacuum tube.' (*Physics Today*, January 1957, p. 16)

In the research on semiconducting materials during the 1950s, the German Siemens Company also participated eagerly. When in 1951 a laboratory at the Erlangen branch (Siemens–Schuckert) was founded, Dr. Heinrich Welker—a former student of Arnold Sommerfeld's and contributor to the theory of superconductivity—joined with a patent containing the idea of the so-called 'III-V compounds.'[1190] As he summarized his idea in the later publication '*Über neue halbleitende Verbindungen* (On New Semiconducting Compounds)':

> The imitation of the semiconducting [chemical] elements of the fourth main sequence of the periodic system (diamond, Si, Ge, grey Sn) by compounds of elements of the third sequence (Al, Ga, Sb) with elements of the fifth sequence (P, Ga, Sb), so far not known to be semiconductors, will be discussed. Because of the quantum-mechanical resonance between homopolar and heteropolar parts of the binding, predictions can be made about the semiconductor properties of these compounds which agree well with experiment. Thus it will be demonstrated that these compounds in certain properties surpass by far those of the elements of the fourth sequence. For example, in InSb electron mobilities up to 25,000 cm^2/Volt sec are obtained. (Welker, 1952, p. 744)

In spite of such advantages, Welker's imitation of the semiconducting elements of the fourth group—later, one also investigated the *II–VI* compounds for semiconductor properties—did not revolutionize the transistor technology. Silicon became *the* preferred raw material, which, compared to germanium and Welker's compounds, retains semiconductivity at higher temperatures—thus, it can be applied up to 200 °C. Ultimately, the silicon transistor replaced the vacuum tubes in big computers but also in the handy electrical tools in common households (e.g., in radio sets). The invention of integrated circuits (in 1958) allowed their use in compact, lightweight electronic missile-guidance systems; in 1971, a further step in microwave technology followed, when the microprocessor (a memory integrated circuit) became available.

[1190] A historical account of the invention of the *III–V* compounds and their use has been given by Ottfried Madelung (1983).

(b) The Celestial Laboratory (1946–1957)

The industrial laboratories, which made use of the previous results from the quantum mechanics of solids for creating new products of the age of microelectronics, clearly possessed a decisive advantage over the laboratories at universities and other research institutions immediately after World War II, as they received financial support not only from their parent companies, but also often from military and governmental sources; during the period of the 'Cold War,' these funds poured in more abundantly than nowadays. For the researchers at universities, however, another 'cheaper' laboratory remained (because the observational tools were supplied by the astronomers) to check old and new consequences from the quantum theory: The stars and other celestial objects offered indeed extra information about nuclear and elementary particle theories. As early as September 1946, George Gamow of George Washington University (Washington, D.C.) submitted a letter to *Physical Review*, dealing with the 'Expanding Universe and the Origin of Elements' (Gamow, 1946). Gamow attributed the observed abundance—especially of heavier nuclei—to 'some kind of unequilibrium process taking place during a limited interval of time' (Gamow, *loc. cit.*, p. 572) and occurring during a period of much faster expansion of the universe 'when the mean density was of the order of 10^6 g/cm^3,' (Gamow, *loc. cit.*, p. 573). In a further note on the topic, written with Ralph Alpher, Gamow—in a stroke of humour, he added the name of Hans Bethe, to complete the sequence 'α, β, γ'—drew detailed consequences from what he imagined to be the situation concerning the state of matter in the early stage of the expanding universe:

> We must imagine the early stage of matter as a highly compressed neutron gas (overheated neutral nuclear fluid) which started decaying into protons and electrons when the gas pressure fell down as the result of universal expansion. The radioactive capture of the still remaining neutrons by the newly formed protons must have led to the formation of deuterium nuclei, and the subsequent neutron captures resulted in the building-up of heavier and heavier nuclei. It must be remembered that, due to the comparatively short time allowed for this process [i.e., about 1 second: see Gamow, 1946, p. 573], the building-up of heavier nuclei must have proceeded just above the upper fringe of the stable elements (short-lived Fermi elements), and the present frequency distribution of various atomic species was attained only somewhat later as the result of adjustment of their electric charges by β-decay. (Alpher, Bethe, and Gamow, 1948, p. 803)

Consequently, Alpher, Bethe, and Gamow attributed the observed abundances 'not to the temperature of the original neutron gas [as was done previously in the equilibrium calculations], but rather to the time period permitted by the expansion process.' They thus derived theoretically a curve for the relative abundance of elements in dependence of their atomic weights by integrating the set of equations,

$$\frac{dn_i}{dt} == f(t)(\sigma_{i-1}n_{i-1} - \sigma_i n_i), \quad i = 1, 2, \ldots, 238, \tag{917}$$

where n_i and σ_i denoted the relative numbers and capture cross sections for nuclei of atomic weight i, and $f(t)$ a factor characterizing the decrease of density with time t. This curve, which showed 'that the relative abundances of various nuclear species decrease rapidly for lighter elements and remain approximately constant for the elements heavier than silver' (Alpher *et al.*, *loc. cit.*, p. 803), fitted the observed data on the average quite well.

While Gamow (1948) and his associates Alpher and Robert Herman (1948, 1949) studied the problem of the behaviour of matter (distribution and total density) in the general relativistic evolution model of the Universe further—it implied, e.g., a unique relationship between the temperature T and time t—the Japanese physicist Chushiro Hayashi pointed out a conceptual defect: If initially only neutrons and photons were present, electron–positron pairs would be created due to the action of the electromagnetic field at the highest temperature, and the positrons would help create protons (via the process $n + e^+ \to p + \bar{\nu}$); thus, there had to exist rather an equilibrium of all these particles (Hayashi, 1950). Several years later, he removed another, even more serious, difficulty of the '$\alpha\beta\gamma$'-theory, namely, the fact that it failed to produce the observed amounts of carbon and oxygen (due to the absence of stable nuclei of mass numbers 5 to 8), by considering the α-capture processes of $3He^4 \to C^{12}$ (Hayashi and Nishida, 1956).

Still, Gamow's view of an evolution of the Universe from a hot condensed phase, the so-called 'Big-Bang' theory (for details, see Alpher and Herman, 1950, 1953) immediately received strong competition. Referring to the then-observed speed of the Universe's expansion, which yielded an expansion time of roughly 2×10^9 years and contradicted the age of the oldest rocks on the Earth, Fred Hoyle (1948) spoke rather in favour of a steady creation of matter, while Hermann Bondi and Thomas Gold expounded the 'perfect cosmological principle,' stating that the universe presents the same large-scale picture at all times to all observers (1948). Very much motivated by this 'Steady-State' theory—although its basis, the high value of the Hubble velocity, had meanwhile been weakened by new astronomical observations (from 500 to 180 km/s), resulting in a much larger possible age of a Big Bang Universe)—E. Margaret Burbidge, Geoffrey R. Burbidge, William A. Fowler, and Fred Hoyle composed their exhaustive report on the 'Synthesis of the Elements in Stars' for *Review of Modern Physics* (1957). They admitted that none of the so-far three proposed theories 'succeeds in meeting all of these requirements' and: 'It is our view that these are mainly satisfied by the fourth theory in which it is proposed that stars are the seat of the origin of elements.' (Burbidge *et al.*, *loc. cit.*, p. 550) The new theory just involved the known 'nuclear transformations currently taking place inside stars' (Burbidge *et al.*, *loc. cit.*). To support their opinion, Burbidge *et al.* took eight types of nuclear processes involved in the synthesis of elements: (i) hydrogen burning (which includes the proton–proton chain of Edwin E. Salpeter, 1952), (ii) helium burning, (iii) α processes (in which α-particles are successively added to synthesize new elements, such as Mg^{24}, Si^{28}, C^{40}, etc.), (iv) e process (i.e., the equilibrium process to build the iron peak in the abundance curve), (v) s process (a long-time scale(n, γ) process),

(vi) *r* process (or short-time sale (n, γ) process), (vii) *p* process (either a (p, γ) process or a process involving a neutron with γ-absorption), and (viii) *x* process (responsible for the synthesis of deuterium, lithium, beryllium, and boron). Evidently, the basic empirical data to check the theoretical calculations of Burbidge *et al.* came from geological studies of the earth's surface (and a little below) and the observation of the visible spectra of stars and other celestial objects.

Up to the early 1950s, optical astronomy provided the only tool to obtain physical information about celestial phenomena, with the 200-in Mt. Palomar telescope, which had been completed in 1949, being the largest instrument. However, the development of radar completed during World War II favoured the emergence of a new important branch of astronomy, radio astronomy (which originated from the discovery of radio emission from the galaxy by Karl Jansky, an engineer at the Bell Labs in 1931). Especially, the discovery of the 21-cm line of molecular hydrogen (predicted in 1945 and detected experimentally in 1951) allowed one to observe previously invisible cold gas clouds in the galaxy and beyond; later followed the discovery of new galaxies (radiogalaxies) and finally that of the surprising quasi-stellar objects (quasars) (spatially relatively concentrated objects of high radio-emission brightness). Both the optical and radio astronomy depend on the possibility of electromagnetic radiation to penetrate through the terrestrial atmosphere—the latter possesses 'windows' for the optical wavelengths, a certain part of the long-wavelength spectrum (only a small window around the 21-cm wave!) but absorbs especially ultraviolet and X-rays. However, since the late 1950s, rockets became available (developed essentially for military purposes), which were sent high up into space and launched satellites; they carried instruments which allowed one to investigate other parts of the spectrum for astrophysical observations, i.e., to establish X-ray and ultraviolet astronomy.[1191] On the other hand, high-energy γ-rays and neutrinos again penetrate through the atmosphere and can also be observed on the Earth. Thus, the Universe has turned into a resourceful labotatory for the human researcher, who makes use of a wide variety of methods known from optics, radio technology, and X-rays to high-energy particle physics. Certain results from these new astrophysical methods, which are especially connected with quantum theory will be recalled later (Subsection 2.5).

2.2 The Application of Known Quantum Effects (1947–1995)

(a) The Casimir Effect and Its Applications (1947–1978)

In a talk presented at the 1998 Workshop on 'The Casimir Effect Fifty Years Later,' Hendrik B. G. Casimir recalled about its origin: 'In 1946, there appeared

[1191] Actually, the first successful study of the ultraviolet radiation from the Sun was made in 1946 by an American Navy Group (which used the further-developed German V-2 rockets). For a condensed historical review of X-ray astronomy and other new astrophysical observation methods, see the article by Michael S. Longair, 1995, especially, Part 3, entitled 'The Opening UP of the Electromagnetic Spectrum.'

a monograph by Verwey and Overbeek, *Theory of the Stability of Lyophobic Colloids*, describing work mainly done in the years 1940 to 1945.' (Casimir, 1999, p. 3). Especially, 'certain suspensions of rather coarse particles were more stable than they ought to be according to theory'; i.e., 'The van der Waals interactions at long distances appear to decrease more rapidly than R^{-6}.' (Casimir, *loc. cit.*, p. 4) He continued:

> It was then that I became interested and felt that I should try to solve the problem.... My young collaborator D. Polder joined me in the effort and played an important part.... At first we were a bit overwhelmed. *QED* in its modern form—renormalization, etc.,—was not yet there, or at least we did not know it. So we had to use makeshift procedures to get rid of infinities.... In the limit of very long distances the interaction [between two neutral atoms] is given by a very simple formula,
>
> $$[\delta E =]U = -\frac{23\hbar c}{4\pi R^7}\alpha_1\alpha_2. \qquad [(918)]$$
>
> [In Eq. (918), α_1 and α_2 denoted the polarizability of the two atoms and R the distance between the two atoms.] (Casimir, *loc. cit.*, p. 5)

On 16 May 1947, the *Physical Review* received the communication of Casimir and Polder, entitled 'The Influence of the Retardation on the London-Van der Waals Forces,' and published it 10 months later (1948). Before working on the interaction between two neutral atoms, Casimir and Polder investigated the simpler problem of the interaction between a neutral atom of polarizability α and a perfectly conducting plane at distance R, and obtained the expression

$$\delta E = -\frac{3\hbar c}{8\pi}\frac{\alpha}{R^4}. \qquad (919)$$

During a visit to Copenhagen, Casimir explained the problem and its solution to Niels Bohr, who thought it over, then mumbled something like '[It] must have something to do with zero-point energy' (Casimir, *loc. cit.*, p. 6). This remark indeed provided a simple method to obtain Eq. (919), as Casimir showed in the following publication 'On the Attraction between Two Perfectly Conducting Plates,' submitted in May 1948 to the Amsterdam Academy (Casimir, 1948).[1192]

Casimir considered a cavity enclosed by (electrically) perfectly conducting walls and placed in it an atom at distance R from one of the walls. Then, the wave modes ω of the electromagnetic radiation contained in the cavity would be displaced; hence, the zero point energy of this radiation $W_0(=\frac{1}{2}\hbar\omega)$ would be

[1192] We have treated the relation between the Casimir–Polder retardation effect and the zero-point energy in detail in a recently published paper (Mehra and Rechenberg, 1999; see also Rechenberg, 1999).

shifted. It turned out that the calculated quantity $\delta_R W_0$ was infinite, but Casimir obtained a finite energy displacement by subtracting the quantity $\delta_\infty W_0$—i.e., the energy shift of the atom was placed very far away from the conducting wall—and thus confirmed the result, Eq. (919). He proceeded to evaluate the zero-point energy shift for two conducting plates separated by a distance a and derived from it the existence of 'an attractive force, ... which is independent of the material of plates' (Casimir, *loc. cit.*, p. 795), namely,

$$F = \frac{\hbar c \pi^2}{240} \frac{1}{a^4} = 0.013 \frac{1}{a^4} \; dyn/cm^2, \tag{920}$$

if a was measured in microns. 'This force may be interpreted as a zero-point pressure of electromagnetic waves,' Casimir stressed and added: 'Although the effect is small, an experimental confirmation seems not to be unfeasible and might be of some interest.' (Casimir, *loc. cit.*) While some theoretical work on the 'Casimir effect' went on in The Netherlands—at Philip's Laboratory in Eindhoven, of which Casimir was director—and in the U.S.S.R.—where Evgeny Lifshitz (1956) and collaborators especially explored the long-range molecular forces and their temperature dependence—it took ten years until M. J. Spaarnay succeeded in substantiating Eq. (920) for distances $a = 0.5$ to 2×10^{-4} cm (Spaarnay, 1958).[1193]

However, after 1960, the zero-point energy effects in cavities became a topic of very intense theoretical and experimental interest. On the one hand, the detailed study of molecular forces and their long-distance behaviour served to describe the properties of refined media, like thin films on the surface of a solid; on the other hand, the Casimir effect was invoked in the theory of elementary particles for explaining quark confinement in the so-called 'bag model.' Even some ingenious speculations about a quantum-theoretical non-Euclidean cosmology made use of the idea of zero-point oscillations. A more recent report on the Casimir effect lists applications in four sections:

[1.] The Casimir effect for various fields and spatial regions.
[2.] Incorporating the real properties of the medium bounding the quantization volume.
[3.] Non-trivial topology of space-time and cosmological applications.
[4.] The Casimir effect in elementary particle physics.

(Mostepanenko and Trunov, 1988, p. 965)

[1193] In summer 1958, in Berkeley, California, Jagdish Mehra told Wolfgang Pauli about his plan to write his doctoral thesis by developing the general theory of London–van der Waals forces on the basis of the covariant quantum electrodynamical methods of Feynman and Schwinger. Pauli immediately approved of the idea and said that he would accept it as his thesis. In December 1958, Pauli died of pancreatic cancer, and Mehra with the help of the award of a handsome fellowship completed his D.Sc. in Switzerland with Charles P. Enz (Pauli's last assistant) and Markus Fierz (Pauli's successor in the theoretical chair at the *ETH*). Mehra developed the quantum electrodynamics of London–van der Waals forces and calculated their temperature dependence results which have been amply verified. Mehra also applied his methods to the theory of imperfect gases in statistical mechanics. (See Mehra, 1963; 1967).

During the past ten years, the interest in the topic has not decreased at all, and the Casimir effect has remained a source of many inspired experimental and theoretical contributions.[1194]

For the last, we have reserved the treatment of Julian Schwinger's interest in the Casimir effect, which was aroused through conversations with Seth Putterman at UCLA,[1195] and perhaps also his conversations with Walter Dittrich may have played a role.[1196] In 1975, Schwinger wanted to explain the effect in the language of his source theory, 'which makes no reference to quantum oscillators and their associated zero-point energy.'[1197] As usual, his presentation was first to his class on field theory, and only then did he write a short paper for publication. Anticipating that the effect of the two polarizations of electromagnetism was merely a doubling of that for a single, massless, scalar mode, his derivation began by obtaining the general expression for the infinitesimal change in the action for a scalar particle under an infinitesimal change in the physical parameters,

$$\delta W = \frac{1}{2} i \int (dx)(dx')D(x,x')\delta D^{-1}(x',x), \tag{921}$$

where D is the massless propagation function of Green's function, or the equivalent change in the energy

$$\delta\varepsilon = -\frac{i}{2}\int (d\mathbf{r})(d\mathbf{r}')d\tau D(\mathbf{r},\mathbf{r}',\tau)\delta D^{-1}(\mathbf{r},\mathbf{r},-\tau), \tag{922}$$

which ignores transient effects. Then, by inserting an appropriate Green's function that satisfies the Dirichlet boundary conditions at $z = 0$, a, written in terms of the longitudinal eigenfunctions, $\sqrt{2/a}\,\sin(n\pi z/a)$, he obtained the following formula for the change in the energy per unit area if the separation is changed by an amount δa, due to the Green's functions in the region $0 < z < a$:

$$\frac{\delta\varepsilon_a}{A} = \frac{1}{4\pi}\frac{\delta a}{a}\frac{1}{i\tau}\frac{d^2}{d\tau^2}\frac{1}{1-e^{-(\pi/a)\tau^2}}, \tag{923}$$

where the limit $\tau \to 0$ is understood. This result is divergent in that limit. But Schwinger then subtracted off the contribution from the region on the other side of the plate, $a < z < L$ (an additional conducting plate is placed at $z = L \gg a$), which may immediately be inferred from Eq. (923) to be

$$\frac{\delta\varepsilon_{L-a}}{A} = -\frac{1}{4\pi}\delta a\frac{1}{i\tau}\frac{d^2}{d\tau^2}\frac{1}{4\pi i\tau}. \tag{924}$$

[1194] See the proceedings of the Leipzig workshop on '*Quantum Field Theory Under the Influence of External Conditions*,' held 14–18 September 1998 (Bordag, 1999).

[1195] See Seth Putterman, Conversation with K. A. Milton, in Los Angeles, California, 28 July 1997.

[1196] See Walter Dittrich, to K. A. Milton, September 1998.

[1197] Julian Schwinger, 1975, p. 43.

The force per unit area is then immediately found from the sum of Eq. (923) and Eq. (924) to be

$$F = -\frac{1}{A}\frac{\partial \varepsilon}{\partial a} = -\frac{\pi^2}{480}\frac{1}{a^4}, \tag{925}$$

indeed, exactly one-half of Casimir's result (920).

Schwinger concluded this note by rederiving the effect of finite temperature, in particular, the high-temperature limit,

$$\mathrm{kT} \gg \frac{\pi}{a}: \quad F_T = -\frac{\zeta(3)}{8\pi}\frac{\mathrm{kT}}{a^3}, \tag{925a}$$

which had first been published by Mehra (1967). Schwinger justified this publication, apart from it giving the Casimir effect a source theory context free from an operator substructure, by quoting from C. R. Hargreaves, who stated that 'it may yet be desirable that the whole general theory be reexamined and perhaps set up anew.'[1198] The context of the latter remark was a discrepancy between the temperature dependence found between conducting plates, and that found by the temperature-dependent Lifshitz formula[1199] when the dielectric constant in the region outside $0 < z < a$ is set equal to infinity, a process which should correspond to a perfect conductor. Unbeknownst to Schwinger, this error had been corrected subsequently.[1200] (Hargreaves had corrected another error in Lifshitz' paper having to do with the effect of perfect conductors.)

It was partly this (nonexistent) discrepancy, but primarily the challenge to understanding the phenomenon in his own language, that led Schwinger, and his postdocs Milton and DeRaad, to write 'Casimir Effect in Dielectrics,' in which the Lifshitz formula for the Casimir force between parallel dielectrics was rederived in an elegant, action-principle–based, Green's function technique.[1201] The key point here was that the effective product of electric fields could be represented in terms of the classical electromagnetic Green's dyadic,

$$\mathbf{E}(\mathbf{r})\mathbf{E}(\mathbf{r}')|_{\mathrm{eff}} = \frac{\hbar}{i}\mathbf{\Gamma}(\mathbf{r}, \mathbf{r}', \omega), \tag{926}$$

where, from Maxwell's equations, the Green's dyadic satisfies

$$-\boldsymbol{\nabla} \times (\boldsymbol{\nabla} \times \mathbf{\Gamma}) + \omega^2 \varepsilon \mathbf{\Gamma} = -\omega^2 \mathbf{1}\delta(\mathbf{r} - \mathbf{r}'). \tag{927}$$

[1198] See C. R. Hargreaves, 1965, p. 236.

[1199] E. M. Lifshitz, (1956); I. D. Dzyaloshinskii, E. M. Lifshitz, and L. P. Pitaevskii, (1961); L. D. Landau and E. M. Lifshitz, (1960), pp. 368–376.

[1200] E. M. Lifshitz, Letter to J. Schwinger, 27 April 1978.

[1201] J. Schwinger, DeRaad, and Milton, 1978a.

From $\mathbf{\Gamma}$, Schwinger, Milton, and DeRaad calculated the changes in the energy, using a method similar to that sketched above, or equivalently, the force directly from the electromagnetic stress tensor,

$$T_{ZZ} = \frac{1}{2}[H_1^2 - H_2^2 + \varepsilon(E_\perp^2 - E_Z^2)], \tag{928}$$

where $\mathbf{H}$ is calculated from $\mathbf{E}$ (and, hence, Γ) using Maxwell's equations. Removing constant divergent terms from the result, the so-called volume stress, which would be present if a given dielectric extended over all space, they succeeded in rederiving the Lifshitz formula. As a special case, they took the perfect conductor limit noted above ($\varepsilon \to \infty$) in the external region and obtained the Casimir result, as well as the high and low temperature limits found by Mehra (1967). They also showed how, in the case of tenuous dielectrics, i.e., in the case when $\varepsilon - 1 \ll 1$, the Casimir force could be thought of as the superposition of the van der Waals attraction between individual molecules (separated by a distance r) that made up the media,

$$\text{large separations:} \quad V = -\frac{23}{4\pi}\frac{\alpha_1\alpha_2}{r^7}, \tag{929}$$

$$\text{small separations:} \quad V = -\frac{3}{\pi r^6}\int_0^\infty d\zeta\, \alpha_1(\zeta)\alpha_1(\zeta), \tag{930}$$

where $\alpha = (\varepsilon - 1)/4\pi N$ is the electric polarizability of the molecules, with number density N. These are the van der Waals potentials originally derived by Casimir and Polder[1202] and Fritz London,[1203] respectively.

These results were all explicitly contained in the much earlier papers by Lifshitz and collaborators (see footnote 1199), to whom due acknowledgement was made. Nevertheless, Lifshitz was somewhat offended by this paper, and he wrote Schwinger a letter:

> Thank you for the preprint of your ... paper ... It was gratifying to know of your interest in my earlier work.
>
> Of course, the method adopted in this paper is far superior than [*sic*] the method which was used in my first paper of 1954. But it seems to me that it is almost identical with the method developed later by I. Dzyaloshinskii, L. P. Pitaevskii, and myself. The derivation of my results by this method was published in our joint paper in *Advances of Physics*, 1961 (identical with the paper in *Soviet Physics Uspechi*, referred to in your preprint); it was also reproduced in the book by Abrikosov, Gorkov, Dzyaloshinskii on the *Field Theoretical Methods in Statistical Physics* (English translation, Prentice Hall, 1963).

[1202] H. B. G. Casimir and D. Polder, 1948, p. 372, Eq. (56).

[1203] See F. London (1930a) for his first tratment of the problem.

> As to the formula for the low temperature limit of the force between the two perfect metallic surfaces (formula 3.17 of your paper), the error in sign in my paper was the result of merely an unfortunate slip in rewriting the Euler summation formula, and not of a deeper origin. This error has since [been] noticed by different authors in our country and elsewhere. (E. M. Lifshitz to J. Schwinger, 27 April 1978)

The only really new result in this paper was an attempt to derive the surface tension for an ideal liquid (liquid helium) from such considerations, by examining the effect of a change of shape of the surface on the energy. 'The second-order change of energy ... is directly related to the surface tension.' (Schwinger *et al.*, 1978a, p. 17) Unfortunately, a quadratically divergent result was obtained. However, with reasonable numbers inserted to provide a physical cutoff to the divergence, a value for the surface tension, and for the latent heat, could be obtained crudely in agreement with the observed values to within a factor of two or three. The idea remains provocative yet unresolved.

A few months later, the same three authors wrote a second paper on Casimir phenomena, entitled 'Casimir Self-Stress on a Perfectly Conducting Spherical Shell' (Schwinger *et al.*, 1978b). The impetus for this work went back to another paper of Casimir, in which he suggested that the attractive Casimir force could balance the Coulomb repulsion of a semiclassical model of an electron (Casimir, 1953). More precisely, it had long been known that a purely electromagnetic classical model of an electron was impossible, that one had to add the so-called Poincaré stresses to stabilize the particle. Casimir had then suggested that those stresses could arise from quantum mechanics. Indeed, if a reasonable guess extrapolated from the parallel plate calculation was used, one could calculate a value for the charge on the electron, or better, the fine structure constant, $\alpha = e^2/\hbar c$, consistent, perhaps, with the experimental value, $\alpha = (137.036\ldots)^{-1}$.

It remained for Timothy Boyer, a student of Sheldon Glashow's at Harvard, to take up the challenge of a real calculation for the spherical geometry in 1965. He calculated the change in the zero-point energy due to the presence of a perfectly conducting spherical shell of radius a. Both modes interior to and exterior to the shell had to be included in order to get a final finite result. This impressive calculation was difficult and subtle, and involved extensive numerical calculation. His result, obtained after three years of work, was accurate to only one significant figure, but it was of *opposite* sign compared to the one found by Casimir in the parallel geometry (Boyer, 1968):

$$E_B = +\frac{0.9}{2a}. \tag{931}$$

His expression was subsequently evaluated more accurately, to three significant figures by B. Davies (1972).

Because this result was so surprising, and devastating to Casimir's electron model, it was an obvious target for a recalculation by Schwinger and his postdocs, now that their improved Green's function machinery had been honed. By the end of 1977, they had derived a compact formula for the Casimir energy of a conducting shell, much simpler than that of Boyer,

$$E = -\frac{1}{2\pi a}\sum_{i=1}^{\infty}(2l+1)\frac{1}{2}\int_{-\infty}^{\infty} dy\, e^{iey} x \frac{d}{dx}\log(1-\lambda_l^2), \tag{932}$$

where the sum is taken over the different angular momentum modes, the integral over (imaginary) frequencies, $y = \frac{1}{2}\omega a$, the quantity $x = |y|$, and the logarithm depends on

$$\lambda_l(x) = (s_l e_l)'(x) \tag{933}$$

(where the prime denotes differentiation). The functions e_l and s_l are given in terms of modified Bessel functions,

$$s_l(x) = \sqrt{\frac{\pi x}{2}} I_{l+1/2}(x), \tag{934}$$

$$e_l(x) = \sqrt{\frac{2x}{\pi}} K_{l+1/2}(x). \tag{935}$$

The expression (932), which is formally divergent, has been regulated by evaluating the underlying Green's function at unequal times, $t = t' + \tau$, i.e., by 'time-splitting.' At the end of the calculation, one is to take the limit $e = \tau/a \to 0$. Unfortunately, at this point, Milton and DeRaad had a bit of difficulty in seeing how to extract a number from this formula, so a few months passed. (Schwinger had contented himself with deriving the formula.) Unfortunately, because just at that point a paper by Balian and Duplantier appeared, who obtained a different formula, based on a multiple scattering formalism, and obtained a result, consistent with Boyer's number, but now accurate to three significant figures (Balian and Duplantier, 1978). So the postdocs worked hard, discovered how to extract a reliable answer based on the sue of uniform asymptotic approximations (the first term of which was accurate to 2%, while Balian and Duplantier's first approximation was only accurate to 8%), and obtained the result accurate to *five* significant figures,

$$E = \frac{0.92353\underline{1}}{2a}. \tag{936}$$

The reaction from Boyer and Balian was rather unexpected. In a letter to Lester DeRaad (DeRaad and Boyer, of course, had been fellow graduate students at Harvard), Timothy Boyer wrote: 'The calculations presented seem sophisticated, and presumably are carefully done. However, the comments on my work in the text of the Casimir paper are hardly generous; my colleagues would characterize them differently.' (Boyer to DeRaad, 12 May 1978) He went on to apprise DeRaad of the Davies calculation, and to give further experimental references, which were incorporated into the published papers. In addition, an appreciative comment about Boyer's work was inserted (Schwinger *et al.*, 1978b, p. 388).

Roger Balian wrote Schwinger to say:

> I guess it would be interesting to compare our respective approaches, which have the common feature of being based on the elimination of fields and consideration of sources. Our formalism was mainly intended to deal with arbitrary geometries; it is based on an expansion which converges rapidly in cases of interest (slightly deformed conducting sheet, spherical shell, etc. . . .). However, we construct the electric Green's function in terms of fictitious monopole currents, and restrict to conductors. Your approach has the advantage of allowing the treatment of dielectrics; I do not see, however, how to use it for arbitrary geometries; on the other hand, would you obtain instabilities of the surface of a dielectric at $T \neq 0$, thus generalizing the effect which we pointed out for the conducting foil?' (Balian to Schwinger, 28 December 1977)

Since this letter was dated 28 December 1977, more than five months before Schwinger's paper on the Casimir effect for a sphere was submitted (Schwinger *et al.*, 1978b), it seems likely that at that point Balian had only seen the dielectric paper (Schwinger *et al.*, 1978a), hence, the remark about geometries.

Schwinger's first papers on the Casimir effect were influential, not for their explicit results, which were mostly well known, but for the development of powerful techniques for attacking such problems, which continue to be exploited. (A recent example is the study of the dimensional dependence of the Casimir effect in hyperspheres by Bender and Milton (1994) and Milton (1997)). However, Schwinger continued his involvement with the Casimir effect for the rest of his life. In the 1980s, he explored, but did not publish, the related connection between acceleration and thermal radiation; and in the last few years of his life, he suggested that the remarkable phenomenon of sonoluminescence was due to the dynamical Casimir effect. (For details, see Mehra and Milton, 2000, Chapter 15, Section 5.)

(b) The Maser and the Laser (1955–1961)

'A new type of device is described below [which] can be used as a microwave spectrometer, a microwave amplifier or as an oscillator,' J. P. Gordon, H. J. Zeiger, and Charles H. Townes thus introduced a paper that was received on 4 May 1955, by *Physical Review*, and explained further:

> The device utilizes a molecular beam in which molecules in the excited state of a microwave transition are selected. Interaction between these extended molecules and a microwave field produces additional radiation and hence amplification by stimulated emission. We call an apparatus utilizing this technique a "maser," which is an acronym for "*m*icrowave *a*mplification by *s*timulated *e*mission of *r*adiation. (Gordon, Zeiger, and Townes, 1955, p. 1264; our italics)

The concept of 'stimulated emission' suggested by Albert Einstein to derive Planck's radiation law from statistical considerations (Einstein, 1916d) had not aroused the experimentalists for more than three decades to search for a direct verification. However, the situation changed in the beginning of the 1950s, especially when it was demonstrated that an inversion of the population of quantum states—i.e., the artificial increase of the population of the higher states as compared to thermal equilibrium—might be actually produced practically (e.g., Purcell and Pound, 1951). Charles H. Townes of Columbia University then became interested in this possibility, when he realized that 'probably only through the use of molecular or atomic beams could coherent oscillators for very short waves be made' (Townes, 1965, p. 832). The design of the first apparatus to accomplish this task, was described by the same authors a year earlier in a letter to *Physical Review*, entitled 'Molecular Microwave Oscillator and New Hyperfine Structure in the Microwave Spectrum of NH_3,' as follows:

> A beam of ammonia molecules emerges from the source and enters a system of focusing electrodes. These electrodes establish a quadrupolar cylindrical electrostatic field whose axis is in the direction of the beam. Of the inversion levels, the upper states experience a radial inward (focusing) force, while the lower states see a radial outward (focusing) force. The molecules arriving at the cavity are then virtually all in the upper states. Transitions are induced in the cavity, resulting in a change in the cavity power level when the beam of the molecules is present. Power of varying frequency is transmitted through the cavity, and an emission line is seen when the klystron frequency goes through the molecular transition frequency. (Gordon, Zeiger, and Townes, 1954, pp. 282–283)

Gordon *et al.* achieved their goal of obtaining the self-sustained oscillations and thus the amplification of the microwaves. Independently, and at about the same time, Nikolai G. Basov and Alexander M. Prokhorov (1955) of the Lebedev Institute in Leningrad suggested a similar idea for the 'laser' (i.e., '*L*ight *A*mplification by the *S*timulated *E*mission of *R*adiation').

A couple of years later, a Dutch associate of the Cruft Laboratory at Harvard University thought of the next step forward in the technology of the stimulated emission of radiation: Nicolaas Bloembergen, in his 'Proposal for a New Type of Solid State Maser,' called 'attention to the usefulness of power saturation of one transition in a multiple energy level system to obtain a change of the sign of the population difference between another pair of levels,' referring to an earlier consideration of Albert W. Overhauser (Bloembergen, 1956, p. 324).[1204] Bloembergen discussed theoretically the case of three unequally spaced energy levels,

[1204] Overhauser had proposed to produce polarized nuclei via resonance absorption of microwaves by atomic nuclei of a metallic solid, due to the existence of electronic spin resonance from electrons in the conduction band, and found: 'The metal is then in a nonequilibrium condition, and we shall show that the dynamic processes which tend to restore the system to its equilibrium state induce nuclear transitions in predominantly one direction, with a resulting steady state nuclear polarization.' (Overhauser, 1953, p. 411) This result has been called the 'Overhauser effect.'

$E_3 > E_2 > E_1$, and derived a positive or negative population difference,

$$n_1 - n_2 = n_3 - n_2, \tag{937}$$

depending on whether

$$w_{21}v_{21} > w_{32}v_{32} \quad \text{or} \quad w_{21}v_{21} < w_{32}v_{32}, \tag{938}$$

where w_{21} and w_{32} denote the transition probabilities connected with the frequencies v_{21} and v_{32}. He believed that the situation of such overpopulated states (either n_3 or n_2) was realized in the cases of certain nickel and gadolinium salts. As Charles Townes recalled in his Nobel lecture (he shared the 1964 Nobel Prize in Physics with the Russian maser–laser pioneers Nikolai Basov and Alexander Prokhorov): 'Until about 1957, the coherent generation of frequencies higher than those which could be obtained from electronic oscillators still had not been directly attacked, although several schemes using molecular-beam masers for the far-infrared were examined from time to time,' and continued:

> But joint work with A. L. Schawlow, beginning at about this time helped open the way for fairly rapid and interesting development of the maser oscillators in the far-infrared, optical, and ultraviolet regions—as much as 1,000 times higher in frequency than any coherent sources of radiation previously available. It is masers in these regions of the spectrum, frequently called lasers (*l*ight *a*mplification by *s*timulated *e*mission of *r*adiation), which have perhaps provided the most striking new scientific tools and results. (Townes, 1965, p. 835)

After 1957, Arthur Schawlow of Bell Labs and Charles Townes indeed pointed out how to extend the maser techniques to the shorter-wavelength regions 'by using a resonant cavity of centimeter dimensions, having many resonant modes . . . by pumping with reasonable amounts of incoherent light' (Schawlow and Townes, 1958, p. 1940), that is, by making use of Bloembergen's method of 'optical pumping.' They further claimed that 'a good many crystals, notably rare earth salts' may be available to achieve the desired goal, although there still remained the practical problem of populating the upper states. Schawlow and Townes concluded: 'For reasonably favorable maser design in the short wavelength regions, highly reflecting surfaces and means of efficient focusing of radiation must be used.' (Schawlow and Townes, *loc. cit.*, pp. 1948–1949)

It took two further years before Theodore H. Maiman of the Hughes Research Laboratory in Malibu, California, could report a breakthrough in the laser problem. In his note on 'Optical and Microwave-Optical Experiments in Ruby,' submitted to *Physical Review Letters* in April 1960, he announced 'the first observation of ground state changes in ruby due to optical absorption between two excited states in this crystal' (Maiman, 1960a, p. 564). About a couple of months later, he confirmed (in a letter published in the August issue of *Nature*) 'Stimulated Optical Radiation in Ruby,' obtained by applying the pumping method (of Bloembergen), advocated by Schawlow and Townes, to his crystal (Maiman, 1960b). The latter contained, in particular, triply-ionized chromium, and Maiman stated:

> When this material is irradiated with energy at a wavelength of about 5500 Å, chromium ions are excited to the $^4F_2$ state and then quickly lose some of their excitation energy through non-radiative transition to the E_2 state. This state then slowly decays by spontaneously emitting a sharp doublet, the components of which at 300K are at 6943 Å and 6929 Å. Under very intense excitation the population of this metastable state (^2E) can become greater than that of the ground state; this is the condition for negative temperatures and consequently amplification via stimulated emission. (Maiman, 1960b, p. 493)

In order to demonstrate this effect, Maiman used a ruby crystal of 1-cm dimension coated on two parallel faces with silver and irradiated it by a high-power Xenon flash lamp emitting 5500 Å radiation that caused absorption into the lower band and thus transition from the $^4A_2$ state to the $^2F_2$; he then obtained an enormously bright line at 6943 Å and noticed: 'These results can be explained on the basis that negative temperatures were produced and regenerative amplification ensued.' (Maiman, 1960b, p. 494)

In the two detailed papers on 'Stimulated Optical Emission in Fluorescent Solids,' Maiman described in detail the functioning of the laser both theoretically (Maiman, 1961) and experimentally (Maiman *et al.*, 1961). Still, in December 1960, a collaboration at Bell Labs demonstrated 'Population Inversion and Continuous Optical Maser [i.e., Laser] Oscillation in a Gas Discharge Containing a He-Ne Mixture'; that is, the first gaseous laser was realized (Javan, Bennett, and Herriott, 1961). The invention of the laser revolutionized optics and assisted as a tool in many other disciplines beside physics; especially, numerous applications could be found in optoelectronics, a field, where laser technology and the ideas of nonlinear optics combined effectively. Nicolaas Bloembergen and Arthur Schawlow would share the 1981 Nobel Prize in Physics for their contributions to nonlinear optics and the use of the lasers.[1205]

(c) Bose–Einstein Condensation (1980–1995)

On 10 July 1924—just a week after he had submitted his German translation of Satyendra Nath Bose's derivation of Planck's radiation law to *Zeitschrift für Physik* (Bose, 1924a)—Albert Einstein presented a communication to the Prussian Academy dealing with the '*Quantentheorie des einatomigen idealen Gases* (Quantum Theory of the Monatomic Ideal Gas),' thereby transferring Bose's new quantum-statistical method of light-quanta to the atoms of noble gases (Einstein, 1924c).[1206] Einstein then discovered a peculiar behaviour of the ideal gas, which

[1205] For a history of optoelectronic physics, we refer to Brown and Pike, 1995, especially, pp. 1417–1432.

[1206] A detailed discussion of Einstein's paper has been given in Volume 1, Part 2, Section V.3.

he called 'degeneracy,' a consequence that—in spite of the otherwise full acceptance of the Bose–Einstein statistics as such—was doubted by many quantum physicists. More than half a century later, three experimentalists from Paris reminded the community again of Einstein's conclusion, summarizing it in the introduction of their note, entitled 'Evidence for Bose-Einstein Statistics in an Exciton Gas':

> It is well known that in an ideal Bose gas the energy distribution of the particles obeys the relation
>
> $$N(E) = \rho(E) f(E), \tag{[(939)]}$$
>
> where $\rho(E) = AE^{1/2}$ is the density of states, $f(E) = \{\exp[(E - \mu)/kT] - 1\}^{-1}$ is the occupation number of levels of energy E, measured from the lowest level $E = 0$, and μ is the chemical potential of the gas, determined by the condition that $\sum_E N(E) = N_t$, the total number of particles. In usual relations, the ratio $-\mu/kT \gg 1$, and the relation [(939)] is well approximated by Maxwell-Boltzmann distribution. If $-\mu/kT < 2$, differences from classical statistics occur with a tendency for particles to accumulate in the states of lowest energy. This quantum effect becomes precipitous if $N_t > N_c = 6.2 \times 10^{15}\ g(m/m_0)^{3/2} T^{3/2}$ (m_0, free electron mass; m, a particle mass; g, degeneracy factor), for which $\mu = 0$, giving rise to a phase transition with a macroscopic occupation of a single quantum state $E = 0$ (Bose-Einstein condensation *BEC*). (Hulin, Mysyrowicz, and Benoît à la Guillaume, 1980, p. 1970)

The authors continued: 'It is generally admitted that *BEC* is the physical origin of the spectacular properties of ^{4}He below the λ-point [i.e., superfluid helium] although an interpretation using the ideal Bose gas as a starting point raises serious questions, because of the strong interactions between atoms in liquid helium. It is therefore important to search for new, more dilute Bose systems, in which purely statistical effects are predominant, and where the interparticle actions may be treated as a small perturbation.' (Hulin *et al.*, *loc. cit.*, pp. 1970–1971) They then presented evidence that a gas of free excitons in Cu_2O 'indeed may manifest the quantum-statistical behavior of a dilute Bose gas,' because especially the density of the orthoexcitons as a function of the effective particle temperature reached the critical density for *BEC*, as predicted by an ideal Bose-gas model (Hulin *et al.*, *loc. cit.*, p. 1971, and p. 1972, Fig. 3).

In the following years, several groups studied the exciton situation in the Cu_2O-system more carefully. Thus, Jia-Ling Lin and J. P. Wolfe reduced the multiplicity of the orthoexciton ground state by greatly applying uniaxial stress and found: 'The paraexciton luminescence spectrum develops an extra component at low energy which is interpreted as a Bose-Einstein condensate.' (Lin and Wolfe, 1993, p. 1222) Still, two years later, a group at the National Institute of Standards and Technology and the University of Colorado at Boulder criticized the Cu_2O results, because 'the interactions in these systems are weak but poorly understood, and it is difficult to extract information about the excitons from the experimental data'

(M. H. Anderson *et al.*, 1995, p. 198); on the other hand, they simultaneously reported 'evidence of *BEC* in a dilute, and hence weakly interacting, atomic vapor.' They summarized their procedure and results as follows:

> A Bose-Einstein condensate was produced in a vapor of rubidium-87 atoms that was confirmed by magnetic fields and evaporatively cooled. The condensate fraction first appeared near a temperature of 170 nanokelvin and a number density of 2.5×10^{12} per cubic centimeter could be preserved for more than 15 seconds. (M. H. Anderson *et al.*, *loc. cit.*, p. 198)

M. H. Anderson, J. R. Enscher, M. R. Matthews, C. E. Wieman, and E. A. Cornell cited three primary signatures of *BEC* in their (above-mentioned) paper: First, a narrow peak in the thermal velocity distribution appeared at zero velocity; second, the fraction of atoms in this low-velocity peak increased abruptly as the sample temperature was lowered; and third, the peak exhibited a nonthermal, anisotropic velocity distribution as expected for the quantum state of minimum energy (and opposed to the isotropic, thermal velocity distribution observed in the broad uncondensed fraction).

The year 1995 actually brought more confirmation for the condensation effect in Bose gases. After the article of Anderson *et al.* had appeared in the *Science* issue of 14 July 1995, two further collaborations reported the effect with other alkali atoms: one group at Rice University in Houston, Texas, used spin-polarized Li-atoms confined in a permanent magnetic trap and cooled to 100 to 400 nanokelvin (Bradley *et al.*, 1995), and the other at MIT worked with a gas of sodium atoms (K. B. Davis *et al.*, 1995). The latter group wrote: 'Our results are distinguished by a production rate of Bose condensated atoms which is 3 orders of magnitude larger than in the previous experiments.' The authors 'trapped the atoms in a novel trap that employed both magnetic and optical forces,' and used evaporative cooling to increase 'the phase-space density by 6 orders of magnitude within seven seconds'; thus, they obtained condensates containing up to 5×10^5 atoms at densities exceeding 10^{14} cm^{-3}, and found:

> The striking signature of Bose condensation was the sudden appearance of a bimodal velocity distribution below the critical temperature of $\sim$2 μK. The distribution consisted of an isotropic thermal distribution and an elliptical core attributed to the expansion of a dense condensate. (Davis *et al.*, *loc. cit.*, p. 3969)

Thus, an advanced technique of trapping atoms (by magnetic fields and laser beams) and of cooling them down to nanokelvin degrees ultimately led to a direct experimental proof of a prediction made 70 years earlier. Although the Bose–Einstein condensates appear under quite exotic physical circumstances (whose practical applications might be limited), their verification endows this essential consequence from quantum-mechanical statistics with a physical reality.

2.3 Superfluidity, Superconductivity, and Further Progress in Condensed Matter Physics (1947–1974)

The quantum-mechanical theory of condensed matter, after an initial breakthrough in the 1920s (notably, on the electron theory of metals) had been worked out to a large extent in the 1930s (see, especially, Section IV.4). Besides the description of metals and insulators and the treatment of dense matter, the phenomena of superfluidity had begun to be attacked successfully (in particular, by the approaches of Fritz London, Laszlo Tisza, and Lev Landau). The onset of World War II interrupted almost all of these activities, but after the end of the war, they were resumed with great energy, and a new, younger generation of quantum theorists joined the efforts of established theoreticians, like Nevill Mott in England, John Slater and J. H. Van Vleck in the United States, and Lev Landau in the Soviet Union. Apart from semiconductor physics, great advances were soon made in many topics of condensed matter physics, as, for instance, in understanding metals and alloys, magnetic and ceramic materials, in which the quantum-theoretical concepts entered crucially.[1207] In the following subsection, we shall focus on several items from the theory of condensed matter at low temperatures and that of the modern theory of phase transitions.[1208]

(a) Rotons and Other Quasi-Particles (1947–1957)

In 1947, Lev Landau published an improved version of his earlier theory of superfluidity (1941a, b), because recent experiments on the velocity of the 'second sound' had revealed certain discrepancies. He now replaced the original *Ansatz* for ε, the energy of the roton, as a function of its momentum,

$$\varepsilon = \Delta + p^2/2\mu \tag{940}$$

(with μ the roton mass and Δ its minimum energy), by a generalized one,

$$\varepsilon = \Delta + \frac{(p - p_0)^2}{2\mu}. \tag{940$'$}$$

This *Ansatz* now described the real situation well, but removed the strict qualitative difference between the phonon and roton excitations (Landau, 1947). Of course, Landau's theory also lacked the atomistic foundation, which Nikolai Bogoliubov of Moscow State University sought to supply. He considered a sys-

[1207] For historical reviews, we may refer to the articles of Stevens (1995), Pippard (1995), and Cahn (1995), as well as to Hoddeson *et al.* (1992), Chapters 6 and 7.

[1208] For details of the historical development of items discussed here, see Hoddeson *et al.*, 1992, Chapter 8, and Mehra, 1994, Chapter 17, for some particular aspects.

tem of N bosons confined in a volume V and interacting through a hard-sphere potential of extension a (Bogoliubov, 1947). However, in spite of the clear results obtained, the condition assumed (i.e., $Na^3V \gg 1$ failed to be satisfied in the case of liquid helium II (where $Na^3V \simeq 0.2$). Still, Bogoliubov's method of introducing 'quasi-particles' both to describe Landau's phonons and rotons became standard in the future theory of collective phenomena.

It was at this stage, when in spring 1953, that Richard Feynman entered the scene. He set himself the task of providing a theoretical understanding of the problem of liquid helium on an atomic basis, which could only be done if one approached the problem from first principles. While he greatly admired Landau's contributions to and successes in the field, Feynman pointed out several weaknesses in Landau's theory. Notably, Landau's quantum hydrodynamical approach treated Helium II as a *continuous* medium which right from the beginning sacrificed the atomic structure of the liquid and thus forestalled the possibility of calculating the various characteristics of the system, such as the various parameters, on an atomic basis. In his first paper on the 'Atomic Theory of the λ Transition in Helium' (Feynman, 1953a), he showed 'from first principles that, in spite of the large interatomic forces, liquid He^4 should exhibit a transition analogous to the transition in an ideal gas' (Feynman, *loc. cit.*, p. 1291). By writing 'the exact partition function as an integral over trajectories, using the space-time approach to quantum mechanics,' Feynman could indeed derive a Landau-type energy spectrum (Feynman, 1953b) and further demonstrate how phonon-like excitations evolved into roton-like ones at large momenta (Feynman, 1954).[1209] Feynman also reconsidered the theory of helium II 'superflow' (first treated by Landau, 1941b) from his point of view—the viscosity η for $T > T_\lambda$ remains zero for velocities smaller than a critical one but rises sharply beyond (Feynman, 1955). For that purpose, he introduced the concept of 'vortices' into the quantum-mechanical treatment as follows: He noticed that the wave function of the fluid system in the lowest state (laminar flow) should contain a phase

$$\psi_{\text{flow}} = \left[\exp i\left(\sum s(\mathbf{R}_i)\right)\right]\Phi, \tag{941}$$

with $s(\mathbf{R})$ being given by

$$s(\mathbf{R}) = \hbar^{-1}\, m\mathbf{v}(\mathbf{R}) \cdot \mathbf{R} \tag{941a}$$

and $\mathbf{v}$ representing the phase velocity; i.e.,

$$\mathbf{v} = \hbar m^{-1} \nabla s \tag{941b}$$

[1209] Two years later, Feynman and his graduate student Michael Cohen published an improved evaluation of the energy spectrum (based on the results of Cohen's Ph.D. thesis); e.g., they obtained the minimum roton energy Δ to be about 11.5 times Boltzmann's constant, in good agreement with observation (Feynman and Cohen, 1956, especially, p. 1189).

(where m denoted the mass of the particles and $\mathbf{R}$ denoted the space point). In this case, the quantum condition,

$$m \int \mathbf{v}_s \, d\mathbf{s} = 2\pi n\hbar = 2\pi n 1.5 \times 10^{-4} \text{ cm}^2/\text{sec}, \quad (n = 0, 1, 2, \ldots), \tag{942}$$

had to be satisfied for the superfluid velocity $\mathbf{v}_S$ [Feynman, 1955, p. 35, Eq. (18)]. Now, in the excited states, vortex lines would arise in the fluid, whose properties Feynman evaluated. In particular, he found a critical velocity $\mathbf{v}_0$ for a helium flow when passing through a slit of width d, above which resistance occurred, i.e., vortex lines would be created, following from the equation

$$v_0 d = \hbar m^{-1} \log(d/a), \tag{943}$$

with a assuming the order of the atomic distance (Feynman, 1955, p. 46). The value derived from Eq. (943) turned out to be only slightly higher than the observed one.

In September 1956, Feynman presented the above results in a talk on 'Superfluidity and Superconductivity,' at the International Congress on Theoretical Physics, held in Seattle, Washington.[1210] After discussing the superfluidity of He^4, he added a few remarks on 'another liquid of great interest,' namely, He^3, stating in particular:

> I do not believe that He^3 is a superfluid. I think there are an enormous number of states reaching [down] to absolute zero.... If there is a high density of states then He^3 will not be a superfluid in the sense of He^4....
>
> Another interesting problem is the following example from He^3. The particles interact with strong forces in a Fermi system. The problem is to determine the temperature-dependence of the viscosity as T goes to zero. There are a large number of similar problems in He^3, and it would be fun to do them before the experiments, for the first time. I don't think that anybody has ever computed anything in solid state physics before the experimental result was out, so we have consistently predicted only what we have observed. (Feynman, 1957, p. 208)

Now, before Feynman even delivered the above talk, the Soviet journal *JETP* received Landau's investigation on 'The Theory of a Fermi Liquid' (1957). In it, Landau considered a Fermi gas at temperatures much below the degeneracy temperature, and then introduced a weak interaction which he turned on gradually, such that the gas would only slowly transform into a liquid with the atoms turning into elementary excitations or quasi-particles which still obey Fermi statistics. Finally, for his Fermi liquid, Landau derived a series of general properties—such as the effective mass of excitation, the compressibility, and the magnetic susceptibility—but he still did not address the He^3 problem.

[1210] Feynman had hoped to be able to discuss his ideas with Lev Landau, who was, however, unable to attend. (See Feynman, 1957, p. 205)

A year later, V. P. Silin extended Landau's new theory to describe the behaviour of electrons in metals (1958). In his publication, he referred to the earlier work on a slightly different, 'collective approach' pursued jointly by David Bohm and David Pines of Princeton University, in a series of papers submitted since late 1950 to *Physical Review* (e.g., Bohm and Pines, 1951). The latter had started from the classical plasma oscillations (resembling sound waves) of Lewi Tonks and Irving Langmuir (1929) which took into account in a natural way the long-range correlations in a metal and translated them into quantum theory. As Pines summarized in a review article:

> The valence electrons in a solid, as a result of Coulomb interactions, are capable of carrying out collective oscillations at high frequency which differ substantially from the majority of the ω_{n0} [i.e., the electronic transition frequencies of the individual-particle picture] and depend, approximately, only on electron charge, mass and density of the solid. (Pines, 1956, p. 185)

The quantum of the plasma oscillations was called 'plasmon' and possessed the energy,

$$\hbar\omega_p = \hbar\left(\frac{4\pi ne^2}{m}\right)^{1/2}, \tag{944}$$

with n denoting the density of the valence electrons, and e and m denoting the charge and the mass electron.

The quasi-particles (phonon, roton, and plasmon, i.e., the quantum-theoretical objects derived in general in the quantum-field theoretical formalism) served well to describe a variety of phenomena in condensed matter physics; however, they did not suffice to solve the old riddle of solid state physics: superconductivity. In the second half of his talk at the Seattle Conference in fall 1956, Feynman also pondered about it, but apart from qualitative remarks on the nature of the ground state, he could just offer vague expectations, such as: 'When we do finally understand this gap, we will understand how the energies will vary with the angular momentum of the states that are created.' (Feynman, 1957, p. 211) He ended his talk with the statement: 'The only reason that we cannot do this problem of superconductivity is that we haven't got enough imagination.' (Feynman, *loc. cit.*, p. 212) At about the same time, there appeared Volume XV of the Springer *Handbuch der Physik*, carrying an extended article on superconductivity by John Bardeen who—for many years—had been an expert on this subject. He admitted that 'in spite of the large amount of experimental and theoretical work devoted to the problem, there remain major unsettled problems,' but he ended on a more optimistic note:

> There are strong indications, if not quite a proof that superconductivity is essentially an extreme case of diamagnetism rather than a limit of infinite conductivity. The isotope effect indicates that the superconductivity phase arises from interactions between electrons and lattice vibrations. (Bardeen, 1956, p. 274)

The breakthrough was indeed lurking around the corner; it would involve a new type of quasi-particle, the 'Cooper-pair,' and the solution of the problem by Bardeen, in cooperation with Leon N. Cooper and John Robert Schrieffer, would open a new era in the theory of low-temperature phenomena, as we will discuss in the following subsection.

(b) The Solution of the Riddle of Superconductivity (1950–1959)

In a review article on 'Recent Developments in Superconductivity,' John Bardeen and John Robert Schrieffer listed 'the most important milestones on the way to our understanding of superconductivity' after World War II, namely:

> [1] The phenomenological extension of the Landau theory by Ginzburg and Landau (1950) for application to the calculation of boundary energies between normal and superconducting phases and other problems.
> [2] The isotope effect, $T_c \propto M^{-1/2}$, discovered independently by Maxwell (1950) and Reynolds *et al.* (1950), which strongly indicates that superconductivity arises from interactions between electrons and lattice vibrations, or phonons.
> [3] Fröhlich's independent development (1950a, b) of a theory based on electron-phonon interaction, which yielded the isotope effect, but failed to predict other superconductivity properties. A somewhat similar approach of one of the authors (Bardeen, 1950) also ran into difficulties.
> [4] Pippard's introduction (1953) of a coherence distance and a nonlocal modification of the London equations to account for several experiments on penetration phenomena. One of the authors (Bardeen, 1955) showed that the Pippard nonlocal relation would most likely follow from an energy gap model.
> [5] Experimental evidence from several sources (1953-present) of an energy gap for excitations of electrons from the superconducting ground state. . . .
> [6] Investigations by Matthias (1957) of the occurrence of superconductivity in a large number of alloys, compounds and solid solutions, and the development of empirical rules for the occurrence of superconductivity based on such factors as atomic volume, mass and valence number of electrons per atom.
> [7] Cooper's proof (1956) that a Fermi sea with net attractive interactions between the particles is unstable against the formation of bound pairs, no matter how weak the interaction.
> (Bardeen and Schrieffer, 1961, p. 176)

While the theory of Lev Landau and Vitaly Ginzburg (1950) marked a certain endpoint of the development of the semi-empirical or phenomenological theory of the London type (see our discussion in Section IV.4), the discovery of the isotope effect opened the door to an atomistic treatment (Maxwell, 1950; Reynolds *et al.*, 1950). Actually, the effect could also be derived from Herbert Fröhlich's recent 'Theory of the Conducting State' (1950a), as the author from Liverpool would point out quickly; in particular, he derived the relation

$$kT_c \sim \frac{1}{\sqrt{M}}, \tag{945}$$

with M the mass of the isotope in question, in perfect agreement with the experimental data (Fröhlich, 1950b, p. 778).

Fröhlich, who spent the spring semester of 1950 at Purdue University, drew attention to the scattering of electrons with lattice vibrations. He opened his first paper by saying that 'Anyone who is familiar with modern field theories will conclude at once that an electron will have self-energy in this vibrational field,' and reminded one of the situation in polar lattices, where the self-energy 'arises from the interaction of the electron with the lattice polarization produced by the electron itself.' He continued:

> In metals it will be necessary to consider all of the electrons together. It will be shown presently that through the influence of the Pauli principle the interaction between the electrons and the vibrational field depends on their distribution in momentum space, and if the interaction is strong enough it will be seen to lead to a new distribution which—subject to later confirmation—will be identified with the superconductivity state. (Fröhlich, 1950a, p. 846)

Fröhlich claimed that from this point of view, it should be considered 'not accidental that the energy of an electron moving with the velocity of sound is of a similar order as the energy per electron involved in the transition between the normal and the superconducting state ... [and] that very good conductors do not become superconductors, for the required relatively strong interaction between electrons and lattice vibrations gives rise to a large normal resistivity' (Fröhlich, *loc. cit.*). Now, the condition of having sufficiently strong interactions led to a relation between the number of free electrons ν per atom and other properties; as a consequence, for small ν without reducing the other parameters, a material could become superconducting. This situation suggested that certain alloys of a monovalent and a transition metal (in which the electron of the former fills the incomplete shell of the latter) should become superconducting. Bob T. Matthias of Bell Labs, who initially investigated the Au–Pd system (recommended by Fröhlich, 1950a, p. 855), found no superconductivity above 1K; however, when studying alloys of the binary cobalt-silicon system (Matthias, 1952), or of the ternary (Co, Rh)Si_2 and Nb(C, N) systems, superconductivity showed up indeed (Matthias, 1953).

While Fröhlich went on to translate his theory of electron–phonon interaction into the language of modern quantum field theory including renormalization (Fröhlich, 1952), Max Robert Schafroth at Pauli's Institute in Zurich showed that it revealed a serious defect: Especially, it would not yield the Meissner effect (Schafroth, 1951). On the other hand, Brian Pippard, at the Mond Laboratory in Cambridge, introduced—based on his own measurements carried out since 1947—a new, important concept into the description of superconducting substances, the 'coherence length' ξ (about 10^{-4} cm) associated with the superconducting state (Pippard, 1953). He demonstrated that ξ followed from a generalized phenomenological theory of the London type (see Section IV.4) when a nonlocal term was used in the definition of the superconducting current. John Bardeen indicated subsequently that Pippard's nonlocality might be connected with an energy gap between the ground state and the excited states (Bardeen, 1955).[1211] He had been stimulated to resume earlier prewar considerations on superconductivity by the discovery of the isotope effect and also examined the electron–phonon interactions (Bardeen, 1950). In 1955, he extended—together with David Pines—the Bohm–Pines collective description of metal electrons (mentioned above) to take into account also the Coulomb interactions between the electrons, and they showed that in this way an attractive potential could be derived (as was necessary for superconductivity), provided the energy exchanged between two electrons near the Fermi surface was not too large (Bardeen and Pines, 1955). These results prepared the next steps for the final solution.

As J. Robert Schrieffer reported later (in his Nobel lecture), 'In 1955, stimulated by writing a review article on the status of the theory of superconductivity, John Bardeen decided to renew the attack on the problem,' and he continued:

> He invited Leon Cooper ... to join in the efforts starting in fall 1955. I had the good fortune to be a graduate student of Bardeen at that time, and ... was delighted to accept the invitation to join them.
>
> We focused on trying to understand how to construct a ground state Ψ_0 formed as a coherent superposition of normal state configurations Φ_n,
>
> $$\Psi_n = \sum_n a_n \Phi_n, \qquad [(946)]$$
>
> such that the energy would be as low as possible. Since the energy is given in terms of the Hamiltonian H by
>
> $$E_0 = (\Psi_0, H\Psi_0) = \sum_{n,n'} a^*_{n'} a_n (\Phi_{n'}, H\Phi_n), \qquad [(947)]$$
>
> we attempted to make E_0 minimum by restricting the coefficients a_n so that only states with negative off-diagonal matrix elements would enter [and] add in phase and E_0 would be low.
>
> By studying the eigenvalues of a class of matrices with off-diagonal elements all of one sign (negative), Cooper discovered that frequently a single eigenvalue split of from the bottom of the spectrum. He worked out the problem of two electrons interacting via an attractive potential V above a quiescent Fermi sea, i.e., the electrons in the sea were not influenced by V and the extra pair was restricted to states with an energy $\hbar\omega_D$, above the Fermi surface. As a consequence of the non-zero density of quasi-particle states $\mathcal{N}(0)$ at the Fermi surface, he found the energy

[1211] Pippard's ξ was indeed related to the energy gap between the superconducting and the normal state as $\frac{p_F}{m}\frac{\hbar}{\xi} \simeq \Delta k T_C$, with p_F the Fermi momentum (see Schrieffer, 1992, p. 100).

eigenvalue spectrum for two electrons having zero momentum had a bound state split off from the continuum of scattering states, the binding energy being

$$E_B \simeq \hbar\omega_D \exp\left(-\frac{2}{\mathscr{N}(0)V}\right) \qquad [(948)]$$

if the matrix elements of the potential are constant, equal to V in the region of interaction. (Schrieffer, 1992, pp. 100–101)

The team, consisting of John Bardeen, Leon N. Cooper, and John Robert Schrieffer at the University of Illinois at Urbana, proceeded more or less according to the plan devised by Bardeen, with Cooper studying the Bohm–Pines interaction and arriving first (in September 1956) at a definite result, namely: the existence of bound-electron pairs, or 'Cooper pairs,' having energies [(948)] below the continuum (Cooper, 1956). The next important progress was made by Schrieffer, who wrote the many-body wave function after simplifying the Hamiltonian to a term denoting the unperturbed energy of the quasi-particle pairs plus a scattering term of the pair—from the momentum combination ($\mathbf{k}, -\mathbf{k}$) to ($\mathbf{k}', -\mathbf{k}'$)—via the potential (acting on the creation and annihilation operators obeying Fermi statistics). He started from the *Ansatz* for the ground state Ψ_0,

$$\Psi_0 = \prod_k (u_k + v_k b_k)|0>, \qquad (949)$$

with the trial function v_k (and $u_k = \sqrt{1 - v_k^2}$), and found by varying v_k in the weak-coupling limit the minimum condensation energy of absolute zero of temperature to assume the value,

$$W = -\frac{1}{2}\mathscr{N}(0)\Delta^2, \quad \text{with } \Delta = 2\hbar\omega_D \exp\left(-\frac{1}{\mathscr{N}(0)V}\right). \qquad (950)$$

In Eq. (950), $\mathscr{N}(0)$ denoted the density of states at the Fermi surface.[1212] 'The idea occurred to me at the end of January 1957, and I returned to Urbana a few days later where Bardeen quickly recognized what he finally believed to be the essential validity of the scheme much to my pleasure and amazement,' Schrieffer recalled (Schrieffer, 1992, p. 104).

Already on 18 February 1957, the *Physical Review* registered the reception of a letter signed by Bardeen, Cooper, and Schrieffer, containing the essence of a 'Microscopic Theory of Superconductivity' (1957a).[1213] 'The present theory is

[1212] We have corrected certain misprints in Schrieffer, 1992, p. 103.

[1213] As Bardeen wrote to Samuel Goudsmit, then the Editor of the *Physical Review*, on 15 February: 'We feel that this work represents a major breakthrough in the theory of superconductivity.' (Bardeen, in Hoddeson *et al.*, 1992, p. 556) based on the fact that the phonon interaction is negative for $E_{\mathbf{k}} - \mathrm{E}_{\mathbf{k}'} < \hbar\omega$ [with $E_{\mathbf{k}} - \mathrm{E}_{\mathbf{k}'}$ the initial and final energies of the electrons and $\hbar\omega$ the average energy of the photon],' the authors argued, adding: 'We believe that the criterion for superconductivity is essentially that this negative interaction dominate over the (positive) matrix element of the Coulomb interaction.' (Bardeen, Cooper, and Schrieffer, *loc. cit.*, p. 162). They then sketched the construction of their Hamiltonian and the calculation (in the weak-coupling limit) of the energy gap of a superconductor. In the case of tin, they obtained

$$E_G = 4\hbar\omega \exp\left[-\frac{1}{\mathcal{N}(0)V}\right] = k \times 13.8\ \mathrm{K}, \tag{951}$$

by using an estimate for $\mathcal{N}(0)$ from the electronic specific heat data. 'This is to be compared with the experimental value of $k \times 11.2$ K,' they finally concluded, praising the advantages of their new theory, especially its simplicity 'to make calculations of thermal, transport and electromagnetic properties of the superconducting state' (Bardeen *et al.*, *loc. cit.*, p. 164). What they promised in their February note, the three authors fulfilled in a detailed paper submitted in July 1957 (and covering 30 pages of *Physical Review*), entitled simply 'Theory of Superconductivity' (Bardeen, Cooper, and Schrieffer, 1957b).

Bardeen *et al.* opened their paper by noting: 'The main facts which a theory of superconductivity must explain are (1) the second-order phase transition at the critical temperature, T_c, (2) an electronic specific heat varying as $\exp(-T_c/T)$ near $T = 0$ K and other evidence for an energy gap for individual particle-like excitations, (3) the Meißner-Ochsenfeld effect ([magnetic field] $\mathbf{B} = 0$), (4) effects associated with infinite conductivity ([electric field)] $\mathbf{E} = 0$), and (5) the dependence of T_c on isotopic mass, $T_c\sqrt{M} = \mathrm{const.}$' They continued proudly: 'We present here a theory which accounts for all these, and in addition gives quantitative agreement for specific heats and penetration depths and their variation with temperature when evaluated from experimentally determined parameters of the theory.' (Bardeen *et al.*, *loc. cit.*, p. 1175) Thus, they reproduced (apart from slight corrections in numerical factors) the results of their previous letter (Bardeen *et al.*, 1957a) and derived further results, e.g., for the transition temperature [Bardeen *et al.*, *loc. cit.*, p. 1186, Eq. (3.29)],

$$kT_C = 1.14\hbar\omega \exp\left[-\frac{1}{\mathcal{N}(0)V}\right], \tag{952}$$

or for the critical magnetic field at $T = 0$ (Bardeen *et al.*, *loc. cit.*, p. 1187, Eq. (3.39)),

$$H_0 = 1.75[4\pi\mathcal{N}(0)]^{1/2} k \cdot T_c, \tag{953}$$

which also showed the correct temperature dependence (Bardeen *et al.*, *loc. cit.*, Eq. (3.44)), i.e., for $T/T_C \ll 1$,

$$H_C \cong H_0[1 - 1.07\ (T/T_C)^2]. \tag{954}$$

Moreover, Bardeen, Cooper, and Schrieffer confirmed the electromagnetic properties of superconductors that behave in their theory as in the successful phenomenological descriptions and computed Pippard's coherence length ξ in fair agreement with the data (see Bardeen *et al.*, *loc. cit.* p. 1196, Eq. (5.50)). Bardeen and Schrieffer, in an early review of the situation, wrote: 'At the time the original theory was worked out, Hebel and Slichter made [the] first measurements of nuclear spin relaxation times in superconducting aluminum,' and noted:

> They found, surprisingly, that as the temperature drops below T_c the relaxation rate increases to values more than double that of the normal state, indicating a larger interaction between electrons and nuclear spins in the superconducting state than in the normal state. (Bardeen and Schrieffer, 1961, p. 174)

The result (Hebel and Slichter, 1957, 1959) greatly supported the fundamental basis of the '*BCS* theory.'[1214]

In spite of several controversies carried out between 1958 and 1960—in which Max Schafroth, Gregor Wentzel, Yoichiro Nambu, Philip W. Anderson, and others participated—the *BCS* theory became generally accepted as *the* solution of the problem of superconductivity.[1215] Thus, Bardeen and Schrieffer summarized:

> An intensive study of the mathematical structure of the theory has been carried out by a number of authors. [Nikolai] Bogoliubov and [J. G.] Valatin have advanced alternative formulations which are often more convenient for calculation purposes and lead to results which in general are in agreement with the original treatment. Largely through the work of Anderson, Bogoliubov and coworkers, Nambu, [G.] Rickayzen, and Pines and Schrieffer, the role of collective excitations of the electrons has been clarified and questions regarding the gauge invariance of the calculation of the Meissner effect based on the original form of the theory have been resolved. (Bardeen and Schrieffer, 1961, p. 171)

In fact, Anderson and Nambu first belonged among the severe critics of the *BCS* theory, while Bogoliubov and collaborators worked out their own scheme, as soon as the first note of Bardeen, Cooper, and Schrieffer (1957a) had arrived in the Soviet Union (Bogoliubov, 1958; Bogoliubov, Tolmachov, and Shirkov, 1958). The basic ideas of the 1957 *BCS* revolution remained untouched by all of these

[1214] See the reference to 'discussions with C. P. Slichter and L. C. Hebel' (Bardeen, Cooper, and Schrieffer, 1957b, p. 1198). The *BCS* theory predicted the nuclear spin relaxation to rise just above T_c, before falling at lower temperatures.

[1215] For the response of the scientific community to the *BCS* theory, we refer to Hoddeson *et al.*, 1992, especially, pp. 558–564.

and further developments, and on 10 December 1972, the inventors of the theory of superconductivity were awarded the Nobel Prize for Physics.

Still, the original *BCS* theory described only one part of the existing superconductivity phenomena. Several years before, the Russian experimentalist N. V. Zavaritskii had investigated the superconducting properties of thallium on the basis of the Ginzburg–Landau phenomenological theory; when altering the technique of preparing samples, such that amorphous structures resulted, he discovered a relationship between the critical magnetic field and the thickness of the probes which differed from the theoretical predictions (Zavaritskii, 1952). Alexei Abrikosov, a young theoretician associated with Lev Landau, had then proposed to introduce a new class, i.e., 'superconductors of the second group'—called 'Type II Superconductors,' characterized—in contrast to the usual, or 'Type I Superconductors'—by a negative surface energy and a different behaviour in magnetic fields (Abrikosov, 1952). During the following years, Abrikosov (often criticized by Landau and his colleagues) worked on establishing a description of the new class of superconductors, in which Landau's equations were not strictly valid. In November 1956, he finally submitted a detailed paper containing a generalized revision of the phenomenological theory that accounted in particular for 'the magnetic properties of bulk superconductors for which the parameter κ of the Ginzburg-Landau theory is greater than $1/\sqrt{2}$.' (Abrikosov, 1957, p. 1174) This dimensionless factor κ had appeared in equations such as

$$-\text{curl curl } \mathbf{A} = |\Psi|^2\mathbf{A} + \frac{i}{2\kappa}(\Psi^*\nabla\Psi - \Psi\nabla\Psi^*), \tag{955}$$

and assumed for pure metals values of about 10^{-1} (Landau and Ginzburg, 1950, p. 553 of the English reprint). However, for the Zavaritskii probes, κ greater than $1/\sqrt{2}$ had to be introduced into the Ginzburg–Landau equations, which led to the following consequences:

> [i] The superconductivity is maintained at fields greater than H_{c_m} at which equilibrium could exist between the normal and superconducting states.
> [ii] At fields higher than H_{c_m}, a state with $\Psi = 0$ is unstable and superconducting sections with $\Psi \neq 0$ may arise.
> [iii] This instability continues to some value H_c, which for a bulk superconductor is $\kappa\sqrt{2}H_{c_m}$. At this value of the field the superconductor undergoes a transition to the normal state by means of a second-order phase transition. (Abrikosov, 1957 p. 1175)

From his detailed study, Abrikosov derived, in particular, the fact that the magnetic field would penetrate in the 'mixed state'—i.e., the state of the Type II Superconductor existing around H_{c_2}—in the form of magnetic vortices, analogous to the hydrodynamic vortices which Feynman had assumed to exist in the case of helium II at temperatures a little above the λ-point. Thus, regions would occur with a core—corresponding to a region of normal conductivity—around which currents can flow that screen out the magnetic field from the rest of the metal (constituting the still superconducting regions).

The Zavaritskii-Abrikosov theory of the second group of superconductors—whose properties were, apparently, independently verified by B. B. Goodman in Grenoble (1961; see also, 1964)—later developed in two directions. On the one hand, Lev Gorkov of Landau's institute extended the *BCS* scheme to obtain a microscopic derivation of the Ginzburg-Landau equations, hence formulated a microscopic theory also of Type II superconductors (Gorkov, 1959).[1216] On the other hand, the vortex picture of Abrikosov was reinterpreted in terms of magnetic flux quanta (see also Subsection 2.4 below),

$$\Phi_0 = hc/2e = 2.07 \times 10^{-7} \text{ gauss} \times \text{cm}^2, \tag{956}$$

having a typical diameter of 10^{-5} cm.[1217] As the applied magnetic field strength increases from H_{c_1} to H_{c_2}, the number of vortices grows linearly until at H_{c_2} the volume is completely filled with vortices and the probe becomes normal conducting; just in small surface regions, superconductivity may continue up to a field strength of $H_{c_3} \approx 1.5 H_{c_2}$. While Type I superconductors retain their property only up to magnetic fields of the order of 0.1 tesla (or 10,000 gauss), the field strengths H_{c_2} for Type II may reach of the order of 10 tesla of even higher. Hence these superconductors have become of considerable practical use in the creation of large magnetic fields (Bean and Schmitt, 1963).

(c) Critical Phenomena and the Renormalization Group (1966–1974)

An extensive review of 'Static Phenomena Near Critical Points,' published in April 1967, started with the following statements:

> In recent years considerable attention has been drawn to the phenomena which may occur near critical points. Several recent conferences have presented a wealth of new experimental data and theoretical ideas in this area. These conferences have broadcast the fact that there are quite marked similarities between apparently very different phase transitions. An antiferromagnetic near its Néel point behaves quite similarly to a liquid near its critical point. The superconducting transition is not very different from several ferroelectric transitions. In all cases, there is an apparently rather simple behavior in the region right around the critical point. (Kadanoff *et al.*, 1967, p. 395)

In greater detail, the behaviour of condensed matter in the vicinity of the transition points could be expressed in terms of 'order parameters'—e.g., in case of liquid–

[1216] Especially, Gorkov derived detailed expressions for the characteristic fields of these superconductors as well as a microscopic expression for the dimensionless parameter κ (see Gorkov, 1959, p. 1366, Eq. (20)).

[1217] Such a reinterpretation of Abrikosov's vortex lines was discussed, e.g., by J. Friedel, P. G. De Gennes, and J. Matricon (1963). See also Onsager (1961).

gas transition, the energy difference $(\rho - \rho_C)$; in case of the ferromagnetism, the magnetization M; and in case of superconductors, the energy-gap parameter Δ—and critical exponents α, α', β, etc., occurring in the expressions for the for the order parameter $\langle p \rangle$ as a function of temperature, say,

$$\langle p \rangle \sim \pm[(T - T_c)/T_c]^{\beta}, \tag{957}$$

with T_c giving the critical temperature. The value of the critical exponent depended on the theory chosen; e.g., $\beta = \frac{1}{2}$ for the Landau theory of ferromagnetism, and $\frac{1}{8}$ and 0.313 for the two- and three-dimensional Ising model of the same phenomenon. These descriptions of natural transition properties must possess a common reason, as Kadanoff and his collaborators pointed out: 'The basic theoretical ideas are introduced via the molecular field approach, which ... suggests that there are close relations among the different phase transition problems.' (Kadanoff *et al.*, *loc. cit.*) However, they immediately continued:

> Although this theory [i.e., the molecular field theory] is qualitatively correct it is quantitatively wrong, [since] it predicts the wrong values for the critical indices. Another theoretical approach, the "scaling law" concept [of Kadanoff, 1966] which predicts relations among these indices ... provides a promising approach to understanding phenomena near the critical point. (Kadanoff *et al.*, *loc. cit.*)

In the paper by Kadanoff referred to in the foregoing, he had considered an Ising model which he divided 'into cells which are microscopically large but much smaller than the coherence length' and then used 'the magnetization within each cell as a collective variable,' with the intention of analyzing the model 'in a manner which is designed to throw light upon the correlations between the order parameter in different regions of the lattice scale when the parameters describing the magnetization from the critical point—in this case $T - T_c$ and the applied magnetic field—are changed' (Kadanoff, 1966, p. 263). He had succeeded in this way to establish relations between different critical exponents for different order parameters.

Kadanoff *et al*, the authors of the review article, admitted that experiments had not yet confirmed the relations obtained from the 'scaling approach.' Therefore, a number of investigations of various explicit cases were undertaken in the following years, on the one hand, while on the other, numerous new relations were suggested on the basis of slightly different approaches. Finally, Kenneth G. Wilson of Cornell University produced, in the spring of 1971, a more general concept which he summarized as follows:

> The Kadanoff theory of scaling near the critical point for an Ising model is cast in a different form. The resulting differential equations are an example of the differential equations of the renormalization group. It is shown that the Widom-Kadanoff scaling laws arise naturally from these different equations if the coefficients in the equations are analytic. (Wilson, 1971, p. 3174)

Without going into details, we shall sketch in the following certain features of Wilson's approach, which makes use of the renormalization technique developed in quantum electrodynamics to derive consequences both in classical and quantum-theoretical atomistic theories of matter transitions.

In all problems of phase transitions, very many molecules are always involved. Now, for instance, in a normal fluid, far from the transition point to the gaseous state, fluctuations occur on the atomic scale. When the value of the external parameter, temperature, is increased toward the boiling point, the fluctuations grow to a scale of 10^3 to 10^4 Å, causing critical opalescence, and very close to the critical point, the correlation length ξ becomes infinitely large. By applying the renormalization-group technique, Kenneth Wilson managed to reduce the number of degrees of the system. In particular, he used a transformation τ of the original Hamiltonian H_0, which preserved the dimensionality and symmetry of the system described but changed ξ to ζ/b with $b > 1$ and the number of degrees of freedom N to N/b^d (with d as the dimension of the system) such that the quantum-mechanical partition function remained unaltered. By iterating the transformation τ, i.e.,

$$\tau(H_0) = H_1, \quad \tau(H_1) = H_2, \ldots, \tag{958}$$

a 'fixed point' H^* may finally be reached such that

$$\tau(H^*) = H^*, \tag{959}$$

with a large class of Hamiltonians H converging to the same H^*.[1218] The critical behaviour of H would then be determined by the behaviour of characteristic quantities near the fixed point H^*, and if the first term of an expansion was written for a function f describing a property of the system, a linear operator emerged, whose eigenvalues were related to the critical exponents. The renormalization-group procedure also satisfied the scaling equations assumed by Kadanoff.

As a special trick to evaluate the critical exponents in four-dimensional systems, Wilson considered the systems rather with the broken dimension 3.99; by using expansions in powers of ε (with $d = 4 - \varepsilon$), he simplified the analysis of the fluctuations (Wilson and Fisher, 1972) and obtained a number of results for ferromagnetic systems in agreement with data (Wilson, 1974). The evident success of Wilson's methods in the difficult problem of describing phase transitions earned him the 1982 Nobel Prize in Physics, which was justified as follows in the citation:

> You are the first theoretical physicist to develop a general and tractable method, where widely different scales of length appear simultaneously. Your theory has given a complete solution of the classical problem of critical phenomena at phase tran-

[1218] For details, see Wilson and Kogut, 1974. The transformation τ had been used in the renormalization-group approach of Stueckelberg and Petermann to quantum electrodynamics (1953); the idea of the 'fixed point' had occurred in a paper of Murray Gell-Mann—who had been Kenneth Wilson's thesis supervisor—and Francis Low (1954).

sitions. Your new ideas and methods seem also to have a great potential to attack other important and up to now unsolved problems in physics. (Stig Lundquist, in *Nobel Lectures*: *Physics 1982–1990*, *Nobel Foundation*, ed., 1993, p. 96)

2.4 New Quantum Effects in Condensed Matter Physics (1958–1986)

After the solution of the problem of superconductivity had been attained, a series of new quantum effects turned up in condensed matter physics, most of them in the low-temperature region; although the existence of some of them had been predicted theoretically, others challenged the imagination of experts for decades. One such predicted effect, that of Yakir Aharanov and David Bohm, will be postponed to the next section, as it was quite connected with the problem of the interpretation of quantum mechanics.

(a) The Mössbauer Effect (1958)

In 1955, Rudolf Mössbauer, a graduate student of Hans Maier-Leibnitz at the *Technische Hochschule* of Munich (under whom he took his *Diplom* in 1955), went on as a research assistant at the Physics Institute of the *Max Planck-Institut für medizinische Forschung* in Heidelberg. Mössbauer's doctoral studies, although principally carried out in Heidelberg, were still guided by Maier-Leibnitz. His investigations concerned the emission and absorption of γ-rays by atomic nuclei. He worked on the problem of resonance fluorescence in nuclei, i.e., the excitation of a nuclear level by incident γ-rays of the same nucleus. Normally, the shift of the γ-lines due to recoil should be large in comparison with the optical analogue, and the resonance condition must be violated unless the thermal motion restored it; therefore, one expected to—and also did—observe increased nuclear resonance effects at higher temperatures. When he investigated the absorption of the 129-keV radiation of Ir^{191} (obtained from the decaying Os^{191}) as a function of temperature in the region from 90 to 370K, however, Mössbauer noticed a strong increase of the cross section, contrary to expectation (Mössbauer, 1958a). He obtained his doctorate in 1958, and explained the results in the publication of his doctoral thesis as indicating a considerable influence of chemical binding on the nuclei, namely:

A free nucleus of mass m takes over, when emitting an energy quantum E_0, a recoil energy R given by

$$R = \frac{E_0^2}{2mc^2}. \qquad [(960)]$$

In the case of chemical binding of the nucleus in a crystal, the crystal has to take over the recoil energy as an internal energy. Because of the quantization of the internal

> energy, however, only discrete energies can be taken over, and the recoil energy depends on the probability for exciting the lattice vibrations of the crystal. For temperatures that are large in comparison with the Debye temperature θ of the crystal, the statistical velocity distribution will be independent of the binding, and an unimpeded transmission of the full recoil energy, due to Eq. [(960)], follows. With the decreasing temperature, especially an increasing number of the high-frequency oscillators of the crystals assume their ground state. These oscillators then cannot transfer anymore energy, hence the line-shape would become asymmetric if the recoil energy is not large compared to the upper limiting energy $\hbar\omega_g$ of the vibrational spectrum of the crystal. (Mössbauer, 1958a, pp. 126–127)

By extending a theory, developed earlier by Willis E. Lamb, Jr., with respect to the resonance absorption of slow neutrons in crystals (Lamb, 1939), to his nuclear γ-resonance absorption, Mössbauer indeed succeeded in explaining the observed data. In accordance with this theory, he concluded that 'for emission and absorption spectra extraordinarily strong lines having the [extremely small] natural line-width will show up in the background of a broad distribution due to the thermal motions of the atoms in the crystal,' and he confirmed this by detecting the unshifted 129-keV resonance line of Ir^{191} by a 'centrifuge method' (Mössbauer, 1958b).

At first, Mössbauer's results, published in 1958—as reported above—were ignored, and then doubted. Within a year, however, recognizing the potential importance of the Mössbauer effect, other physicists repeated his experiments and confirmed the result. The fact that recoil-less nuclear absorption makes it possible to measure extraordinarily small energy differences between two systems (just large enough to hinder resonance fluorescence) provided the method for a broad range of important applications. With their remarkably constant wavelengths and frequencies, fluorescence γ-rays are used as extremely precise measuring tools for gauging the effect of such natural forces as gravity, electricity, and magnetism on infinitesimal particles.

One of the first spectacular applications of the Mössbauer effect took place in 1959, when R. V. Pound and G. A. Rebka, Jr., at Harvard used it to confirm Albert Einstein's prediction that a gravitational field would change the frequency of electromagnetic radiation (Pound and Rebka, 1960). This support for Einstein's general theory of relativity took the form of a measurable change in the frequency of γ-ray photons produced by the difference in gravity between the top and the bottom of a 70-ft tower. Use of the Mössbauer effect also provided information about the magnetic and electrical properties of nuclei and about the electrons that surround them (nuclear Zeeman effect). Applications of the Mössbauer effect were soon made in fields as diverse as archaeology, chemical catalysis, molecular structure, valency, solid-state and atomic physics, and biological polymers (see, e.g., Boyle and Hall, 1962, pp. 521–522).

Rudolf Mössbauer was to have been appointed a full professor at the Munich Technical University, but frustrated by what he regarded as the bureaucratic and authoritarian organization of German universities, he refused the promotion and took a leave of absence from Heidelberg in 1960 to become a research fellow at the California Institute of Technology, where he was appointed a professor the following year. Also, in 1961, Mössbauer shared the Nobel Prize in Physics (with Robert Hofstadter of Stanford University); he was awarded the prize 'for his researches concerning the resonance absorption of gamma radiation and his discovery in this connection of the effect which bears his name.' By Mössbauer's discovery, said Ivan Waller of the Royal Swedish Academy of Sciences in his presentation speech, 'it has become possible to examine precisely numerous important phenomena formerly beyond or at the limit of attainable accuracy of measurement.' (Waller, in his presentation speech in Stockholm, on 10 December 1961, in *Nobel Foundation*, ed., 1964, p. 559)

In 1964, however, Mössbauer returned to Germany as professor of physics at the Munich Technical University in a department modelled on those found in American universities. Some scientists facetiously referred to this change in German academic organization as 'the second Mössbauer effect.' From 1972 to 1977, Mössbauer directed the Laue–Langevin Institute in Grenoble, France, and then returned to his professorship at Munich.

(b) Experimental Proof of Magnetic Flux Quantization (1961)

Following from the equations of the phenomenological theory of superconductivity (F. and H. London, 1935; see Section IV.4), the magnetic flux in hollow cylinders of superconducting material must be constant if the thickness of the walls is large compared to the penetration depth of the magnetic field—and not too high external magnetic field strengths are applied. In his book on *Superfluids*, Fritz London derived from wave-mechanical considerations that the magnetic flux 'frozen' in a tube—it could also be a ring of superconducting material—should be quantized according to the equation,

$$\Phi_0 = \frac{hc}{e} = 4.12 \times 10^{-7}\ \text{gauss} \cdot \text{cm}^2. \tag{961}$$

(See London, 1950, p. 152.) More than a decade later, two teams—Bascom S. Deaver, Jr., and William M. Fairbank at Stanford University and R. Doll and M. Näbauer of the Bavarian Academy Laboratory in Hersching near Munich—independently came out with an experimental proof of Eq. (961), which they published simultaneously in the same 15 July 1961, issue of *Physical Review Letters*. While Deaver and Fairbank (1961) used two hollow tin cylinders, one of 0.8 cm length and 2.33×10^{-3}-cm outside and 1.33×10^{-3}-cm inside diameter and the other of 0.9 cm length and having 1.64×10^{-3}-cm and 1.35×10^{-3}-cm outside and inside diameters, respectively, fabricated by electroplating tin on a copper wire, their German counterparts evacuated lead on a quartz fibre of 10-μm diameter (Doll and Näbauer, 1961) to obtain their small-sized test cylinders of superconducting materials. Both teams then found a discrete value for the observed flux—however, by a factor of 2 smaller than the one predicted by London, or

$$\Phi_0 = \frac{hc}{2e} \pm 20\% \quad \text{(Deaver and Fairbank)}, \tag{962a}$$

$$\Phi_0 = 0.4\frac{hc}{e} \quad \text{(Doll and Näbauer)}. \tag{962b}$$

Moreover, in both experiments, the data—showing the magnetic flux versus the magnetic field strength in the direction of the cylinder's axis—indicated additional steps in the flux values at certain higher field strengths; thus, Deaver and Fairbank noted: 'The ratio of the fields at which the steps occur, are approximately 1, 3, 5 and 7' (Deaver and Fairbank, 1961, p. 45), and similarly Doll and Näbauer displayed a second step also at a field strength three times that of the first step (Doll and Näbauer, 1961, p. 51).

In the lively discussion following the publication of the above-mentioned experimentalists, the theoreticians Nina Byers and Chen Ning Yang (1961), Lars Onsager (1961), John Bardeen (1961), and J. B. Keller and Bruno Zumino (1961) participated. The first group argued that London's derivation of Eq. (961) was not valid, because the Meissner effect rendered this solution unstable; rather, the Cooper-pair correlation in superconductivity spoke in favour of Eqs. (962). Also, Onsager, Bardeen, Keller, and Zumino derived the same value for the flux quanta, again motivated by the electron-pair effect; in particular, they drew attention to the fact that Lev Gorkov's microscopic reformulation of the Ginzburg–London theory of superconductivity demanded the use of a charge $e^* = 2e$ in the supercurrent (see Gorkov, 1959, p. 1366, Eq. (16)).

(c) The Josephson Effect (1962)

Brian Josephson, a young graduate student at the Cavendish Laboratory in Cambridge, England, submitted in early June 1962 a note to *Physics Letters*, the contents of which he characterized as follows: 'We here present an approach to the calculation of tunnelling currents between two metals that is sufficiently general to deal with the case when both metals are superconducting. In that case new effects are predicted, due to the possibility that electron pairs may tunnel through the barriers leaving the quasi-particle distribution unchanged.' (Josephson, 1962, p. 251) The crucial property of the wave function describing superconducting electron pairs, which influenced the predicted tunnelling current, Josephson recognized to be the phase. By applying the proper field-theoretical formalism (due to Bogoliubov), he obtained an equation for the current operator describing the transfer of electron-pairs across the barrier; this equation involved—besides the constant term (J_0) two terms (J_1 and J_2^*) oscillating with the frequency $\nu = 2\text{eV}/h$ (with V the applied voltage). Hence, the equation predicted, in particular:

(i) At finite voltage the usual DC current occurs but there is also an AC supercurrent of amplitude $|j_1|$ and frequency $2eV/h$ (1 μV corresponding to 483.6 Mc/s).
(ii) At zero voltages, j_0 is zero but a DC supercurrent up to a maximum of $|j_1|$ can occur. (Josephson, *loc. cit.*, p. 252)

Moreover, Josephson noticed that the case of an applied radio-frequency current might be treated as well—then the oscillations of V just would modulate the supercurrent—and the quantum-mechanical interpretation of the above-mentioned processes was as follows. Item (i) corresponded to the transfer of electron-pairs across the barrier associated with photon emission, and item (ii) corresponded to the same transfer, but without photon emission. Further, the effects must be largest at low temperatures, when all contributions were in phase; hence, $|j_1|$ would become 'equal to the current flowing in the normal state at an applied voltage equal to 2π times the energy gap, assumed to be the same on both sides' (Josephson, *loc. cit.*). Higher temperatures, however, would reduce the effects derived. Finally, Josephson argued that magnetic fields destroyed the phase and thus decreased the value of $|j_1|$, which might be recognized from the equation for the tunnelling-current density; i.e.,

$$j = j_0 + \frac{1}{2} j_1 \psi_l^* \psi_r + j_1^* \psi_r^* \psi_l, \tag{963}$$

where ψ_l and ψ_r denoted the effective superconducting wave functions in the film on both sides of the barrier. Indeed, Eq. (963) predicted that:

> In very weak fields diamagnetic currents will screen the field from the space between the films, but with a large penetration depth owing to the smallness of j_1. In larger fields, owing to the existence of a critical current density, screening will not occur; the phases of the supercurrents will vary rapidly over the barrier, causing the maximum total supercurrent to drop off rapidly with increasing field. (Josephson, *loc. cit.*, p. 262)

Josephson concluded that effects similar to those mentioned already should occur when two superconducting regions are separated by a normal-conducting region. Although he did not succeed in verifying the predicted effects himself experimentally, they would soon be detected by other people at other places (Anderson and Rowell, 1963; Shapiro, 1963). For his discovery of the tunnelling effect in superconductors, Josephson was awarded the Nobel Prize in Physics in 1973.

(d) Superfluid Helium III: Prediction and Verification (1961–1972)

The lightest noble gas, helium, exists in nature in two stable isotopes: He^4 and He^3; the latter was detected only in 1939, since it occurs suppressed by a factor of 10^{-6} as compared to the abundant He^4-isotope (hence, it is obtained usually from artificial nuclear transformations). Because of the low masses (two protons and two neutrons, and two protons and one neutron, respectively) and the weak forces acting between the atoms, helium under normal pressure remains a fluid down to the lowest temperatures: 3.2 K for He^3 and 4.2 K for He^4. Now, He^4 was found to be a superfluid (see Section IV.4), which could be explained theoretically as being due to Bose–Einstein condensation; but He^3 as a microscopic particle would obey Fermi statistics and should not therefore exhibit this property. Lev Landau's theory of Fermi liquids (1957) did not change this outlook, as the liquid behaved according to it much like a free gas; except that the phenomenon of 'zero sound'—a peculiar collective excitation occurring in the strongly degenerate region of lowest temperatures—had to be considered. The atoms of liquid helium, therefore, should behave like electrons in a metal; but the latter could create a superconducting state by forming—according to the *BCS* theory—'Cooper pairs'; hence, the theoreticians speculated since the early 1960s whether a similar behaviour might be found in He^3. In a paper, entitled 'Generalized Bardeen-Cooper-Schrieffer States and the Proposed Low Temperature Phase of Liquid He^3' and submitted in May 1961, Philip W. Anderson and P. Morel investigated this possibility first; they summarized the results as:

> Particle interaction in a Fermi gas may be such as to attract pairs near the Fermi surface more strongly in $l = 1, 2, 3$ or higher states than in the simple spherically symmetrical state [as in Cooper-pairs]. In that case the Bardeen-Cooper-Schrieffer condensed state must be generalized, and the resulting state is an anisotropic superfluid. We have studied the properties of this type of state in considerable detail, especially for $l = 1$ and 2.... The ground state for $l = 2$ is different from those previously considered, and has cubic symmetry and no net angular momentum.... We apply our results to liquid He^3; after correction for scattering ... it is found that the predicted transition should take place below 0.02 K. (Anderson and Morel, 1961, p. 1911)

That is, while Anderson and Morel realized that the strong repulsion between the 'hard cores' of the helium atoms (obeying the Pauli principle) made the formation of a 'superconducting pairing' of particles in an *s*-wave, spin-singlet state impossible, there might still arise for non-zero orbital angular momentum l (which would reduce the effect of the core-repulsion) a stable groundstate at very low temperatures, definitely below the continuum He^3–He^3 states.[1219]

Thus, a general *BCS* theory, admitting different types of particle-pairs, appeared to describe a condensation phenomenon also in systems of neutral He^3-atoms, which cannot exhibit superconductivity but should rather become a superfluid; just the onset of the phenomenon was uncertain. Ten years after the first theoretical prediction, in fall 1971, Douglas D. Osheroff—a graduate student

[1219] A similar result was obtained by R. Balian and N. R. Wertheimer (1963), who studied in particular the case of a relative *p*-wave; they further concluded a sharp diminution of the critical temperature if impurities were present.

at Cornell University—when measuring pressure changes during the cooling of liquid to solid helium, observed anomalies between 3 and 1.5 mK. On 10 February 1972, *Physical Review Letters* received a paper, written by Osheroff and his Ph.D. thesis supervisors David M. Lee and Robert C. Richardson, reporting 'Evidence for a New Phase of Solid He^3'; in particular, they claimed the existence of such a phase in solid He^3 'below 2.7 mK of a fundamentally different nature than the anticipated antiferromagnetically ordered state,' and, 'at lower temperatures, evidence of possibly a further transition' (Osheroff, Richardson, and Lee, 1972, p. 885). The following experimental study of the new magnetic phenomena of He^3 below 3 mK (by nuclear magnetic resonance observation) revealed, however, that the anomalous behaviour had to be associated rather with the fluid state of He^3, which exhibited three new phases called A, B, and B' (or later A_1): The liquid susceptibility dropped to about half its Fermi-degenerate value at B and returned to the original value at B' (Osheroff, Gully, Richardson, and Lee, 1972).

The real experimental situation turned out to be a rather complex one. The theoretical explanations include, besides the previously discussed mechanisms, a new spin-fluctuation effect: The attraction which binds the pairs of He^3 would not arise, as in the case of *BCS* superconductors, but rather from the polarization of the spins of the background He^3-liquid (Anderson and Brinkman, 1973). In any case, the later efforts finally succeeded in disentangling and clarifying all low-temperature phenomena of the He^3-system.[1220]

(e) The Quantum Hall Effect and Lower-Dimensional Quantization (1980)

On 4 February 1980, Klaus von Klitzing at the Grenoble high-magnetic field laboratory of the Stuttgart *Max Planck-Institut für Festkörperforschung* noted in his scientific diary the design of an apparatus plus a set of formulae leading to a definite resistance value of 25812 Ω. Less than three months later, he submitted the results in a three-man note, entitled 'New Methods of High-Accuracy Determination of the Fine-Structure Constant Based on Quantized Hall Resistance,' to *Physical Review Letters* (Klitzing, Dorda, and Pepper, 1980). 'The Discovery of the Quantized Hall Effect (*QHE*) was the result of systematic measurements on silicon field transistors,' he said later (von Klitzing, 1998, p. 316), referring to the earlier pioneering work of A. B. Fowler and collaborators at the IBM Watson Research Laboratory in Yorktown Heights, New York, who had used these devices to observe new quantum phenomena in two-dimensional electron systems (Fowler, Fang, Howard, and Stiles, 1966). Exactly, 'a two-dimensional gas is absolutely necessary for the observation of the *QHE*,' von Klitzing now emphasized (von Klitzing, 1998, p. 316).

As had been known for over a century, the Hall effect consisted in creating an

[1220] For a fairly nontechnical review, see Vollhardt, 1983.

electric voltage U_{H} perpendicular to the current, characterized in classical electron theory by the Hall resistance R_{H},

$$R_{\mathrm{H}} = \frac{1}{ne} B, \tag{964}$$

with n denoting the electron density in the two-dimensional electron gas. By studying the Hall voltage U_{H} of a silicon metal-oxide semiconductor field effect transistor (MOSFET) at the low temperature of 1.5 K in the high magnetic field (18 tesla) of a Bitter coil as a function of the gate voltage V_{G}, von Klitzing registered steps in the U_{H}-function occurring always at the same values of V_{G}; further, the voltage drop between the potential probes (being proportional to the resistivity in the longitudinal x-direction) showed zeros at the plateaus. The steps in the Hall resistance could now be identified with the quantized values,

$$R_{\mathrm{H}} = \frac{h}{e^2}\frac{1}{i}, \quad i = 1, 2, \ldots, \tag{965}$$

which von Klitzing *et al*. explained as being due to the quantization of the electrons in the two-dimensional electron gas of the MOSFET. That is, in a strong magnetic field, the electrons do have restricted orbits whose energies are given by the so-called Landau levels,

$$E_{\mathrm{N}} = \left(N + \frac{1}{2}\right)\hbar\omega, \quad N = 1, 2, \ldots, \tag{966}$$

where $\omega = eB/m^*$ (and m^* indicates the effective mass). If i is such that Landau levels are completely occupied, the electron density becomes

$$n_i = ieB/h, \tag{967}$$

which, together with Eq. (964), yields the experimentally discovered relation (965). For $i = 1$, the value R_{H} became defined in fall 1988 as the international reference standard of electrical resistance and named, in honour of its discoverer, the Klitzing constant,

$$R_{\mathrm{K}} = 25812.807\ \Omega. \tag{968}$$

The later investigations of the quantum Hall effect, especially in heterostructures, such as GaAs, in which the gallium was replaced by other atoms, served to improve the precision of the determination of the quantum Hall resistance values due to Eq. (965). In 1982, a group at Bell Labs detected the ‘anomalous’ quantum Hall effect; i.e., they discovered fractional values of i in Eq. (965), e.g., $\frac{1}{3}$, $\frac{2}{3}$, $\frac{2}{5}$, etc. (Tsui, Störmer, and Gossard, 1982). The present theoretical understanding favours the explanation of the fractional quantum Hall effect by a novel type of condensation phenomena created by the many-electron interaction.

(f) High-Temperature Superconductors (1986)

The phenomenon of superconductivity remained restricted for seven decades to the domain of low temperatures. Thus, the search for higher transition points had stopped at $T_c = 23.3$ K for the Nb_3Ge alloy, and the route to higher temperatures appeared to be quite blocked, even after the quantum-theoretical understanding had been reached by the *BCS* theory. In fall 1983, K. Alex Müller, then heading an IBM Fellows group at Rüschlikon, Switzerland, approached his former doctoral student J. Georg Bednorz and invited him to assist in investigating superconductivity in oxides of the perovskite type, which were candidates for comparatively high transition temperatures, since, for example, $BaPb_{1-x}\,Bi_xO_3$ possessed $T_c = 13$ K.[1221] Bednorz and Müller combined speculations from the *BCS* theory with consequences from the Jahn–Teller effect to support their endeavours. 'It was in late 1985 that the turning point was reached,' recalled Bednorz several years later, especially:

> I became aware of an article by the French scientists C. Michel, L. Er-Rakho and B. Raveau, who had investigated a Ba-La-Cu oxide with perovskite structure exhibiting conductivity in the temperature range between 300 and –100 °C. . . . In the Ba-La-Cu oxide with a perovskite structure containing Cu in two different valencies all our concept requirements [for a search for of high-T_C superconductivity] seemed to be fulfilled. . . .
>
> In mid-January 1986 . . . I decided to restart my activities in measuring the new compound. When performing the four-point resistivity measurement [with the Ba-La-Cu oxide probes] the temperature dependence did not seem to be something special. During cooling, however, a metallic-like decrease was first observed, followed by an increase at low temperatures, indicating a transition to localization. My tension, always increasing as the temperature approached the 30 K range, started to be released when a sudden resistivity drop occurred at 11 K. Was this the first indication of superconductivity?
>
> Alex [Müller] and I were really excited, as repeated experiments showed perfect reproducibility and an error could be excluded. Compositions as well as the thermal treatment were varied and within two weeks we were able to shift the onset of resistivity drop to 35 K. This was an incredibly high value compared to highest T_C in the Nb_3Ge superconductor. (Bednorz and Müller, 1993, pp. 429–433)

Since Bednorz and Müller were not able to demonstrate the presence of the Meissner–Ochsenfeld effect, the infallible proof of superconductivity, they submitted their paper to *Zeitschrift für Physik* in April 1986 with the cautious title 'Possible High T_c Superconductivity in the Ba-La-Cu-O System' (Bednorz and Müller, 1986). However, together with their Japanese guest-collaborator Masaaki Takashige (who joined the IBM Laboratory in February 1986), Bednorz and Müller provided the necessary demonstration of the Meissner–Ochsenfeld effect later in 1986 (Bednorz, Takashige, and Müller, 1987).

[1221] For details of the story of the discovery of high-temperature superconductivity, see Bednorz and Müller, 1993, especially, pp. 424–433.

The results from Switzerland greatly excited the low-temperature community, and soon various groups, especially in the United States and Japan, joined in the chase for higher transition temperatures. [Paul] C. W. Chu and others from the University of Houston, in Houston, Texas, together with colleagues from Alabama, already reached in February 1987 in a mixed phase Y-Ba-Cu-O compound system, $T_c = 93$ K (M. K. Wu *et al.*, 1987); i.e., they pushed into the technically easily obtainable region of liquid nitrogen. The high-temperature superconductors shared a number of properties: In general, they exhibited chemically complicated structures, and in particular, they contained copper oxide in well-separated planes; hence, they were also called 'cuprates' or 'copper-oxide superconductors.' A. J. Leggett, an expert in the field, stated: 'It seems overwhelmingly probable that the mechanism of superconductivity is to be sought in these CuO_2 planes, and that the role of off-plane atoms is mainly to act as donors of electrons (or, more usually, of holes) to the planes and as spacers between them.' (Leggett, 1995, p. 959) He then pointed to the fact that superconductivity occurred in the substances mentioned just in a region of the phase close to an antiferromagnetic phase, and emphasized the highly anomalous properties of the substances in the normal-conducting state. Moreover, the cuprates turned out to be Type II superconductors, having a very large H_{c_2}, and their transition to the superconducting state appeared as less sharp than for conventional superconductors (and dependent on the direction in the crystal). On the other hand, Leggett noticed that 'nearly every feature of the *BCS* model shows up at least qualitatively, for example both the spin susceptibility and the electronic specific heat drop off sharply below T_c and there appears to be a relatively well-defined energy gap' (Leggett, *loc. cit.*, 961). The questions of whether cooper-oxide superconductors should be described as Fermi liquids, or how the Cooper-pair mechanism works, or whether dimensionality plays a role in the theoretical descriptions, have still not been decided.[1222]

The new discoveries in condensed matter physics, to which we may add that of an unexpected, remarkably stable cluster of 60 carbon atoms, the soccer-ball-like 'fullerene' molecule, announced in the issue of *Nature* of 14 November 1985, by Harold W. Kroto and collaborators at Rice University in Houston (1985)—which, besides having a most interesting chemical structure also exhibited physical peculiarities, e.g., it might become superconducting when properly doped[1223]—gave the whole field an enormous push. A large fraction of the Nobel Prizes in Physics went to the discoverers of the effects discussed above: 1961 to Rudolf Mössbauer, 1973 to Brian Josephson (and to Ivar Giaever and Leo Esaki for detecting tunneling effects in semiconductors and superconductors), 1977 to Philip Anderson (and Nevill Mott and John H. Van Vleck) for their work in complex

[1222] For a fairly recent overview of the facts and their theoretical explanation, see Ramakrishnan, 1997.

[1223] In 1996, Robert F. Curl, Harold W. Kroto, and Richard E. Smalley, were awarded the Nobel Prize in Chemistry for their discovery of the fullerenes. For a review of their superconducting properties, we refer, e.g., to Fink and Sohmen, 1992.

solid-state phenomena, 1985 to Klaus von Klitzing, 1987 Georg Bednorz and Alex Müller, 1996 to David Lee, Douglas Osheroff, and Robert Richardson, and 1998 to Robert Laughlin, Horst L. Störmer, and Daniel Tsui—thereby increasing the importance and reputation of the 'solid-state physicists' in the entire scientific community. The effects and phenomena in condensed matter physics also stimulated great interest that reached far beyond science and technology.[1224] They demonstrated to the broader public the presence of Planck's constant also in the macroscopic domain, because h indeed gave rise to directly observable *macroscopic quantum effects*, of which superconductivity represented a principal and—as far as the theoretical explanation is concerned—pioneering example.

2.5 Stellar Evolution, the Neutrino Crisis, and 3 K Radiation (1957–1999)

The following selected topics from astrophysics demonstrate how this field has indeed flourished during the past fifty years as a laboratory to test the consequences from quantum theory; certain results from it even provided new stimulus for extending the existing standard schemes. As we have already mentioned (in Subsection 2.1), in our universe, the quantum and relativistic phenomena form a close symbiosis. Notably, considerations from general relativity theory, which thus far entered only in the discussion of the universe on a large scale, began to determine processes in the domain of nuclear physics. This trend would intensify in the topics to be dealt with below.

Half of the 1978 Nobel Prize in Physics went to Arno E. Penzias and Robert W. Wilson for their discovery of cosmic background radiation (the other half to Peter Kapitza 'for his discoveries in low-temperature physics'). Penzias used his Nobel lecture to talk 'On the Origin of Elements,' and reviewed first how the early ideas of the relativists in the 1920s, such as Alexander Friedmann and Abbé Georges Lemaître, sneaked into the discussions of the astrophysicists. In particular, he said:

> Not widely popular among respectable scientists of the time, this idea of the expanding universe was taken up in the 1940s in part because the theories of the stellar origin of elements had failed in the 1930s.... The title of Chandrasekhar and Henrich's paper "An Attempt to Interpret the Relative Abundances of the Elements and Their Isotopes" reflects the tentative and unsatisfactory nature of the state of understanding at that time. The paper begins, "It is now generally agreed that the chemical elements cannot be synthesized under conditions *now believed* [emphasis added] to exist in stellar interiors." [(Chandrasekhar and Henrich, 1942, p. 288)] As an alternative, the authors suggested that the expansion and cooling of the early universe

[1224] For instance, the Josephson effect served metrology to introduce an absolute quantum-theoretical measure for the electric voltage (i.e., it allowed one to reduce the voltage standard to atomic constants), and the quantum Hall effect, via the Klitzing constant, Eq. (968), provided the definition of the electrical resistance on the same basis (see, e.g., Kose and Melchert, 1991).

> might be a possible site for the processes. In this view, each of the elements had its abundance "frozen out" at an appropriate stage of the expansion of the hot ($\geq 10^9$ K), dense ($\geq 10^6$ gr/cm^3) universe. (Penzias, 1992, p. 449)

We have sketched above (in Subsection 2.1) how George Gamow and his collaborators continued to investigate the origin of chemical elements by assuming their production in the early history of the universe under nonequilibrium conditions, and how Fred Hoyle and associates opposed the 'Big Bang' theory by a 'Steady-State' theory, which claimed that the elements could well have been created at any time in stars under suitable conditions.

The creation of new elements should occur in the interior of any standard star in the course of its history: Such a star is assumed to come into existence by the contraction of a certain gaseous mass consisting essentially of hydrogen; the stellar mass thus heats up to a temperature igniting nuclear reactions, first yielding helium. In the 1950s and early 1960s, the astrophysicists succeeded in actually deriving a generally consistent picture of the stellar evolution and the production of elements in known stars. Later on, also the physics of newly discovered stellar objects like the 'pulsars,' which are rapidly pulsating radio sources, was clarified: They could be explained as rotating neutron stars which had been predicted in the late 1930s (see Section IV.4).

After outlining certain important items concerning stellar structure and evolution, we shall turn to an observed defect of the standard solar model: the fact that the sun emits too few neutrinos, in order to account for the nuclear processes in its interior, which seems to imply certain properties of these particles that deviate from the usual concepts, notably, a finite mass which follows from the Standard Model. On the other hand, a number of different astronomical observations support the existence of large amounts of 'dark matter'—or so far unobserved mass—in the universe that might be due to neutrinos with a mass or some other yet unknown massive objects.

The standard picture of stellar evolution and nuclear processes in stars also suffers from another difficulty. Let us again quote Penzias, who addressed it in his Nobel lecture:

> Ironically it was Fred Hoyle himself who found the gap that could not be filled in the stellar picture, a gap in the best understood process of them all, the formation of helium from hydrogen. Although the burning of hydrogen into helium provides the sun and other stars with their energy and with building blocks for the formation of heavier elements, Hoyle concluded that about ninety percent of the helium found in stars must have been made before the birth of the galaxy. The basis of this conclusion was an energy argument: the total amount of energy released before formation of all the observed helium is some ten times greater than the energy radiated by the galaxies since their formation. Thus, "it is difficult to suppose that all the helium has been produced in ordinary stars" (Hoyle and Tayler, 1964, p. 1108). Instead attention was turned to helium creation in early stages of an expanding universe, reviving work begun by Gamow some sixteen years earlier. (Penzias, 1992, p. 452)

This revival also led to a reconsideration of a consequence of the expanding-universe model of Gamow *et al.*, namely, the prediction of a basically homogeneous background radiation; this was actually discovered immediately in 1965 as the blackbody radiation at 3 degrees Kelvin or '3 K radiation,' fitting perfectly into the presently accepted 'standard model of hot big bang cosmology,' in which the ideas of the Big Bang model have been merged with ideas of the Standard Model of elementary particles (which we discussed in Subsection 1.5)

(a) Stellar Evolution and New Types of Stars (1957–1971)

Martin Schwarzschild, the son of the Berlin astrophysicist Karl Schwarzschild (who had discovered the 'black hole' solution of Einstein's gravitational theory and contributed to quantum theory before his early death in 1916), signed the preface to a book on *Structure and Evolution of Stars* in November 1957. Schwarzschild, who was a Professor of Astronomy at Princeton University, wrote especially in the introductory notes about the recent development and present state of the field:

> A little more than a decade ago research on the stellar interior underwent a profound change. The central cause of this change was the introduction of nuclear physics into astronomy. Nuclear physics has provided the theory of the stellar interior with the last—but not the least—of the fundamental physical processes which determine stellar structure and evolution. Thus a new and far-reaching development in this field of research became available.
>
> Simultaneously with this new theoretical development occurred an equally far-reaching upsurge in the relevant fields of observational astronomy, an upsurge largely due to the introduction of new spectrographic and photoelectric techniques. The combination of these developments suddenly opened up an unprecedentedly wide front of contact between observation and theory in the research field of stellar interior, in striking contrast with the situation twenty-two years ago, where there was only one major point of contact, the mass-luminosity relation. (Schwarzschild, 1958, p. xv)

We have referred to the first application of nuclear theory to astrophysics (in Subsection 2.1 and earlier), in connection with describing the abundance of elements. For dealing with the internal structure and evolution of the stars, Schwarzschild was less concerned about how the distribution of chemical elements observed in stars and the universe came about, but rather about how did the known stars get their energy when evolving during their lifetime. He therefore combined the physical treatments of the processes already contained in the publications of Arthur S. Eddington and others (in the 1920s and 1930s)—radiative and convective energy transport in the interior of the stars, opacity considerations, etc.—and included a discussion of the nuclear energy production along the lines of Burbidge *et al.* (1957) to arrive at an overall satisfactory description of the phenomena in main-sequence stars and white dwarfs. Five years later, Chushiro

Hayashi, Rēun Hōshi, and Daiichiro Sugimoto presented a kind of final report on the 'Evolution of the Stars,' 183 pages long in *Progress of Theoretical Physics*, covering more or less the complete calculation of the life-story of stars (Hayashi, Hōshi, and Sugimoto, 1962). In particular, the authors made use of Hayashi's discovery: Because of convection (in addition to radiation as considered before), the gravitational contraction of the stars would occur in a much shorter time period (10^4 to 10^9 years) than the age of the universe. The type of the star depended essentially on its initial mass and its chemical composition; e.g., if the star possessed below 0.08 solar mass, it could never heat up to burn hydrogen but would cool off toward an invisible black dwarf lying off the main sequence in the Hertzsprung–Russell diagram (see Hayashi and Nakano, 1963). The larger the initial mass, the faster would the evolution proceed from lower luminosity toward the main-sequence region. After spending some time (essentially the longest period in its life) on the main sequence, the star would leave it (especially when the hydrogen core was burnt out); the temperature would rise then, until the burning of helium sets in, and (after the helium was also exhausted) at still higher temperatures, the burning of carbon, etc., would take over. Thus, nuclear theory, together with relativistic, thermodynamical, and gas-dynamical considerations, led to a complete understanding of the formation, the structure, and the evolution of stars, in full agreement with the observations.[1225]

In their 1962 paper, Hayashi and collaborators covered the general theory of stellar evolution, illustrated with several completely evaluated 'life-stories' of stars with specific mass values. Thus, in Chapter 9, they discussed the situation of a 'Final Phase toward White Dwarfs,' which they summarized as:

> Complete exhaustion of nuclear fuels in the central core of massive stars as stated in the preceding chapter will eventually lead to a collapse of the core since its mass exceeds the Chandrasekhar limit for completely degenerate configurations. In the explosion of the stellar envelopes as corresponding to a type II supernova, a considerable fraction of the mass will be ejected into the interstellar space. The remnant of this supernova will contract and cool off releasing its thermal and gravitational energy. Centrifugal force in its outer region will become greater and greater with the contraction as compared with gravitational force, and then the stellar mass will be reduced below the Chandrasekhar limit through continuous ejection of mass and angular momentum. Finally the star will cool down to a white dwarf and then to a black dwarf. (Hayashi, Hōshi, and Sugimoto, 1962, p. 157)

Black dwarfs could not be seen, but another star-like object, predicted by relativistic quantum and nuclear theory, was discovered in the late 1960s.

In the fall 1967, Jocelyn Bell, working as a research student at the Mullard Radio Astronomy Observatory of the Cavendish Laboratory at Cambridge, found with the help of the large 81.5-MHz array of radio telescopes of her thesis advisor

[1225] Detailed calculations of different star types were made in particular by Rudolph Kippenhahn and his collaborators at the *Max Planck-Institut für Physik and Astrophysik* in Munich; for a recent presentation of the results, see Kippenhahn and Weigert, 1990.

Anthony Hewish a strange source, consisting entirely of scintillating radio signals. In November of that year, Hewish, Bell, and collaborators, analyzed this source with a high-speed recorder showing 'that the signals, when present, consisted of a series of pulses, each lasting ~0.3 s and with a repetition period of about 1.337 s which was soon found to be maintained with extreme accuracy' (Hewish *et al.*, 1968, p. 709). During the following months, they detected three other similar sources, having periods between 0.25 and 3 s; hence, the authors suggested that the pulses might be emitted either by neutron stars or white dwarfs (Hewish *et al.*, *loc. cit.*, p. 712). Soon after this first report appeared (in *Nature*, issue of 24 February 1968), Thomas Gold of Cornell University expounded: 'The case that neutron stars are responsible for the recently discovered radio sources appears to be a strong one,' because: 'No other theoretically known astronomical object would possess such short and accurate periodicities as those observed, ranging from 1.33 to 0.25 s.' (Gold, 1968, p. 731) He argued, in particular, that it must be rotating neutron stars, since only those objects provided a possibility to explain the phenomenon, namely: 'Because of the strong magnetic fields and high rotation speeds, relativistic velocities will be set up in any plasma in the surrounding magnetosphere, leading to radiation in the pattern of a rotating beacon.' (Gold, *loc. cit.*)

The 'pulsating radio sources' or 'pulsars' would indeed soon be shown to be connected with the remnants of supernovae explosions.[1226] Later, pulsar theories connected the emission mechanism with a continuous creation of electron–positron pairs, which would not operate when the period exceeds an upper limit (determined by the theory of neutron star matter). However, M. D. Young *et al.* also found a period of 8.51s, exceeding this limit; hence, they concluded that 'either the model assumptions are wrong, or current theories of radio emission must be revised' (Young, Manchester, and Johnston, 1999, p. 848). Like many other problems of astrophysics, that of pulsars leaves some questions to be answered; nevertheless, one expects that the basic explanation of pulsating radio signals as being created by rapidly rotating neutron stars remains correct.

(b) The Solar Neutrino Problem and the Neutrino Mass (1964–1999)

As early as 1946 and 1949, Bruno Pontecorvo and Luis Alvarez pointed out that there existed a 'most promising method for detecting solar neutrinos,' namely, the endothermic reaction

$$\mathrm{Cl}^{37} + \nu^{\mathrm{solar}} \to \mathrm{Al}^{37} + e^{-}, \tag{969}$$

having a threshold energy of 0.81 MeV and giving rise to radioactive argon that should be easily observed (see Bahcall, 1964, p. 300). Still, this suggestion would not be followed, and it took until the mid-1960s when the measurement was

[1226] It should be mentioned here that a theory of "Neutron Star Matter" was worked out by Gordon Baym, Hans Bethe and Christopher J. Pethick (1971).

actually started.[1227] In 1964, the theoretician John N. Bahcall from Caltech, remarked on the origin and importance of solar neutrinos:

> The principal energy source for main-sequence stars like the sun is believed to be fusion, in the deep interior of the star, of four protons to form an alpha particle. The fusion reactions are thought to be initiated by the sequence ${}^{1}\mathrm{H}(pe^{+}\nu)\ {}^{2}\mathrm{H}(p,\gamma)\ {}^{3}\mathrm{He}$, and terminated by the following sequences: (i) ${}^{3}\mathrm{He}\ ({}^{3}\mathrm{He},2p)\ {}^{4}\mathrm{He}$; (ii) ${}^{3}\mathrm{He}\ (\alpha,\gamma)\ {}^{7}\mathrm{Be}(e^{-},\nu)\ {}^{7}\mathrm{Li}(p,\alpha)\ {}^{4}\mathrm{He}$. No *direct* evidence for the existence of nuclear reactions in the interiors of stars has yet been obtained because the mean free path of photons emitted in the center of a star is typically less than 10^{-10} of the radius of the star. Only neutrinos, with their extremely small interaction cross sections, can enable us to *see into the interior of a star* and thus verify directly the hypothesis of nuclear energy generation in stars. (Bahcall, 1964, p. 300)

On taking the reaction, Eq. (969), he estimated 'the total predicted number of absorptions per terrestrial ${}^{37}\mathrm{Cl}$ atom per second ... to be

$$\Sigma\phi_{\nu}(\text{solar})\sigma_{\text{abs}} = (4 \pm 2) \times 10^{-35}\ \text{sec}^{-1},\text{'} \qquad [(970)]$$

(Bahcall, *loc. cit.*, p. 301).

Raymond Davis of the Brookhaven National Laboratory now put up an apparatus consisting 'of two 500-gallon tanks of perchlorethylene, C_2Cl_4, equipped with agitators and an auxiliary system for purging with helium ... located in a limestone mine 2,300 feet below the surface (1800 meters of water-equivalent shielding)' (Davis, 1964, p. 303). He estimated, however, that in order to measure the solar neutrino flux about 100,000 gallons of C_2Cl_4 would have to be used in a mine 4,500 feet deep (Davis, *loc. cit.*, p. 304). Thus, he moved from the first site at Baberton, Ohio (i.e., the limestone mine of the Pittsburgh Plate Glass Company) to the bottom of the Homestake Goldmine in South Dakota; and after running the experiment for twenty years (with first results being reported in 1968), he and his collaborators finally arrived in 1992 at the result (as summarized in a later report):

> The long-term averaged solar neutrino production rate measured with the radiochemical Homestake Chlorine Detector is:
>
> $$2.23 \pm 0.22\ \text{SNU}. \qquad [(971)]$$
>
> (1 SNU = 1 neutrino reaction per second in 10^{36} target atoms). There is a persistent discrepancy, called the "solar neutrino problem" between the result of the prediction from the standard solar model, 8.0 SNU (Bahcall and Pinsonneault, 1992), which is usually attributed to a deficiency of solar ${}^{8}\mathrm{B}$ neutrinos ($E_{\nu} < 15$ MeV). This conclusion has been confirmed by the Kamiokande-II water Čerenkov detector which observes only $(48 \pm 8)\%$ of the ${}^{8}\mathrm{B}$ neutrino flux expected from the Standard Solar Model (Nakamura, 1993). (Hampel, 1994, p. 3)

[1227] Raymond Davis, though he suggested the observation already in 1957, did attack the question seriously only seven years later, cooperating with J. N. Bahcall (see below).

Like the later ones—i.e., the GALLEX experiment using 30.3-t gallium (in the form of 110-t $GaCl_3$–HCl solution) in the Italian Gran Sasso Underground Laboratory (reported in Hampel, 1994) and the Kamiokande-II experiment in Japan (Nakamura, 1993)—the pioneering Homestake experiment of Davis yielded a definite deficit of solar neutrinos which had to be explained.

Hans Bethe addressed the now popular solution in a note, 'Possible Explanation of the Solar Neutrino Puzzle,' published in March 1986 in *Physical Review Letters*, which he introduced by drawing attention to 'a very important paper' of S. P. Mikheyev and A. Yu Smirnov (1986) that 'discussed a mechanism by which a large fraction of the neutrinos ν_e emitted by the sun may be converted into ν_μ when traversing the sun, and thereby be reduced unobservable' (Bethe, 1986, p. 1305). The Russian theorists had, at a previous meeting in Finland, indeed suggested a mechanism for how to mix electron and muon neutrinos, whose eigenstates have to be described (with θ the mixing angle) by

$$\begin{aligned} |\nu_e\rangle &= |\nu_1\rangle \cos\theta + |\nu_2\rangle \sin\theta, \\ |\nu_\mu\rangle &= -|\nu_1\rangle \sin\theta + |\nu_2\rangle \cos\theta, \end{aligned} \tag{972}$$

in terms of fundamental states (analogous to the neutral kaon states) $|\nu_1\rangle$ and $|\nu_2\rangle$ having masses m_1 and m_2, respectively. Consequently, if one followed a ν_e-beam of energy E emerging from a source (reactor or sun) over a distance L, neutrino oscillations would be observed yielding a survival probability

$$P(\nu_e \to \nu_e) = 1 - \sin^2(2\theta)\,\sin^2\left(\frac{1.27\Delta m^2 L}{E}\right), \tag{973}$$

with $\Delta m^2 = m_2^2 - m_1^2$, and L measured in metres and E in MeV. Now, the mixing mechanism of Mikheyev and Smirnov arose from the coupling of ν_e and ν_μ to the charged weak boson $W^\pm$: While neutral weak currents act equally on ν_e and ν_μ, the charged ones will exchange ν_e with the electrons in matter (but not the ν_μ) and produce an extra term in the neutrino Hamiltonian, creating a mass difference

$$\Delta m^2 = 2\sqrt{2}GN_eE_\nu, \tag{974}$$

when ν_e of energy E_ν traversed matter having the electron density N_e. From the preliminary data then available from the Davis experiment (Bahcall *et al.*, 1985), and taking reasonable values for the solar mass (and electron) density, Bethe derived for Δm^2 a value of the order of 10^{-6} $(\mathrm{eV})^2$; that is, a difference of about 1 meV/c^2 between the masses of electron and muon neutrinos would suffice to remove the solar neutrino deficit.

In the past fourteen years, the empirical situation has not changed essentially: The latest data from the presently available experiments (from Homestake and

GALLEX to Super Kamiokande) confirm the neutrino deficit first noticed by Bahcall and Davis on the basis of zero-mass electron neutrinos, as demanded by the Standard Model.[1228] A nonzero neutrino mass and the existence of neutrino oscillations would indeed account for a number of observed effects, especially the neutrino flux ratio ν_μ/ν_e as dependent on the zenith angle (where a deficit of upgoing muons has been detected: Fukuda *et al.*, 1998). And it might help to soften, though not remove, the presently existing 'dark matter problem' in the universe.[1229]

(c) 3 K Radiation and the Early Universe (1965–1990)

In his article in *Nature* on 'The Evolution of the Universe,' George Gamow presented, almost fifty years ago, a visionary picture of the early stages of the Universe, developed from the observational fact of the redshift in the spectra of distant galaxies, on the one hand, and the idea that the relative abundances of various atomic species constitute 'the most ancient archaeological document pertaining to the history of the universe' (Gamow, 1948, p. 680). From more detailed calculations of his student Ralph Alpher, Gamow drew some sharp consequences, such as: The building-up processes of the chemical elements must have occurred in the first thirty minutes where the temperature of the hot initial state had dropped to about 10^9 K and 'the expansion of the universe was governed entirely by radiation and not yet matter' (Gamow, *loc. cit.*, p. 681). By evaluating further the time-dependence of the radiation density and the matter density, respectively, he argued that the formation of galaxies began once the densities had become equal. Alpher and Robert Herman then improved upon some of the sloppy errors in Gamow's theory and obtained an age of 10^7 years for the Universe at that moment; i.e., 'the temperature of the gas at the time of the condensation was 600 K, and the temperature in the universe at the present time is found to be about 5 K' (Alpher and Herman, 1948, p. 775).[1230]

In summer 1964, when Hoyle and Tayler reported difficulties of the steady-state model to reproduce the observed helium abundance, the English translation of a short article by A. G. Doroshkevich and I. D. Novikov appeared in the journal *Soviet Physics-Doklady*, entitled 'Mean Density of Radiation in the Metagalaxy and Certain Problems in Relativistic Cosmology,' in which they tried to compute the spectral composition of the distribution of electromagnetic radiation in the

[1228] For a recent review, see Konijn, 1999.

[1229] The latest Super Kamiokande data, reported at the 1999 International Neutrino Physics and Astrophysics Conference, confirm that the neutrino masses obtained would not contribute much to the solution of the 'dark matter problem,' which claims a large mass deficit o the expanding universe (see, e.g., Steinhardt, 1997).

[1230] In a letter, written by Gamow to Alpher, he said at that time: 'The space temperature of about 5 K is explained by the present radiation of stars (C-cycles). The only thing we can tell is that the residual temperature from the original heat of the universe is *not higher* than 5 K.' (Gamow to Alpher, 1948, quoted in Penzias, 1992, p. 454)

Universe. Referring to Gamow's earlier prediction of a residual radiation temperature, they noticed essential agreement between the theoretical estimate and preliminary measurements at Bell Labs, adding: 'Additional measurements in this region (preferably on a satellite) will assist in the final solution of the problem of the correctness of the Gamow theory.' (Doroshkevich and Novikov, 1964, p. 113). Apparently, independently of the Russian astrophysicists, a Princeton group of theoreticians and experimentalists, led by Robert Dicke, became involved in an investigation of 'Cosmic Black-Body Radiation'—as they entitled their publication in the July 1965 issue of the *Astrophysical Journal* (Dicke, Peebles, Roll, and Wilkinson, 1965). In particular, they asked the question: 'Could the universe have been filled with blackbody radiation from [a] possible high temperature state?' and answered:

> If so, it is important to notice that as the universe expands the cosmological redshift would serve to adiabatically cool the radiation, while preserving the thermal character. The radiation temperature would vary inversely as the expansion parameter (radius) of the universe. (Dicke *et al.*, *loc. cit.*, p. 415)

Dicke *et al.* then considered accordingly the situation in the expanding universe, both at very high temperatures $T > 10^{10}$ K (where the large thermal electron and photon density also establishes a thermal equilibrium abundance of electron-type neutrinos) and at temperatures below 10^{10} K. At about 10^{10} K (where the temperature, rather kT, corresponds to $m_e c^2$), the development toward the present state of the universe would begin, with the successive formations of protons, chemical elements, etc. Dicke *et al.* estimated in their scenario an upper limit for the temperature determining the blackbody radiation remaining from the primordial fireball, namely, 40 K, because this value 'provides an energy density of 2×10^{-29} gm cm^3, very roughly the maximum total energy density compatible with the observed Hubble constant and acceleration parameter'; they concluded: 'Evidently it would be of considerable interest to attempt to detect this primeval thermal radiation directly.' (Dicke *et al.*, *loc. cit.*, p. 415) While the experimental setup of P. G. Roll and David T. Wilkinson of the Princeton group—'a radiometer and receiving horn capable of an absolute measure of thermal radiation at a wavelength of 3 cm'—did not yield any results up to May 1965, when the paper was submitted, the Princeton authors wrote: 'We recently learned that Penzias and Wilson at the Bell Telephone Laboratories have observed background radiation at 7.3-cm wavelength,' and reported further:

> In attempting to eliminate (or account for) every contribution to the noise seen at the output of their receiver, they ended with a residual radiation of 3.5 ± 1 K. Apparently this could be only due to radiation of unknown origin entering the antenna. (Dicke *et al.*, *loc. cit.*, p. 416)

As Robert W. Wilson recalled in his Nobel lecture, the 20-ft horn reflector (referred to by Doroshkevich and Novikov above) was 'built in 1960 to be used with an ultralow-noise communication receiver for signals bounced from the Echo satellite' (R. W. Wilson, 1992, p. 463). After describing the experiment, he went on to say:

> In 1963, when the 20-foot horn-reflector was no longer needed for satellite work, Arno Penzias and I started preparing it for the use of radio astronomy.... Its sensitivity, or collecting area, could be accurately calculated and in addition could be measured using a transmitter located less than 1 km away. With this data it could be used with a calibrated radiometer to make primary measurements of intensities of several extraterrestrial radio sources.... In addition, we would be able to understand all sources of antenna noise..., so that the background regions could be measured absolutely. Traveling-wave maser amplifiers were available for use with the 20-foot horn-reflector, which meant that for large diameter sources (those subtending angles larger than the antenna beam width) this would be the world's most sensitive radio telescope. (R. W. Wilson, *loc. cit.*, p. 466)

Especially, the instrument appeared to be ideal for determining the weak halo radiation at shorter wavelengths, and Penzias and Wilson began with a programme of measurements at 7 cm (after which they planned to build a similar radiometer at 21 cm).[1231]

Before proceeding with the programme at all, Penzias and Wilson carried out an absolute flux determination and arrived at both a disturbing and exciting result, which they found worthwhile to announce in a note, entitled 'A Measurement of Excess Antenna Temperature at 4,080 Mc/s,' submitted on 13 May 1965, to the *Astrophysical Journal*, namely:

> Measurements of the effective zenith noise temperature of the 20-foot horn-reflector antenna at the Crawford Hill Laboratory, Holmdel, New Jersey, at 4,080 Mc/s have yielded a value about 3.5 K higher than expected. This excess temperature is, within the limits of our observations, isotropic, unpolarized, and free from seasonal variations (July 1964–April 1965). A possible explanation for the observed excessive noise temperature is the one given by Dicke, Peebles, Roll and Wilkinson (1965) in a companion letter in this issue. (Penzias and Wilson, 1965, pp. 419–420)

That is, of the total antenna temperature of 6.7 K measured at the zenith, Penzias and Wilson ascribed 2.3 K to atmospheric absorption and 0.9 K to ohmic losses in the antenna and backlobe response; hence, the remaining 3.5 ± 1.0 K had to be supplied by a different effect, most probably by the remnant Big Bang blackbody radiation. P. G. Roll and David T. Wilkinson of the Princeton team soon, i.e., toward the end of January 1966, sent a note to *Physical Review Letters*, confirming the conclusion: with their Dicke-type radiometer operating at 3.2 cm wavelength, they measured a background temperature,

$$T_{\mathrm{BG}} = 3.0 \pm 0.5 \text{ K} \tag{975}$$

[1231] The experimental preparations included, e.g., the construction of reference noise sources operating at helium temperatures, amplifiers, etc. (For further details, see R. W. Wilson, *loc. cit.*, pp. 467–471.)

(Roll and Wilkinson, 1966, p. 406). Their observed monochromatic brightness and the Penzias–Wilson value at 7 cm fitted nicely into a Planck curve with a cosmic background radiation temperature of 3 K (see Roll and Wilkinson, *loc. cit.*, Fig. 2, p. 407); further, Roll and Wilkinson found that the 'T_{BG} measured with the antenna pointing in various directions was isotropic ... to within $\pm 10\%$' (Roll and Wilkinson, *loc. cit.*). A dozen years later, the 3 K blackbody law had been substantiated in the wavelength range between 100 and 0.1 cm (see Fig. 12 in R. W. Wilson, 1992, p. 481); at that time, in December 1978, Penzias and Wilson shared the Nobel Prize in Physics with Peter Kapitza. After a dozen more years, the results of the COBE (Cosmic Background Explorer) satellite, launched in November 1989, became available; they fixed the cosmic background temperature at 2.725 ± 0.01 K (Mather *et al.*, 1990, p. L39).

The discovery of the essentially isotropic 3 K blackbody radiation of the Universe established the Big Bang hypothesis practically as a fact. Even its old opponent Fred Hoyle contributed to the proof by showing that a background radiation of 2.7 K did explain the helium abundance (about 25%) as due to the primordial nucleosynthesis (Wagoner, Fowler, and Hoyle, 1967). In his exciting, popular book *The First Three Minutes*, Steven Weinberg narrated in detail the history of the early universe in five scenes, from the first hundredth second up to three minutes, during which the composition of the universe changed from a primordial soup to the era of nucleosynthesis (Weinberg, 1977, Chapter 5). This fascinating scenario has been extended further, back to earlier and forward to much later moments, but it still stimulates the scientific and literary imagination of astrophysicists and particle physicists alike (see, e.g., the books of Joseph Silk, 1980, and Harald Fritzch, 1983). While many of these presentations, similar to the purely scientific investigations of the topics, rest on the Standard Model of elementary particle physics (described earlier in Section I.5) and the standard model of the hot Big Bang Cosmology (described above in this section), Alan H. Guth of Stanford suggested a few extensions in his paper, 'Inflationary Universe: A Possible Solution of the Horizon and Flatness Problems,' which he claimed to be 'completely natural in the context of grand unified models of elementary particles' (Guth, 1981, p. 347). Although it may be too early for the historian to embark upon such speculative theories, one may conclude that the recent developments provide a wonderful demonstration of the power of quantum theory, here even the fundamental law of Max Planck's which started this entire development, in throwing light on perhaps the deepest problem of physical science: the origin of the Universe.

3 New Aspects of the Interpretation of Quantum Mechanics

Bryce S. DeWitt and R. Neill Graham introduced their 'Resource Letter on the Interpretation of Quantum Mechanics,' published in the July 1971 issue of the *American Journal of Physics* by stating:

> No development of modern science has had a more profound impact on human thinking than the advent of quantum theory and the discovery of the laws of quantum mechanics. Wrenched out of centuries-old patterns, physicists of a generation ago found themselves compelled to embrace a new metaphysics. The distress which this reorientation caused continues to the present day. Basically, physicists have suffered a severe loss: their hold on reality.
>
> Philosophers have been quick to step in with assurances that the problem is only in the mind, and that if physicists would just adopt the right epistemological stance, all difficulties would disappear. But physicists are, at bottom, a naive breed, forever trying to come to terms with the "world out there" by methods which, however imaginative and refined, involved in essence the same element of direct contact as a well-placed kick. The reality of direct contact imparts a view of nature which no metaphysical arguments, however ingenious, can erase. Time after time the old malaise over quantum mechanics returns. Many a grand old man of physics (and many another not so grand) has had his try of solving the metaphysical problem. But the same issues keep coming back again and again, and half forgotten "solutions" are refurbished and served up as new by successive generations. (DeWitt and Graham, 1971, p. 724)

In spite of the 'naive' epistemological view of the physicists about the 'world out there,' the treatises on quantum mechanics until the 1950s did not reflect much concern at all about the interpretation of the theory. As in P. A. M. Dirac's *The Principles of Quantum Mechanics* (1930d), the question is not addressed in Jakov Frenkel's *Wave Mechanics* (1934), Linus Pauling and E. Bright Wilson's *Introduction to Quantum Mechanics* (1935), etc., up to Leonard I. Schiff's standard postwar textbook *Quantum Mechanics* (1949).[1232]

The situation began to change with the new book on *Quantum Theory*, written by David Bohm, a young associate professor at Princeton University, who declared right away in the preface: 'It is not generally realized that the quantum theory represents a radical change, not only of the content of scientific knowledge, but also of the fundamental conceptual framework in terms of which such knowledge can be expressed.' (Bohm, 1951, p. iii) Indeed, Bohm devoted full chapters to the 'fundamental conceptual framework,' especially Chapter 8 to 'An Attempt to Build a Physical Picture of the Quantum Nature of Matter' and Chapter 22 to the 'Quantum Theory of the Measurement'; the latter included, besides a detailed presentation of the standard views of Bohr and Heisenberg, also a discussion of the ideas of Einstein, Rosen, and Podolsky. And he (Bohm) would

[1232] Edwin C. Kemble, in the prewar standard American treatise *Fundamental Principles of Quantum Mechanics*, dealt in Section 14 with the 'Statistical Interpretation of the Wave Theory of Matter' and just emphasized 'the necessity of introducing Gibbsian assemblages [in order to] interpret the results from the mathematical machinery of quantum mechanics,' and noted that: 'In calling attention to the parallelism between quantum mechanics and classical statistical mechanics we must emphasize that the quantum-mechanical study of the behavior of assemblages whose statistical behavior can be described by a single wave function is *not* the quantum-mechanical way of treating assemblages of the older form of statistical mechanics, [because] the quantum-mechanical generalization of classical statistical mechanics ... has to do with assemblages of systems including many quantum-mechanical states and thus involving many independent wave functions.' (Kemble, 1937, p. 55, footnote) soon move on to a severe criticism of the orthodox ‘Copenhagen interpretation’ of quantum mechanics. The old pioneers of the ‘Copenhagen spirit’ replied to Bohm rather harshly; however, Louis de Broglie and several younger physicists reacted more positively. Thus, Bohm initiated a new debate on the old arguments that had been formulated in the late 1920s and early 1930s; in particular, he discussed the concepts of indeterminism, complementarity, and the reduction of wave packets, and especially the idea of ‘hidden variables.’

The renewed debate on the foundation of quantum mechanics focused on several aspects. One of these concerned the analysis of the existing quantum-mechanical scheme and the endeavour to cast it into a more rigorous, modernized mathematical language. As an example, we may refer to the reflective book of Günther Ludwig on ‘*Grundlagen der Quantenmechanik*’ (1954). Ludwig, who had obtained his doctorate in 1943, wished ‘to cast the essential (*eigentliche*) quantum mechanics—i.e., without [quantum] field theory—in a way that one recognizes it as a mathematically well-founded, closed system like that of classical point mechanics’ and at the same time also bring to light ‘the inner harmony existing between the mathematical and physical structure’ (Ludwig, 1954, p. vii). Ludwig’s book evidently continued the German tradition established by Hermann Weyl and John von Neumann in prewar times; and it stimulated, at least in Western Europe, a trend in theoretical research which developed, e.g., the use of ‘rigged Hilbert space’ and ‘C*-algebras’ in quantum mechanics. In the United States, on the other hand, Eugene Wigner’s investigations led to a new tradition of a stricter mathematical analysis of the quantum-mechanical theory, which resulted in such treatises as *The Mathematical Foundations of Quantum Mechanics* written by the Harvard mathematician George Whitelaw Mackey (1963).

In connection with the new mathematical foundation of quantum mechanics, the question of an alternative form of logical structure, deviating from the classical Aristotelian two-valued logic, arose, especially after Hans Reichenbach had claimed that one should axiomatize the theory on the basis of a ‘three-valued’ logic (Reichenbach, 1944). As a result of extensive investigations, Peter Mittelstaedt arrived at a somewhat modified conclusion: The classical logic remains valid also in modern atomic theory, as long as only simultaneously measurable quantities are considered; however, in order to deal with arbitrary, especially noncommuting quantities, two possibilities have to be considered:

> Either one uses, when applying the [conventional] logic, [in addition] the knowledge of quantum theory; then some laws of [this] logic cannot be applied, although they remained valid further.... Or one does not use the knowledge of quantum theory when applying the laws of logic.... Then some laws of [the conventional two-valued logic] turn out to be wrong. The reason is that the quantum-mechanical statements, because of the particular physical conditions, for which alone they can be proven, cannot be applied in an unlimited way, but must be restricted. Consequently, essential restrictions become evident in the effective logic. Then, especially, the theorem $A \rightarrow (B \rightarrow A)$ [$\rightarrow$ means “implies”] turns out to be invalid [in the effective quantum logic]. Because of the explicit form of the statements used, i.e., due to the possibility

> to represent statements [considering] subspaces of the Hilbert space, also here further theorems can be proven. Thus, in particular, the *tertium non datur* remains valid also in the complete quantum logic. (Mittelstaedt, 1963, p. 146)

Other than logical investigations, carried out by mathematicians and physicists (see, e.g., von Weizsäcker, 1955), relatively few analyses of quantum mechanics existed by the professional philosophers; most of the relevant investigations on this subject were carried out by the physicists themselves—see, e.g., the book *Conception de la Physique Contemporaine* by Bernard d'Espagnat (1965), in which he reviewed the existing interpretations of quantum mechanics in the light of the standard philosophical views of realism, positivism, and idealism.[1233]

In spite of the perhaps disappointing lack of deeper response from the professional philosophers to the challenges raised by the new world view provided in the atomic theory in the twentieth century, the literature of the past fifty years on the interpretation of quantum mechanics appears to be quite abundant and deals with many of its aspects very exhaustively; hence, we shall restrict ourselves to sketch only three characteristic developments.[1234] The first development summarizes the last responses of the old pioneers to the renewed questions on the meaning and significance of their theories; in particular, they clarified or made firm their previous standpoints on the problem of the interpretation of quantum mechanics, which did not always reflect the orthodox Copenhagen point of view (Subsection 3.1). From 1952 onward, several physicists attempted to create a different interpretation of microscopic phenomena; occasionally, these attempts even involved alterations of the existing quantum-mechanical theory, and considerable debates followed regarding the consistency of the various proposals (Subsection 3.2). In the course of these discussions, the physicists conceived a series of crucial experimental tests which should allow one to decide between the diverging interpretations or even theories. The actual performance of these experiments since the early 1970s then confirmed rather brilliantly the standard quantum mechanics and, at the same time, emphasized the characteristic features of this theory, especially the fundamental 'nonlocal structure' of the interaction between atomic particles (Subsection 3.3). Important results from these efforts seem to have been incorporated recently as standard knowledge in the textbooks. Thus, Leslie E. Ballentine wrote in the preface of the new edition of his book on *Quantum Mechanics. A Modern Development*:

> Although there are many textbooks that deal with the formal apparatus of quantum mechanics and its applications to standard problems, before the first edition of this book (1990) none took into account the development in the foundations of the sub-

[1233] The list given in B. S. DeWitt and N. Graham (1971, Section IV) contains mainly the books and reports on the various interpretations of quantum mechanics by the principal pioneers themselves (such as Bohr, de Broglie, Heisenberg, and Schrödinger) or by other physicists.

[1234] A rather comprehensive history of *The Philosophy of Quantum Mechanics* has been given by Max Jammer (1974), and a more condensed, selective report on the more recent developments by Abner Shimony (1989).

> ject which have taken place in the last few decades. There are specialized treatises on various aspects of the foundations of quantum mechanics, but they do not integrate those topics into the standard pedagogical material. I hope to remove that unfortunate dichotomy, which has divorced the practical aspects of the subject from the interpretation and broader implications of the theory. (Ballentine, 1998, p. xi)

The new aspects developed in the interpretation of quantum mechanics during the latter half of the twentieth century thus complete this Epilogue of *The Historical Development of Quantum Theory*. Although not all questions have been answered, this development exhibits the vitality of the great theory which Max Planck introduced exactly one hundred years ago, and which has demonstrated its power so brilliantly during the course of the twentieth century.

3.1 The Copenhagen Interpretation Revisited and Extended (1948–1966)

'It is a curious historical fact that modern quantum mechanics began with two quite different mathematical formulations: the differential equation of Schrödinger, and the matrix algebra of Heisenberg,' thus Richard P. Feynman introduced his essay on 'Space-Time Approach to Non-Relativistic Quantum Mechanics,' in which he published the most important parts of his 1942 Ph.D. thesis at Princeton. He continued:

> The two, apparently dissimilar approaches, were proved to be mathematically equivalent. These two points of view were destined to complement one another and to be ultimately synthesized in Dirac's transformation theory.
>
> This paper will describe what is essentially a third formulation of nonrelativistic quantum theory ... suggested by some of Dirac's remarks (Dirac, 1933, 1945) concerning the relation of classical action to quantum mechanics. A probability amplitude is associated with an entire motion of the particle as a function of time, rather than simply with a position of the particle at a particular time. (Feynman, 1948a, p. 367)

Feynman then described (as we have mentioned earlier in Section 1.1(b)) the probability an atomic particle, travelling between two space points, as a sum of contributions emerging from each possible path, and claimed that 'the formulation is mathematically equivalent to the more usual formulations' (Feynman, *loc. cit.*). At that time, it did appear to be completely consistent with the standard interpretations of quantum mechanics, although—as we shall discuss in Section 3.2—it could also be later associated with a different interpretation, the so-called 'many-world interpretation' of quantum mechanics.

Before Feynman sat down to compose the above-mentioned paper, Wolfgang Pauli—meanwhile back in Zurich (after spending the war years in Princeton)—reported to Niels Bohr in Copenhagen that he had been asked 'to organize a special issue of [the new philosophical periodical journal] *Dialectica* on complementarity and the foundations of quantum mechanics' and wrote: 'My answer to the editor will very much depend on your reaction to this suggestion.' (Pauli to Bohr, 5 May 1947, in Pauli, 1993, p. 438) Bohr agreed with the project and also to write a paper, and Pauli sent out invitations for further contributions to Louis de Broglie, Jean-Louis Destouches, Paulette Destouches-Fevrier, Albert Einstein, and Werner Heisenberg. The issue number 3/4 of *Dialectica*, which appeared in fall 1948, indeed contained essays by these and other authors, including one by Ferdinand Gonseth, coeditor of the journal, and the editorial by Wolfgang Pauli himself. All of the articles, except one, more or less supported the concept of complementarity—of which Bohr provided a lucid presentation: it involved a detailed definition of a physical state in atomic physics, especially listing the experimental conditions to observe it and describing the apparatus in the 'common language supplemented with the terminology of classical physics' (Bohr, 1948, p. 313). Einstein, on the other hand, used the opportunity to repeat again his objections that had been familiar since the Einstein–Podolsky–Rosen paper (1935), recalling that 'if in quantum mechanics we consider the ψ-function as (in principle) a complete description of a real physical situation we thereby imply the hypothesis of action-at-a-distance, a hypothesis which is actually hardly acceptable,' and 'if, on the other hand, we consider the ψ-function as an incomplete description of a real situation, then it is hard to believe that, for this incomplete description, strict laws of temporal dependence hold' (Einstein, 1948, p. 323). Pauli commented on this in his editorial remarks as follows:

> According to my opinion, one cannot draw from the particular cases of correlated systems [as considered in the Einstein–Podolsky–Rosen paradox] any new conclusions which are not already contained in the previously mentioned requirement in quantum mechanics of giving up the general predictability of the results of individual observations on a single atomic system in a given state. In view of both the empirical facts and the existence of logically consistent quantum-mechanical formalism it seems to me that only this renouncement enables us still to use in physics the concept [of a] "closed system" and the usual perception of space and time, which are too closely connected with each other. It is in this sense that I consider the quantum-mechanical description to be complete. (Pauli, 1948, p. 309)

A more explicit description of 'closed systems' was provided by Heisenberg (1948) in the same issue of the *Dialectica*.

Bohr and Einstein continued to exchange their divergent views concerning the description of phenomena in atomic physics and the meaning of reality in physics in Einstein's seventieth-birthday volume, *Albert Einstein: Philosopher-Scientist*, edited by Paul Arthur Schilpp (1949). There, Bohr published his historic reminiscences of 'Discussions with Einstein on Epistemological Problems in Atomic Physics' (Bohr, 1949)—which we have dealt with in Chapter II—and Einstein replied, still unmoved after twenty years of their discussions at the fifth and sixth Solvay Conferences in Brussels:

> What does not satisfy me in that theory [of quantum mechanics], from the standpoint of principle, is the attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation). (Einstein, 1949, p. 667)

In particular, if a quantum theoretician assumed the description of a radioactively decaying atom by the ψ-function as complete, he must reject also the postulation of a specific decay time and would run into the 'cat paradox' described by Erwin Schrödinger (1935a). Then, he would have to accept the interpretation—Einstein continued—that the macroscopic registration of a decay event 'is essentially dependent upon the carrying out of the observation made in a registration-strip.' And he commented further:

> Such an interpretation is certainly by no means absurd from a purely logical standpoint; yet there is hardly likely to be anyone who would be inclined to consider it seriously. For, in the macroscopic sphere it simply is considered certain that one must adhere to the program of relativistic description of space and time. (Einstein, 1949, p. 671)

Einstein concluded: 'Within the framework of statistical quantum theory there is no such thing as a complete description of an individual system,' or: 'The attempt to conceive the quantum-theoretical description as the complete description of the individual systems leads to unnatural theoretical interpretations, which becomes immediately unnecessary if one accepts the interpretation that the description refers to ensembles of systems and not to individual systems.' (Einstein, *loc. cit.*, pp. 671–672) With this position, Einstein approached the understanding of some physicists, especially in the Soviet Union; they attacked—on the basis of Lenin's dialectical materialism—Bohr's views on complementarity as being 'subjective,' even as 'idealistic and agnostic speculations,' and insisted that 'quantum mechanics in reality investigates the objective nature of the quantum-mechanical assembly (*Gesamtheit*) existing independently of the observer.' (Blochinzev, 1957, p. 500)[1235]

Einstein continued to advertize his views in the *Scientific Papers Presented to Max Born*—actually, the *Festschrift* of contributions assembled for his friend on the occasion of his retirement in 1953 from the Tait Chair of Natural Philosophy at the University of Edinburgh—under the title '*Elementare Überlegungen zur Interpretation der Grundlagen der Quantenmechanik* (Elementary Considerations on the Interpretation of Quantum Mechanics)' (Einstein, 1954). Einstein illustrated in this article his doubts about the description of the 'real state' of an individual atomic system by the wave function with the help of a specific example: a ball about 1 mm in diameter oscillates between two parallel wells having 1 m distance between them (Einstein, *loc. cit.*, p. 35ff.). After writing the quantum-mechanical formulae and obtaining the result in terms of Born's probability

[1235] Also see Heisenberg's analysis (1955, p. 21) of Blochinzev's views.

approach, he again arrived at the conclusion that the standard formalism could not describe the individual systems; hence, it failed to account for what he considered to be the 'real state' (Einstein, *loc. cit.*, p. 38).

Born and Einstein had, since 1947, exchanged letters dealing with their diverging views about the questions of the interpretation of quantum mechanics (see the original German edition of Einstein, H. and Max Born, 1969, especially, pp. 214–249, English translation, 1971, pp. 157–225), and Born had contributed an article on 'Einstein's Statistical Theories' to the Schilpp volume, in which he stressed Einstein's pioneering role on the path to his (Born's) statistical interpretation of the quantum-mechanical wave function (Born, 1949). Now, after receiving in fall 1953, Einstein's official reply to this article (Einstein, 1954), Born reconsidered the problem of interpretation and himself examined the details of Einstein's example.[1236] Wolfgang Pauli, who spent several weeks in Princeton in spring 1954, got involved in the exchange between the views of the two opponents (Einstein and Born), and helped to clarify their respective standpoints by talking to Einstein and writing to Born, although—in principle—he shared Born's viewpoint.[1237] Thus, he commented to Born on one of Einstein's crucial assumptions about the 'objective description of reality' as follows:

> That a macroscopic body should always have a quasi-sharply-defined position, I don't believe to be true, for I cannot see any difference in principle between a microscopic and a macroscopic body. Hence a largely indeterminate position must always be assumed where the *wave-aspect* of the physical object under investigation manifests itself. (Pauli to Born, 31 March 1954, in Einstein *et al.*, 1969, p. 295; 1971, p. 223)

This exchange of correspondence with Pauli assisted Born in formulating a detailed answer to Einstein in a manuscript, entitled 'Continuity, Determinism and Reality' and submitted for publication (in a Danish *Festschrift* dedicated to Niels Bohr on his seventieth birthday) just a few days before Einstein's death on 18 April 1955 (Born, 1955). In this investigation, Born treated Einstein's example in one dimension; i.e., he computed the case of a one-dimensional gas both in classical mechanics and quantum mechanics, and arrived at a conclusion quite different from Einstein's, namely:

> It is misleading to compare quantum mechanics with the deterministically formulated classical mechanics; instead, one should first reformulate the classical theory, even for a single particle, in an indeterministic, statistical manner. After that some of the distinctions between the two theories disappear, [while] others emerge with great clarity. Amongst the first is the feature of quantum mechanics, that each measurement interrupts the automatic flow of events and introdues new intitial conditions (so called

[1236] See the letters exchanged between 26 November 1953 and fall 1954, in Einstein and H. and M. Born, 1969, pp. 275–303.

[1237] See Pauli to Born, 3 and 24 March 1954, and 15 April 1954, in Einstein, H. and M. Born, 1969, pp. 293–299.

"reduction of probability"); this is true just as well for a statistically formulated classical theory. (Born, *loc. cit.*, pp. 25–26)

Furthermore, the specific quantum effects, as expressed in the uncertainty relations, 'for macroscopic bodies can be made small in the beginning and then remain small for a long time ... but there is always a critical moment t_c, where this ceases to be true and the quasi-individual is transforming itself into a genuine statistical ensemble' (Born, *loc. cit.*, p. 26). Pauli congratulated Born by saying: 'Your paper in the Danish *Bohr-Festschrift* now is very pleasant to read. The epistemological (*erkenntnistheoretische*) content has now become very clear, and I agree with everything.' (Pauli to Born, 11 December 1955, in Einstein *et al.*, 1969, p. 301)

Max Born had carried out the considerations by examining the behaviour *of particles* in the classical and quantum theories, respectively. Certain physicists found the use of the particle picture particularly adequate also in quantum mechanics, in order to arrive at *the* realistic picture of microscopic objects. For example, Fritz Bopp, Arnold Sommerfeld's successor at the University of Munich, investigated in a series of papers the statistics of quantum-mechanical many-body systems (1952) and applied the results to the creation and annihilation processes of particles in space and time (Bopp, 1953). As Heisenberg noted, 'he [Bopp] interprets the laws of quantum mechanics as a special case of correlation statistics of such events, [hence] the symmetry between corpuscle and wave ... could only be assumed if the corresponding correlation statistics were developed for three-dimensional waves as well' (Heisenberg, 1955, p. 20).[1238] Even more, Lajos Janossy in Budapest and Alfred Landé in Columbus, Ohio, attempted to restore the particle picture and thereby the 'unity' of quantum mechanics. Thus, Janossy (1953) constructed a (classical) model of a photon, which could be absorbed and emitted as a pointlike object, in order to remove what he called the 'idealistic and positivistic' concepts of quantum mechanics (like complementarity, influence of the observer on the object) and to replace them by those which followed more the 'materialistic' ones (acceptable to the Communist doctrines). Landé, on the other hand, published in 1955 his new book on the *Foundations of Quantum Mechanics*, in which he based the theory on 'a general postulate of continuity for cause-effect relations' and symmetry, such that 'one can avoid the idea that the basic aim of nature is that of presenting us with an enigmatic duality between wave and particle traits of matter' (Landé, 1955, p. v). Starting from a discussion of the diffraction of light in crystals, he arrived at the result:

The idea that the principal plan of nature is to conceal forever, through a principle of duality, whether matter consists of waves or particles is a rather limited point of view. Wave-particle duality is not a fundamental principle but rather a consequence (a) of

[1238] Also see the later article of Bopp, '*Statistische Mechanik bei Störung des Zustandes eines physikalischen Systems durch die Beobachtung* (Statistical Mechanics in Case of the Perturbation of the States of a Physical System by Observation),' in which the author developed 'a new access to quantum mechanics' based on treating physical systems as consisting of particles (Bopp, 1961).

> the general superposition of probability amplitudes which follows from the postulate of entropy continuity and from the supposed existence of a general q-relation; (b) of the postulate that certain observables, q and p, are in such a relation that the q-reaction determines the p-reaction uniquely; and (c) the application of these postulates to the most elementary variables, i.e., those of location in space-time, as conjugate to the most fundamental conservative quantities, momentum and energy, which together form a four-vector $(p, E/ic)$ conjugate to the four-vector $(\mathbf{r}, ict)$. (Landé, *loc. cit.*, p. 76)

Landé followed, in his new foundation of quantum mechanics an old pre-quantum-mechanical treatment of William Duane of the scattering of light by a crystal (1923), which he transferrfed to the case of electrons; thus, a 'corpuscular-mechanical interpretation of matter scattering' was obtained 'without thinking of the dualistic nature of electrons.' In this spirit, he claimed:

> Duane's theory is a precursor and essential constituent of the *unified* quantum mechanics of particles and particle-systems. One does not need the shaky picture that matter (and also radiation) *sometimes* manifests itself as a corpuscle and *sometimes* as a wave. (Landé, 1961, p. 121)

When he enlarged upon these ideas of a nondualistic quantum theory in another, later book (entitled *New Foundations of Quantum Mechanics*: Landé, 1965), he received critical comments from two sides. On the one hand, Abner Shimony, in a review of the book, attacked the basic assumptions of Landé—the 'doctrine of absolute chance,' the 'principle of cause-effect continuity,' and the 'law of interdependence for transition probabilities'—on physical, logical and mathematical grounds (Shimony, 1966). Max Born and Walter Biem, on the other hand, objected because 'Landé has not realized the historical origin of the dualistic interpretation and does not correctly describe its physical meaning'; hence, 'his fight against "dualism" in modern quantum theory seems to be a tilt against windmills' (Born and Biem, 1968, p. 51). Upon recalling the origin of the dualistic picture of radiation, as introduced by Einstein (1909a) when considering the energy fluctuation $\Delta\overline{E^2}$ of the blackbody radiation in a cavity of volume V,

$$\Delta\overline{E^2} = \rho^2 V + h\nu\rho V \tag{976}$$

(with ν the frequency and ρ the density) and the application of this picture to 'explain' Bose statistics (Einstein, 1925a), Born and Biem remarked that Landé had to assume—in order to transfer Duane's treatment of X-ray scattering of material particles—Louis de Broglie's correspondence of particles and waves, that is, exactly the dualistic picture which he wanted to abolish. They further corrected several of Landé's misunderstandings of Einstein's position in the interpretation problem—Einstein believed in a nonlinear field theory of matter as well as of Schrödinger's position—the latter thought of a (unitary) wave theory of matter[1239]—and finally concluded:

> The discussion (of the question) of dualism or nondualism appears to be superfluous. Since Einstein's discovery of the fluctuation equation [(976)] it has become more and more obvious that nature can be described not by particles or waves alone, but by a more sophisticated mathematical theory. This is the quantum theory which supersedes both models and only in certain limits represents one or the other. Quantum theory has become known to us as a complete whole since the end of the 1920s. We need not turn from a particle picture to a wave picture arbitrarily, and we need not be without real comprehension when using it. It is, on the contrary, possible to represent the states of a system in different ways, and the representations are connected by unique transformations. (Born and Biem, 1968, pp. 64–65)

The pioneers of the standard interpretation of quantum mechanics did not indeed see any reason to abandon the main concepts introduced between 1926 and 1927. Thus, Heisenberg concluded his critical review of 1955:

> The criticism of the Copenhagen interpretation rests quite generally on the anxiety that, with this interpretation, the concept of "objective reality," which forms the basis of classical physics, might be driven out of physics. As we have here exhaustively shown, this anxiety is groundless, since the "actual" plays the same decisive part in quantum theory as it does in classical physics. The Copenhagen interpretation is indeed based on the existence of processes which can be simply described in terms of space and time, i.e., in terms of classical concepts, and which thus compose our "reality" in the proper sense. If we attempt to penetrate behind this reality into the details of atomic events, the contours of this "objectively real" would dissolve—not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of a mathematics whose laws govern the possible and not the actual. It is of course not by chance that the "objective reality" is limited to the realm of what Man can describe simply in terms of space and time. At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore is dependent on Man. The idealistic argument that certain ideas are *a priori* ideas, i.e., in particular come before all natural science, is here correct. The ontology of materialism rests upon the illusion that the kind of existence, the direct "actuality" of the world around us, can be extrapolated into the atomic range. This extrapolation, however, is impossible. (Heisenberg, 1955, p. 28)

Still, within the framework of the standard interpretation, certain amendments and clarifications appeared possible or even necessary in the perhaps central part of that interpretation—the quantum-mechanical theory of measurement—and some were indeed proposed.

[1239] For Schrödinger's final stand concerning the question of interpretation, see his posthumously published notes on that topic in the Dublin seminars (1945–1955) and other later essays published posthumously (Schrödinger, 1995).

The standard theory of measurement had been formulated mathematically in detail by John von Neumann in his book *Mathematische Grundlagen der Quantenmechanik* (1932a) and presented in a less-technical formulation by Fritz London and Edmond Bauer several years later (1939). After World War II (and, especially, after the death of John von Neumann, whom he had assisted in writing his canonical text of 1932), Eugene Wigner began to take great interest in the measurement problem, both in the nonrelativistic as well as the relativistic theories.[1240] Thus, for example, he worked out with T. D. Newton the concept of the position operator in a relativistic elementary particle system (Newton and Wigner, 1949), and in another paper on '*Die Messung quantenmechanischer Operatoren* (The Measurement of Quantum-Mechanical Operators,' he emphasized that 'the usual assumption of the statistical interpretation of quantum mechanics, i.e., all Hermitean operators represent measurable quantities, will in general be recognized as a comfortable mathematical idealization, not as expression of a fact' (Wigner, 1952, p. 101). In particular, he discovered that 'already the validity of conservation laws for quantized variables (like angular momentum and electric charge), governing the interaction between the observed object and the measuring apparatus, permits one to measure most operators only in an idealized limit [i.e., approximately]' (Wigner, *loc. cit.*). Besides investigating tricky details, such as the difficulties brought into the measurement problem by the existence of states connected by the so-called 'superselection rules'—these separate subspaces in the Hilbert space describing, say, *n*-particle states having a definite electric charge (Wick, Wightman, and Wigner, 1952)—Wigner also turned to the fundamental conceptual point in von Neumann's quantum-mechanical measurement theory, the position of the '*von Neumann Schnitt* (cut),' i.e., where the reduction of the quantum-mechanical wave packet actually occurred. In a contribution, 'Remarks on the Mind-Body Question,' to a book *The Scientist Speculates*, Wigner addressed that point, which inevitably involves an interaction between the object and the measuring apparatus and the observer, respectively:

> In general, there are many types of interactions into which one can enter with the system [to be observed], leading to different types of observations or measurements. Also the probabilities of the various possible impressions gained at the next interaction may depend not only on the last, but on the results of many prior observations. The important point is that the impression which one gains at an interaction may, and in general does, modify the probabilities with which one gains the various possible impressions at later interactions. In other words, the impression which one gains at an interaction, called also *the result of an interaction*, modifies the wave function of the system. The modified wave function is, furthermore, in general unpredictable before the impressions gained at the interaction has entered our consciousness: it is the entering of an impression into our consciousness which alters the wave function because it modifies our appraisal at the probabilities for different impressions which

[1240] For a review of Wigner's concern with this topic, we refer to Abner Shimony's annotation of Wigner's relevant papers (Shimony, 1997).

> we expect to receive in future. It is at this point that the consciousness enters the theory unavoidably and unalterably. If one speaks in terms of the wave function, its changes are coupled with the entering of impressions into our consciousness. If one formulates the laws of quantum mechanics in terms of probabilities of impressions, there are *ipso facto* the primary concepts with which one deals. (Wigner, 1961, pp. 175–176 of 1967 reprint)

This reference to consciousness—sometimes referred to as 'Wigner's friend'—transposed von Neumann's '*Schnitt* (cut)' from the physical systems into the observer himself. In the following years, Wigner remained a careful observer of the developments in the quantum-mechanical measurement problem and would contribute several critical analyses on the proposed solutions (which we shall describe in Subsection 3.2).

Günther Ludwig attempted, at about the same time as Eugene Wigner, a quite different treatment of the measurement problem. In a paper on '*Der Meßprozeß* (The Process of Measurement),' he argued that 'the decisive point of any measurement with microscopic objects is contained in the fact that, as a final result, a thermodynamically irreversible process has happened in the macroscopic region, independently of any further action of the observer' (Ludwig, 1953, p. 483). He studied over several years the concept of 'macroscopic variables' in particular (see Ludwig, 1958a, b), because:

> The measurements of microscopic systems like atoms or electrons require an apparatus which is large compared with the object. Apart from restrictions caused by superselection rules all subspaces of the Hilbert space of a microscopic object correspond to possible physical observations, the so-called yes-no observations. No observable here is distinguished from another. If we go over the macroscopic systems, which contain a very large number of particles, the situation is quite different. The Hilbert space is then the product of all particles. From quantum theory only, no subspace, i.e., no observable, is distinguished from another. The situation is the following one. Some properties of macroscopic objects are measurable with the help of apparatus, which are not large but even small compared with the object. On the other hand, surely one needs apparatus (measuring some other observables) which are large compared with microscopic objects.... I think it is possible that some observables admissible by quantum mechanics are really unmeasurable, since the construction of such measuring devices is impossible because of their enormous size. (Ludwig, 1961a, p. 59)

Hence, the main problem of measurement consisted, according to Ludwig, in determining the macroscopic variables. At the end of his detailed presentation, he concluded:

> It seems to me to fail if one hopes that all questions and problems concerning the behaviour of macroscopical systems can be solved "in principle" only with the help of quantum mechanics. For this purpose one needs further physical axioms, e.g., such as the choice of macropscopic observables.... New restrictive principles must be added to quantum theory. By these quantum theory is not repealed but restricted so that

> from quantum theory one gets now the real behaviour of large systems as *limiting case under additional conditions*. Since we have to find new additional principles we have a principal problem and not a problem of the application of well-known theories. (Ludwig, *loc. cit.*, p. 113)

Ludwig called the new theory thus obtained 'macrodynamics.' It lay outside the range of quantum mechanics and could, under special conditions, either lead to equilibrium thermodynamics or to nonequilibrium thermodynamics (which would describe both living and dead systems). As Ludwig stated in another article: 'Quantum theory and classical theory are parts of a more general, although not yet developed theory, in which *both* parts (quantum physics and classical physics) appear as limiting cases.' (Ludwig, 1961b, p. 156) Hence, there followed in particular: the quantum theory alone does describe only the microscopic systems; the irreversible processes generating the reduction of the wave packet in the measurement process, on the other hand, require additional laws outside the quantum-mechanical formalism.

In the following year, an Italian group of quantum theoreticians from Milan and Messina, consisting of A. Danieri, Angelo Loinger, and G. M. Prosperi, declared Ludwig's proposal as 'the most complete and satisfactory from a physical point of view' and expanded it mathematically in an investigation entitled, 'Quantum Theory of Measurement and Ergodicity Conditions' (1962). Danieri *et al.* considered von Neumann's reduction of the wave packet as a 'radically subjectivistic (solipsistic)' assumption which had still to be proven by a proper quantum-mechanical procedure establishing the relation between the microscopic observed system and the macroscopic, large body represented by the measuring apparatus. 'In particular, it must be allowed to truncate von Neumann's chain [consisting of the observed system I and the following systems II, III, etc., of increasing size until the measuring apparatus is reached] immediately after the first macroscopic system $S^{(i)}$,' they demanded, and added: 'The interference terms [between the different quantum states involved] must disappear owing to the nature of macroscopic observations and to the properties of the Hamiltonian system $S^{(1)} + S^{(2)} + \cdots S^{(i)}$.' (Danieri *et al.*, 1962, p. 305) Practically, they described the transition from the microscopic system $S^{(1)}$ to the macroscopic system $S^{(i)}$ by constructing a particular interaction Hamiltonian, H_{int}—coupling $S^{(1)}$ and $S^{(i)}$—which had to fulfill a specific ergodicity condition. In the simplest case of a one-step von Neumann procedure, implying only the object system I and the measuring system II, they obtained the result:

Initial state of the system I + II:

$$\Sigma c_r \phi_r \Phi_0; \tag{977a}$$

State at the end of the interaction:

$$\exp\left[-\frac{i}{\hbar} H_{\text{int}} \tau\right] \sum_r c_r \phi_r \Phi_0 = \sum_r c_r \phi_r \Phi_r, \tag{977b}$$

with the macrostate of the ergodic system starting from Φ_0 and approaching the equilibrium state Φ_r in the thermodynamical sense. Simultaneously, the statistical operator before the measurement, i.e.,

$$W_0 = P^{\mathrm{I}}_{-\left[\sum_r c_r \phi_r\right]} P^{\mathrm{II}}_{[\Phi_0]}, \tag{978}$$

was transformed at the end of the interaction into

$$W' = P_{\left[\sum_r c_r \phi_r \Phi_r\right]} \tag{978a}$$

and:

> The time-evolved of this operator after a large time t can be identified, as far as the macroscopic quantities II are concerned, with
>
> $$\tilde{W}_t = \sum_r |c_r^2| P^{\mathrm{I}}_{[\exp\{-i/hH_{\mathrm{int}}t\}} \cdot \frac{1}{s_{re_r}} P^{\mathrm{II}}_{C_{re_r}}. \qquad [(978\mathrm{b})]$$
>
> (Danieri *et al.*, *loc. cit.*, p. 313)

In Eqs. (978), $\sum_r c_r \phi_r$ denoted the initial wave packet of the microsystem, C_{re_r} was a possible state of the macroscopic system in equilibrium (associated with the state ϕ_r of the microsystem), and s_{re_r} was a weight factor. Danieri *et al.* found further that the probability for all macroscopic states except one turned out to be negligible; hence, the desired reduction of the wave packet was achieved even under very weak ergodicity conditions.[1241]

Léon Rosenfeld, who carefully watched over the 'pure' Copenhagen interpretation after the death of Niels Bohr, praised the approach of the Italian physicists as being 'in complete harmony with Bohr's idea' and formulated their result almost verbatim in an essay on 'The Measuring Process in Quantum Mechanics,' submitted to the 'Commemorative Issue for the 30th Anniversary of the Meson Theory by Dr. H. Yukawa,' as:

> We have to visualize the measurement as an interaction between the observed atomic system and a registering device, ultimately leading to the formation to some permanent record, uniquely related to a definite quantity characterizing the state of the atomic system. How does such a permanent mark come about? Since it is of macroscopic character, it cannot be a direct result of the initial atomic interaction; the latter

[1241] The measuring apparatus was thus defined as 'a macrosystem in a thermodynamically metastable state, such that a very small perturbation makes it evolve towards a thermodynamically stable state, dependent on the state of the micro-object' (Danieri *et al.*, 1962, p. 298). In a later note, Danieri, Loinger, and Prosperi generalized this definition of the measuring apparatus applied to all detection methods in elementary particle physics (Danieri *et al.*, 1966, especially, p. 121).

> is rather of the nature of a triggering effect. It starts off within the measuring apparatus a macroscopic reaction, terminating when a state of stable equilibrium is reached, in which the characteristic mark has appeared. The triggering process only lasts for a time interval of the order of magnitude typical for individual atomic reactions; the ensuing macroscopic process necessitates a "relaxation time" which, although it may be very short on the macroscopic scale, will in any case involve a large number of atomic reactions. This is the decisive point: the formation of the permanent mark is a process of ergodic character, entailing the wiping out of all structural details of the initial state of the total system, known as the "reduction" of this state. (Rosenfeld, 1965, p. 225)

Rosenfeld went on to stress a particular point arising in this treatment of the quantum-mechanical measurement process, namely that 'the reduction of the initial state of the atomic system has nothing to do with the interaction between the system and the measuring apparatus,' since it was rather 'related to a process taking place in the latter apparatus after all interaction with the atomic system has ceased' (Rosenfeld, *loc. cit.*, p. 230). Finally, he mentioned how the situation described by Einstein, Podolsky, and Rosen might be described in the formalism of Danieri, Loinger, and Prosperi without revealing any paradoxical situation but rather a normal situation in atomic physics. He concluded:

> The kind of measurement we decide to make irrevocably prescribes for all time the nature of the description we can give of the system. Since, however, we are not limited in the scope of measurements we may choose to perform on many such atomic systems, we are able to explore all aspects of the behaviour of matter on the atomic scale. (Rosenfeld, *loc. cit.*, pp. 230–231)

3.2 Causality, Hidden Variables, and Locality (1952–1967)

On 10 December 1954, Max Born received the Nobel Prize for Physics 'for his fundamental research in quantum mechanics, especially for his statistical interpretation of the wave function.' In his presentation speech, Ivar Waller, Chairman of the Nobel Committee for Physics, said: 'In contradiction to the deterministic predictions of the older mechanics, quantum mechanics accordingly poses laws which are of a statistical character, and as regards single phenomena will only determine the probabilities that one or another of various possibilities will occur,' and added:

> For material bodies of ordinary dimensions the uncertainty of the predictions of quantum mechanics is practically of no significance. But in atomic phenomena, on the other hand, it is fundamental. Such a radical break with older ideas could not of course prevail without opposition. But Born's conception is now generally accepted by physicists, with few exceptions. (Waller, in *Nobel Foundation*, ed., 1964, p. 254)

Actually, by the time Born was finally honoured with the Nobel Prize, some physicists of the younger generation had joined certain senior representatives of the physics community in opposing his statistical interpretation. Thus, in the *Scientific Papers Presented to Max Born*, David Bohm—like Louis de Broglie—raised new doubts (Bohm, 1954; L. de Broglie, 1954). As we have reported in Chapter II, Section II.6, Louis de Broglie had developed the idea of a 'double solution' of the Schrödinger equation (1927d, e). That is, besides the continuous wave function ψ, whose absolute square describes the probability of a particle, there should exist another *singular* solution u which describes the 'real' motion. It should have the form:

$$u(x, y, z, t) = f(x, y, z, t) \exp\left[\frac{i}{\hbar} S(x, y, z, t)\right], \tag{979}$$

where f (like ψ) satisfied the equation

$$U = -\frac{\hbar^2}{2m} \frac{\Box f}{f}, \tag{980}$$

with $\Box = \dfrac{\partial^2}{\partial x^2} + \dfrac{\partial^2}{\partial y^2} + \dfrac{\partial^2}{\partial z^2} - \dfrac{1}{c^2} \dfrac{\partial^2}{\partial t^2}$ and U the quantum-mechanical potential acting on a particle of mass m [see L. de Broglie, 1954, p. 24, Eq. (5)]. Then in the early 1950s, he and his student Jean-Paul Vigier revived the old ideas of the double solution (L. de Broglie, 1952a, b, c; 1953; Vigier, 1952). As de Broglie remarked: 'J. P. Vigier has pointed out to me the analogies existing between my considerations of 1927 on the double solution and Einstein's ideas of particles moving like field singularities in general relativity,' and added:

> Mr. Vigier thinks that one could also improve on the theory of the double solution with the ideas of Einstein, who has always sought to represent particles as singular regions of the field, perhaps also with the nonlinear electrodynamics of Mr. Born. Although one cannot yet give a definite judgment about the suggestions of Mr. Vigier, they allow one to grasp at some hope of seeing the theories of general relativity and quanta meet within the framework of the same representation, where causality will be reestablished. (L. de Broglie, 1954, p. 25)

In the article for the Born *Festschrift*, de Broglie also referred to the revival of his theory of the 'pilot wave' by David Bohm, but he thought that it was 'unacceptable in its present form' (L. de Broglie, *loc. cit.*, p. 22). He provided detailed reasons for this in Chapter 12 of the manuscript of a book, entitled '*On the Probabilistic Interpretation of Wave Mechanics and Various Related Questions*,' written in 1951–1952 and published posthumously:

> The theory of Bohm naturally runs into the same objections as mine [in 1927, see the discussion in our Chapter II.6], and they appear to me always insurmountable. As Bohm noted, the theory makes sense (in particular where the introduction of the quantum potential is concerned) only if the wave ψ is a "physical reality." But this is exactly what seems to me impossible to admit. First, the wave function ψ is represented

> by a function that is essentially complex, and, in the general case, it propagates through a space which is clearly abstract and fictitious, the configuration space. Already this makes it quite difficult to view ψ as a physical reality in the old sense of this phrase in classical mechanics. Furthermore, every localization experiment abruptly reduces the extension of the wave ψ in space and thereby changes its form (reduction of the wave packet), such that a measurement performed in one region of space will modify entirely the form of ψ in other regions remote from the former; and this fact, too, appears to argue against the characterization of ψ as a physical reality. (L. de Broglie, 1990, p. 178)

In contrast to Bohm, de Broglie was not sure that the 'pilot wave theory solves the difficulty pointed out by Einstein, Podolsky, and Rosen ... because after a collision the wave ψ is represented by a series of wave packets separated from one another in configuration space, and a localization within any one of these wave packets will cause the other one to vanish' (L. de Broglie, *loc. cit.*).

We will not embark upon the details of Bohm's 'causal theory' of the ψ-wave right here, but shift it slightly to below. We will also not go into de Broglie's alternative 'double solution' approach, which—unlike Bohm's theory—did not lead to an accomplished scheme that would contest the standard probabilistic quantum mechanics. However, we should add a few remarks about a joint paper of Bohm and Vigier dealing with a 'Model of the Causal Interpretation of Quantum Theory in Terms of a Fluid with Irregular Fluctuations' (Bohm and Vigier, 1954), which extended the old hydrodynamical model of Erwin Madelung (1926a, b).

Bohm and Vigier started from 'the assumption that an electron is a particle following a continuous and causally defined trajectory with a well-defined position $\xi(t)$, accompanied by a scalar field $\psi(\mathbf{x}, \mathrm{t})$' and added the following 'supplementary assumptions':

1. $\psi(\mathbf{x}, \mathrm{t})$ satisfied Schrödinger's equation.
2. $\dfrac{d\xi}{dt} = \nabla S/m$, where $\psi = R\exp(iS/\hbar)$.
3. The probability distribution in an ensemble of electrons, having the same wave function, is $P = |\psi|^2$. (Bohm and Vigier, 1954, p. 208)

After demonstrating the consistency of the above assumptions (and replying to Wolfgang Pauli's criticism, who claimed that the assumption 3 was not compatible with a causal theory), they adopted a generalization of Madelung's hydrodynamical model. The latter was described by the equations derived earlier by Bohm (1952a, with $\nabla = \mathrm{grad}$):

$$\frac{\partial R^2}{\partial t} + \mathrm{div}(R^2 \nabla S/m) = 0 \tag{981a}$$

and

$$\frac{\partial S}{\partial t} + \frac{(\nabla S)^2}{2m} + V_{\mathrm{qu}} + V_{\mathrm{cl}} = 0, \tag{981b}$$

with V_{cl} denoting the classical and V_{qu} the 'quantum potential,' namely,

$$V_{\mathrm{qu}} = -\frac{\hbar^2}{2m}\frac{\nabla^2 R}{R} = \frac{-\hbar^2}{4m}\left[\frac{\nabla^2 \rho}{\rho} - \frac{1}{2}\left(\frac{\nabla \rho}{\rho}\right)^2\right]. \tag{982}$$

Since Madelung's model failed to describe the actual location of a particle—which Bohm considered to be a necessary prerequisite for a causal interpretation of quantum theory—Bohm and Vigier completed the model by postulating particles in the fluid which have the form of a highly localized inhomogeneity moving with the local velocity $\mathbf{v}(x, t)$. Then, by introducing the hypothesis of a very irregular and effectively random fluctuation in the motion of the fluid, Bohm and Vigier were able 'to prove that an arbitrary probability density ultimately decays into $|\psi|^2$' (Bohm and Vigier, *loc. cit.*, p. 208) and to reject Pauli's criticism concerning assumption 3. They further succeeded in generalizing the result to spinor particles obeying the Dirac equation and to many-particle situations.

As we have mentioned earlier, besides the older physicists—who, like Einstein or de Broglie, either sympathized with or participated in the attempts at a more causal version of quantum theory—a number of younger theoreticians carried the burden of investigating the anti-probabilistic schemes in atomic theory. Especially David Bohm—who was born on 20 December 1917, in Wilkes Barre, Pennsylvania, and had studied physics at Caltech (1939–1941) and Berkeley (1941–1943), obtaining his Ph.D. with J. Robert Oppenheimer, and had contributed to war-related work on uranium separation—came out with the first and—as it turned out, the most discussed—proposal, 'A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables' (Bohm, 1952a, b). Later, because of his political views, he was not able to continue his scientific career in the United States and went in 1951 to the University of São Paulo in Brazil, and then in 1955 to Israel (to teach at the Technion in Haifa); he finally obtained positions in Great Britain.[1242] At Haifa, Bohm taught Yakir Aharonov as a graduate student; he took Aharonov with him to Great Britain, where they entered into a long-term fruitful collaboration.[1243] As the third important theoretician of the younger generation, John Stewart Bell, born on 28 July 1928 in Belfast, Ireland, must be

[1242] David Bohm first obtained a fellowship at the University of Bristol in 1957 and then moved in 1961 to a professorship of theoretical physics at Birkbeck College, London. He died on 27 October 1992, of a sudden heart attack.

[1243] Yakir Aharonov was born on 28 August 1932, and was educated at the Technion in Haifa (B.Sc. in 1956) and Bristol University (Ph.D. in 1960). From England, Aharonov went to the United States, taking up positions at Brandeis University (Research Associate, 1961–1964), Yeshiva University (Assistant Professor, 1964–1967; Associate Professor, 1967–1973), Boston University (Visiting Professor since 1973), and University of California at Berkeley (Miller Professor, 1991–1992). Since 1973, he occupied joint professorships at Tel Aviv University, Israel, and universities in the United States, especially the University of South Carolina.

mentioned.[1244] Since 1952, Bell became interested in the problem of the interpretation of quantum mechanics when he discussed with Franz Mandel at Harwell the work of David Bohm on hidden variables and also the old disproof of these by John von Neumann (Mandel supplied the knowledge of the German language, as von Neumann's book did not yet exist in English translation); however, he published the results of his own investigations on the interpretation of quantum mechanics only later, beginning in 1964. The work of Bohm, Aharonov, and Bell, and the various—often critical—responses offered by several theoreticians, largely determined the history of the interpretation of quantum mechanics since 1952. We shall now outline the main aspects of these lively debates.

(a) The Hidden Variables and von Neumann's Mathematical Disproof Revisited (1952–1963)

In 1951, David Bohm published the results of his several lecture courses given at Princeton University as a new book on *Quantum Theory*; in it, he attempted to present the material in the light of the Copenhagen point of view, as far as the interpretation was concerned, but he also displayed in detail the paradox of Einstein, Podolsky, and Rosen (Bohm, 1951, especially, Section 22.15). After finishing the book, Bohm felt dissatisfied with the Copenhagen view, and the conversations which he had with Einstein deepened his doubts in its correctness.[1245] In a set of papers, entitled 'A Suggested Interpretation of the Quantum Theory, etc.' (Bohm, 1952a, b), Bohm published what he called 'a consistent alternative interpretation,' whose characteristics he described as:

> In contrast to the usual interpretation, this alternative interpretation permits us to conceive of each individual system as being in a precisely definable state, whose changes with time are determined by definite laws, analogous to (but not identical with) the classical equations of motion. Quantum-mechanical probabilities are regarded (like their counterparts in classical statistical mechanics) as only a practical necessity and not as a manifestation of an inherent lack of complete determination in the properties of matter at the quantum level. As long as the present general form Schrödinger's equations is retained, the physical results obtained with our suggested alternative interpretation are precisely the same as those obtained with the usual interpretation. We shall see, however, that our alternative interpretation permits modifications of the mathematical formulation which could not even be described in terms of the usual interpretation. (Bohm, 1952a, p. 166)

[1244] John Stewart Bell was educated at Queen's University in Belfast (1948–1949) and at the University of Birmingham, where he obtained his doctorate with a thesis on the CPT-theorem under Rudolf Peierls. In between, he worked at the Atomic Energy Establishment at Harwell (1949–1960, first under Klaus Fuchs on reactor physics, and later on reactor design). In 1960, he joined the Theory Division at CERN, near Geneva. Bell died suddenly on 1 October 1990, at Geneva.

[1245] See Bohm's contribution to the Born *Festschrift*, 'A Discussion of Certain Remarks by Einstein on Born's Probability Interpretation of the ψ-function' (Bohm, 1954), referred to above.

That is, from the usual Schrödinger equation, Bohm derived exactly Eqs. (981a, b) containing the 'quantum potential,' Eq. (982), in his alternative interpretation. By modifications, he would mean the replacement of the equation of motion for a particle of mass m,

$$m\frac{d^2\mathbf{x}}{dt^2} = -\text{grad}\{V_{\text{cl}}(\mathbf{x}) + V_{\text{qu}}\}, \tag{983}$$

say, by the equation

$$m\frac{d^2\mathbf{x}}{dt^2} = -\text{grad}(V_{\text{cl}} + V_{\text{qu}}) + f(\mathbf{p} - \text{grad}\, S(\mathbf{x})), \tag{983$'$}$$

where $f(\mathbf{p} - \text{grad}\, S(\mathbf{x}))$ vanished for the momentum value $\mathbf{p} = \text{grad}\, S(\mathbf{x})$, such that f 'is large only in processes involving very short distances (where grad $S(\mathbf{x})$ should be large)' (Bohm, *loc. cit.*, p. 179). 'Evidence indicating the need for adopting our interpretation instead of the usual one could therefore come only from experiments, such as those involving phenomena associated with distances of 10^{-13} cm or less, which are not now adequately understood in terms of the existing theory,' Bohm concluded Part I of his 'Suggested Interpretation' (Bohm, *loc. cit.*).[1246]

In Part II of the investigation on hidden variables, David Bohm worked out the quantum theory of measurement in the new interpretation, in which 'the uncertainty principle is regarded, not as an inherent limitation of the precision with which we can correctly conceive the simultaneous definition of momentum and position, but rather as a practical limitation on the precision with which the quantities can be measured, arising from unpredictable and uncontrollable disturbances of the observed system by the measuring apparatus' (Bohm, 1952b, p. 180). This conclusion followed directly by using the Schrödinger equation and the conventional equation of motion for microscopic objects, such as Eq. (983) (for a particle of mass m). 'Hence, as long as we are restricted to making observations of this kind, the precise values of the particle position and momentum must, in general, be regarded as "hidden," since we cannot at present measure them,' Bohm concluded (Bohm, *loc. cit.*, p. 183). However, he considered the '"observables" of the usual interpretation not as a complete description,' because:

> This means that the measurement of an "observable" is not really a measurement of any physical property belonging to the observed system. Instead, the value of an "observable" measures only an incompletely predictable and controllable potentiality belonging just as much to the measuring apparatus as to the observed system itself. (Bohm, *loc. cit.*)

[1246] For a comparison of the new views of David Bohm with the standard complementarity views of Niels Bohr, see Cushing (1994b). A very detailed survey of old and new hidden variable theories has been given by Frederick J. Belinfante (1973).

Finally, Bohm analyzed von Neumann's famous demonstration 'that no single distribution of hidden parameters could be consistent with the results of quantum theory.' Bohm found it to be 'irrelevant here, since in our interpretation of measurements ... the distribution of hidden variables varies in accordance with the different mutually exclusive experimental arrangements of matter that must be used in making different kinds of experiments' (Bohm, *loc. cit.*, pp. 187–188).[1247]

Bohm's causal interpretation of quantum mechanics received many responses, both immediate and delayed. Evidently, the authors of the Copenhagen view raised 'fundamentally positivistic' and 'purely physical' objections. Heisenberg, in particular, criticized Bohm's interpretation in his comprehensive review for the Bohr *Festschrift*:

> This objective "description" ... reveals itself as a kind of "ideological superstructure," which has little to do with immediate reality; for the "hidden parameters" of Bohm's interpretation are of such a kind that they can *never* occur in the description of real processes, if the quantum theory remains unchanged. In order to escape this difficulty, Bohm in fact expresses the hope that in future experiments (e.g., in the range beyond 10^{-13} cm) the hidden parameters may yet play a physical part, and that the quantum theory may then be proved false.... In actual fact, the fulfilment of Bohm's hope would cut the ground from beneath not only the quantum theory, but also Bohm's interpretation. (Heisenberg, 1955, p. 18)

For the moment, Heisenberg felt strongly that quantum mechanics would stand correct; further, he argued that 'Bohm's language says nothing about physics that is different from what the Copenhagen language says,' hence 'there then remains only the question of the suitability of his language'; indeed, he found it to be unsuitable because it 'destroys the symmetry between p and q which is implicit in quantum theory' (Heisenberg, *loc. cit.*, p. 19).[1248]

Heisenberg was not the first of the old guard to criticize Bohm. While Bohm and Pauli were together at Princeton, and later by an exchange of letters, they discussed the new theory, which Pauli rejected totally, especially the involvement of hidden variables, which he considered nonsense.[1249] On the other hand, Bohm was able to satisfy one crucial demand of the standard interpretation of quantum

[1247] In case the equation of motion, (983), had to be replaced by the modified Eq. (983′), 'von Neumann's theorem is likewise irrelevant, this time because we are going beyond the assumption of the unlimited validity of the present general form of quantum theory, which plays an integral part in his proof,' Bohm concluded (Bohm, 1952b, p. 188).

[1248] Bohm had defended his preference of space-time in his theory, and even claimed: 'Common experience suggests that *absolute* canonical invariance is most implausible.' (Bohm, 1953c, p. 278) Earlier, he had also contradicted other causal interpretations that made use of the momentum representation (Bohm, 1953a).

[1249] Two letters of Bohm to Pauli exist in the published Pauli correspondence (Pauli, 1996, pp. 343–346); unfortunately, Pauli's letters with his particular objections have been lost. In letters to Markus Fierz, Pauli moreover accused Bohm of 'plagiarizing' Louis de Broglie's idea of pilot waves of 1927 (Pauli to Fierz, 10 January 1952, in Pauli, 1996, p. 505), a point of view shared by de Broglie himself, who always referred to Bohm with negative comments.

mechanics: the relation between the probability and the wave function remained essentially intact also in his approach (Bohm, 1953b). Notably, he found that 'as a result of random collisions, an arbitrary probability density P [in the hidden variable theory] will ultimately decay into one with a density $|\psi(x,t)|^2$,' although in the extensions suggested, say, in the domain below 10^{-13} cm 'there would be a tendency to create discrepancies between P and $|\psi|^2$, a tendency whose cumulative effects should be felt even at the atomic level.' He concluded: 'However, because those discrepancies have been shown to die out as a result of collisions, we can expect that under normal conditions the difference between P and $|\psi|^2$ would be negligible.' (Bohm, *loc. cit.*, p. 458) Still, he did not exclude experimental situations, where the difference might show up. Therefore, Hans Freistadt of Newark College of Engineering, New Jersey, praised Bohm's causal theory as being 'more flexible than the usual formulation, with which one may attack problems in which the usual formulation has failed' (Freistadt, 1957, p. 3), while Vladimir Fock refuted it as contradicting the essential peculiarities of quantum phenomena (see, e.g., Fock, 1958).[1250]

From the mathematical side, several authors felt stimulated to investigate more closely von Neumann's disproof of hidden variables of 1932. Gerhard Schulz of Adlershof, for example, found that von Neumann had restricted himself just to 'experimentally accessible subensembles,' but others existed to be considered as well (Schultz, 1959). However, several years later, Joseph M. Jauch and Constantin Piron of Geneva reformulated von Neumann's disproof in the modern logical language of an 'ortho-complemented lattice,' i.e., in the lattice-theoretical language of quantum logic (Jauch and Piron, 1963).[1251] By applying this sophisticated tool, they arrived at the 'reduction of the question concerning the hidden variables to an empirical one,' since 'the lattice operations have a physical interpretation which is accessible to empirical verification'; the new question was, 'whether there exist [logical] propositions which are not compatible' (Jauch and Piron, *loc. cit.*, p. 836). Jauch and Piron immediately continued to argue as follows:

> To rule out hidden variables it suffices to exhibit two propositions of a physical system which are not compatible. It turns out that this is quite easy. In fact, the occurrence of incompatible propositions leads to gross macroscopic effects which can easily be verified. With this result, the possible existence of hidden variables is decided in the negative. (Jauch and Piron, *loc. cit.*, p. 837)

Actually, they showed that if a propositional lattice contained 'dispersion-free states,' as required by the hidden-variable theory, the system involved incompatible propositions.

[1250] For more details of the early debate on Bohm's hidden variable theory, we refer to Jammer, 1974, pp. 289–302.

[1251] For a review of Jauch and Piron's demonstration and the fundamental paper of Andrew M. Gleason of Harvard University, upon which it was based (Gleason, 1957), see Jammer, 1974, pp. 296–302. A simplified version of the ortho-complemented lattice approach was provided by Piron (1972).

Still, the situation did not turn out to be that easy. Soon a new major participant, John Stewart Bell, would come upon the scene and demonstrate, by explicit construction, that a hidden-variable description free from contradictions might exist for certain quantum-theoretical situations. The partisans of David Bohm's theory again became optimistic in expecting a final proof in favour of the causal interpretation of quantum mechanics.

(b) The EPR Paradox Revisited, Bell's Inequalities, and Another Return to Hidden Variables (1957–1968)

In Part II, Section 8, of his pioneering investigation on hidden variables, David Bohm discussed 'The Hypothetical Experiment of Einstein, Podolsky and Rosen,' first from the point of view of Niels Bohr (who had declared that the correlation between the properties of the two particles involved should not be analyzed in a particle model with local interaction), and then from his own different point of view. He arrived at the following conclusion:

> In our suggested new interpretation of the quantum theory, however, we can describe this experiment in terms of a single precisely definable model.... If we measure the position of the first particle, we introduce uncontrollable fluctuations in the wave function of the entire system, which, through the "quantum-mechanical forces" [V_{qu}] bring about corresponding uncontrollable fluctuations in the momentum of each particle. Similarly, if we measure momentum of the first particle, uncontrollable fluctuations in the wave function for the system bring about, through the "quantum-mechanical forces," corresponding uncontrollable changes in the position of each particle. Thus the "quantum-mechanical" forces may be said to transmit uncontrollable disturbances instantaneously from one particle to another through the medium of the ψ-field. (Bohm, 1952b, p. 186)

Evidently, this answer shared the gross nature of Bohr's view, as it did not state in detail how the theory had to be applied to the experiment proposed by Einstein *et al.* (1935).

However, five years later, Bohm published—together with his Israeli student Yakir Aharonov—a deeper analysis of the *EPR*-problem. In particular, they studied a simplified version of the two-body situation; i.e., they considered a molecule consisting of two atoms, each having spin $\frac{1}{2}\hbar$. The system was then described by the wave function

$$\psi = \frac{1}{\sqrt{2}}[\psi_+(1)\psi_-(2) - \psi_-(1)\psi_+(2)], \tag{984}$$

with $\psi_+(1)$ referring to spin $+\frac{1}{2}\hbar$ for particle A, $\psi_-(2)$ to spin $-\frac{1}{2}\hbar$ for particle B, etc. (Bohm and Aharonov, 1957, p. 1070). After the separation of the atoms without changing the spin status, a measurement of the spin of A would clearly determine the spin of B as being opposite. In quantum theory, then, a difficulty could arise, since only one component of the spin of each particle should assume a definite value at a given time. But, according to Bohr, 'the observing apparatus plus what is observed form a single indivisible combined system not capable at the quantum level of being analyzed correctly into separate and distinct parts'; hence, the *EPR* paradox disappeared (Bohm and Aharonov, *loc. cit.*, p. 1072). Yet, a 'deeper exploration' existed with the help of the causal hidden-variable theory of Bohm which claimed as a fact 'that this combined system is at least conceptually analyzable into components which satisfy appropriate laws'—although Bohm and Aharonov admitted that the quantum potential V_{qu} responsible for the causal model explanation 'seems rather artificial in form, besides being subject to the criterion that it implies instantaneous interaction between distant particles, so it is not consistent with the theory of relativity' (Bohm and Aharonov, *loc. cit.*). However, thus far, evidence spoke against the strictly local forces, as they pointed to already performed experiments, which rather seemed to exhibit the presence of quite strange kinds of correlation in the properties of distant entities. They referred here to an investigation with polarized photons, carried out by Chien-Shung Wu and Irving Shaknov of Columbia University, who had observed in 1949 'The Angular Correlation of Scattered Annihilation Radiation' and found an asymmetry in the ratio

$$\frac{\text{coincidence counting rate } \perp}{\text{coincidence counting rate } \|} = 2.04 \pm 0.08 \tag{985}$$

(Wu and Shaknov, 1950, p. 136). Bohm and Aharonov now calculated the above ratio on the basis of two different hypotheses: first, the usual quantum mechanics is correct in all cases; second, the usual quantum theory is correct only when the wave functions of the photons overlap (this would rather be consistent with the causal theory of Bohm). In the first case, they obtained the value 2.00, while in the second case (depending on further assumptions), values between 1 and 1.5; hence, they concluded:

> The results … show that this experiment is explained adequately by the current quantum theory which implies distant correlations, of the type leading to the paradox of *ERP* [Bohm an Aharonov always cited Rosen before Podolsky!], but not by any reasonable hypothesis implying a breakdown of the quantum theory that could avoid the paradox of *ERP*. (Bohm and Aharonov, 1957, p. 1075)[1252]

In November 1964, John Bell joined the considerations 'On the Einstein Podolsky Rosen Paradox'. He involved in the description of the situation a

[1252] In a further paper, Bohm and Aharonov displayed the evaluation of the Wu–Shaknov experiment in more detail, in order to reply to certain objections that had been raised. Simultaneously, they demonstrated that the way out of the paradox shown earlier by Wendell Furry (1936a, b)—who had assumed that the many-body Schrödinger equation broke down for macroscopic distances—contradicted the outcome of the Wu–Shaknov experiment (Bohm and Aharonov, 1960, especially, p. 975).

parameter λ, denoting either 'a single variable or a set of variables, or even a set of functions,' the parameter assuming discrete or continuous values (Bell, 1964, P. 195). In the particular case of the Bohm–Aharonov example and a continuous parameter λ, he found:

> The result A of measuring $\boldsymbol{\sigma}_1 \cdot \mathbf{a}$ is then determined by $\mathbf{a}$ and λ, and the result B of measuring $\boldsymbol{\sigma}_2 \cdot \mathbf{b}$ in the same instance is determined by $\mathbf{b}$ and λ, and
>
> $$A(\mathbf{a}, \lambda) = \pm 1, B(\mathbf{b}, \lambda) = \pm 1. \qquad [(986)]$$
>
> The vital assumption [of causality and locality] is that the result B for particle 2 does not depend on the setting $\mathbf{a}$ of the magnet for particle 1, nor A on $\mathbf{b}$.
>
> If $\rho(\lambda)$ is the probability distribution of λ then the expectation value of the product of the two components $\boldsymbol{\sigma}_1 \cdot \mathbf{a}$ and $\boldsymbol{\sigma}_2 \cdot \mathbf{b}$ is
>
> $$P(\mathbf{a}, \mathbf{b}) = \int d\lambda \rho(\lambda) A(\mathbf{a}, \lambda) B(\mathbf{b}, \lambda). \qquad [(987)]$$
>
> This should be equal to the quantum-mechanical expectation value, which for the singlet state [for the Bohm-Aharonov molecule] is
>
> $$\langle \boldsymbol{\sigma}_1 \cdot \mathbf{a}\, \boldsymbol{\sigma}_2 \cdot \mathbf{b} \rangle = -\mathbf{a} \cdot \mathbf{b}. \qquad [(988)]$$
>
> But it will be shown that this is not possible. (Bell, *loc. cit.*, pp. 195–196)

Bell proved the last statement by arguing as follows: $P(\mathbf{a}, \mathbf{b})$—Eq. [(987)]—could be written as

$$P(\mathbf{a}, \mathbf{b}) = -\int d\lambda \rho(\lambda) A(\mathbf{a}, \lambda) B(\mathbf{b}, \lambda), \qquad (987')$$

and if $\mathbf{c}$ were another unit vector, the inequality

$$1 + P(\mathbf{b}, \mathbf{c}) \geq |P(\mathbf{a}, \mathbf{b}) - P(\mathbf{a}, \mathbf{c})| \qquad (989)$$

might be derived. This evidently meant: Unless P was constant, the right-hand side for small $(\mathbf{b} - \mathbf{c})$ would have to be of the order of $(\mathbf{b} - \mathbf{c})$; hence, Bell arrived at the result: '$P(\mathbf{b}, \mathbf{c})$ cannot be stationary at the minimum value (-1 at $\mathbf{b} = \mathbf{c}$) and cannot equal the quantum-mechanical value [(988)]' (Bell, *loc. cit.*, p. 198).' And he further demonstrated that the quantum-mechanical correlation, Eq. [(988)], cannot be approximated arbitrarily closely by the form [(987)]; thus, he finally stated the important general conclusion:

> In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the

> reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant. (Bell, *loc. cit.*, p. 199)

Bell's inequality (989), or its generalization to various other situations (Bell, 1971)—for example, to the polarization correlation of a pair of optical photons [see Clauser, Horne, Shimony, and Holt, 1969, p. 881, Eq. 1(a)]—

$$|P(a,b) - P(a,c)| \leq 2 - P(b',b) - P(b',c), \tag{990}$$

where $P(a,b)$ represented a correlation function depending on parameters a and b of the two apparatuses I_a and I_b through which the first and second particle move, respectively—with each apparatus selecting one of the two channels $+1$ and -1 (i.e., $A(a)$ and $B(b)$ equal ± 1)—would determine the experimental search for proving or disproving hidden variables in atomic theory, as we shall report in Subsection 3.3.

John Bell had thought about the problem of hidden variables and von Neumann's verdict already since his student years. In summer 1964, while (on leave of absence from CERN) at SLAC in Stanford, he submitted a paper 'On the Problem of Hidden Variables in Quantum Mechanics' to *Reviews of Modern Physics*, which (after long editorial delays) would be published only in the July 1966 issue (Bell, 1966), together with two other papers of David Bohm and Jeffrey Bub (1966a, b). Bell first stated the task to have been accomplished quite clearly, namely:

> The question at issue is whether the quantum-mechanical states can be regarded as ensembles of states further specified by additional variables, such that given values of these variables together with the state vector determine precisely the results of individual measurements. These hypothetical well-defined states are said to be "dispersion free." (Bell, *loc. cit.*, p. 448)

The answer to the question, how to obtain these dispersion-free states—which obviously satisfied the requirement of having deterministic properties—Bell demonstrated in a simplified quantum-mechanical example, a system of (spin-$\frac{1}{2}$) particles having no translational motion. The state in such a system would then be represented by a two-component spinor ψ, and the observables by the expression

$$\mathcal{O} = \alpha + \boldsymbol{\beta} \cdot \boldsymbol{\sigma}, \tag{991}$$

where α denoted a real number multiplied by the (2×2) unit matrix, $\boldsymbol{\beta}$ denoted a real vector, and $\boldsymbol{\sigma}$ denoted the vector composed by the three Pauli spin-matrices. The measurement of the observable $\mathcal{O}$ then yielded one of the eigenvalues $\boldsymbol{\alpha} \pm |\boldsymbol{\beta}|$, with the relative probability given by the expectation values

$$\langle \boldsymbol{\alpha} + \boldsymbol{\beta} \cdot \boldsymbol{\sigma} \rangle = \int \psi^* (\boldsymbol{\alpha} + \boldsymbol{\beta} \cdot \boldsymbol{\sigma}) \psi \, d\tau = (\psi, [\boldsymbol{\alpha} + \boldsymbol{\beta} \cdot \boldsymbol{\sigma}]\psi). \tag{992}$$

Now, Bell constructed a hidden-variable system by associating with each ψ a real parameter λ, chosen from the interval $[-\frac{1}{2}, +\frac{1}{2}]$ such that the measurement of $\mathcal{O}$, Eq. (991), depended on ψ *and* the value of λ. He further took the quantum state as defined by a uniform averaging over λ in the interval and derived

$$\langle \boldsymbol{\alpha} + \boldsymbol{\beta} \cdot \boldsymbol{\sigma} \rangle = \alpha + \beta_z; \tag{993}$$

i.e., he obtained with *certainty* a particular expectation value of the system under investigation. Though John Bell had to admit that he could not attribute any physical significance to his parameter λ, he had shown 'that at the level considered by von Neumann such a reinterpretation is not excluded' (Bell, *loc. cit.*, p. 448). Hence, he succeeded in escaping from the verdict on hidden variables. His result would not contradict the conclusions of Jauch and Piron, because the latter had based their disproof of hidden variables on a mathematical theorem of Gleason (1957), which applied only to Hilbert spaces having dimensions higher than two—while the Hilbert space in Bell's special example possessed just two dimensions.

Jauch and Piron's claim also stimulated David Bohm to reconsider the hidden-variable question. Together with Jeffrey Bub, a graduate student at Birkbeck College from South Africa, he reviewed again carefully the principles of quantum mechanics and the standard interpretation and argued again that it would be 'more natural' to introduce 'new kinds of variables, at present "hidden" but in principle ultimately observable with the aid of suitable methods of observations,' because:

> The Copenhagen interpretation of quantum mechanics, in which the collapse of the wave packet is accepted as a fundamental and irreducible phenomenon..., entails the renunciation of any conception of the order and structure of movement of a microsystem in favor of a set of rules for the prediction of the results of specific experiments. (Bohm and Bub, 1966a, p. 457)

Bohm and Bub rather insisted on the point that 'science also aims at an *understanding* of the overall structure and order of movement of matter from the atoms to the galaxies' (Bohm and Bub, *loc. cit.*). Indeed, Bohm and his followers desired more than just predictions from a physical theory; they expected nothing less than 'the whole of the act of understanding, in which one grasps the order, the structure of a complex process in a unified coherent set of concepts' (Bohm and Bub, *loc. cit.*). Thus, Bohm and Bub argued that refined analyses of the measurement process, as provided, for instance, by Günther Ludwig or Danieri *et al.* (as discussed earlier), would not solve the problem since 'the order and structure of the process by which a microsystem such as an electron is measured cannot be conceived within the formalism of quantum mechanics alone' (Bohm and, Bub, *loc. cit.*, pp. 459–460). In a hidden-variable theory, on the other hand, one could easily escape from von Neumann's conclusions by generalizing the linear relation between the measured value $\bar{R}$ of a variable R,

$$R = \sum_{m,n} U_{n,m} R_{mn}, \tag{994}$$

—with U_{nm} the matrix defining the statistics of the quantum-mechanical ensemble under investigation—to the nonlinear relation

$$\overline{R'} = \int F(\psi, \lambda, R_{mn})\rho(\lambda)\,d\lambda, \qquad (994')$$

where F represented a nonlinear function of the wave function ψ, the hidden-variable parameter λ, and the density function $\rho(\lambda)$ of the hidden variables. As a consequence of the *Ansatz* (994′), the crucial assumption in von Neumann's disproof, namely, the linear equation

$$(aR + bS + \ldots)_{\text{average}} = aR + bS + \ldots, \qquad (995)$$

involving variables R, $S, \ldots$ and real numbers a, $b, \ldots$, would break down, and with that the hidden variables were admitted. In their subsequent paper in *Reviews of Modern Physics*, Bohm and Bub turned to analyze the new treatment of Jauch and Piron, who had not used the linear relation (995); still, they discovered also a loophole in this work, namely:

> The conclusions of Jauch and Piron ... are indeed seen to follow from a false assumption; i.e., that the impossibility of propositions that describe simultaneously the results of two noncommuting observables is an "empirical fact." Actually, it is shown that this assumption follows if and only if one first assumes what the authors set out to prove; i.e., that the current liguistic structure of quantum mechanics is the only one that can be used correctly to describe the empirical facts underlying the theory. (Bohm and Bub, 1966b, p. 470)

Jauch and Piron, in a letter to the editor of the journal, vehemently contradicted that argument which, in their opinion, rested on the misunderstandings of Bohm and Bub. In addition, they emphasized: 'It is contrary to good scientific methodology to modify a generally verified scientific theory for the sole purpose of accommodating hidden variables.' (Jauch and Piron, 1968, p. 229) While two mathematicians of the University of Wisconsin, Stanley P. Gudder and Jerald H. Tutsch, took opposite stands on the problem (also in letters to the journal), Bohm and Bub replied to Jauch and Piron by saying simply:

> The basic question at issue here is a point often overlooked: i.e., that the axioms of a theory stand on a different level from the experimental facts underlying the theory. It is therefore wrong to equate any set of axioms whatsoever with facts in the way that is done by Jauch and Piron (as well as by von Neumann). Rather, axioms are always assumptions from which one draws *inferences* about what is factually observable. If these inferences agree with the facts, the assumed structure of axioms is confirmed; and if they disagree, it is refuted. But if the axiomatic structure is confirmed by the facts available at a given time, this can never imply that no other axiomatic structure is possible that could agree with the same set of facts. (Bohm and Bub, 1968, p. 235)

With all of the mathematical and conceptual argumentations given, each of which contained some part of the truth, the decision in favour or against hidden variables was postponed. In fact, neither mathematical nor conceptual tools sufficed, but new experiments would finally settle the case, at least for the moment.

(c) The Aharonov–Bohm Effect (1959–1963)

While the experiments of the 1960s did not provide much encouragement to Bohm and his partisans in substantiating their ideas on hidden variables, they confirmed an effect which seemed to point in the direction envisaged. In May 1959, Aharonov and Bohm submitted a paper on the 'Significance of Electromagnetic Potentials in the Quantum Theory' to *Physical Review*, in which they pointed out that while in classical electrodynamics 'the fundamental equations of motion can always be expressed directly in terms of fields [which are derivatives of the electromagnetic potentials] alone ... in quantum mechanics, [where] the canonical formalism is necessary..., the potentials cannot be eliminated from the basic equations' (Aharonov and Bohm, 1959, p. 485). To prove this claim, the authors considered the case of an electron beam split into two parts, such that each passed through a long tube on an equivalent path, and the parts were later combined again. Now, if a vector potential **A** created by a solenoid placed in the centre of the two paths (between the two tubes) acted on the electron, an interference effect would arise due to a phase difference

$$\frac{\Delta S}{\hbar} = \frac{e}{\hbar c}\Phi_0, \tag{996}$$

where

$$\Phi_0 = \int \mathbf{A} \cdot d\mathbf{x} \tag{996a}$$

constituted the total flux (through every circuit containing the origin) outside the solenoid. Hence, Aharonov and Bohm derived as the 'essential result'

> that in quantum theory, an electron (for example) can be influenced by the potentials even if all the field regions are excluded from it. In other words, in a field-free multiply connected region of space, the physical properties of the system still depend on the potentials.... It would therefore seem natural at this point to propose that, in quantum mechanics, the fundamental entities are the potentials, while the fields are derived from them by differentiations. (Aharonov and Bohm, *loc. cit.*, p. 490)

This particular property emerged from the very structure of the fundamental equations in quantum mechanics, i.e., the Schrödinger equation and the Dirac equation, in which the potentials entered rather than the fields—and it distinguished quantum mechanics from classical dynamics, in which the Lorentz force $\left[e\mathbf{E} + \left(\frac{e}{v}\right)\mathbf{v} \times \mathbf{H}\right]$, depending only on the electric and magnetic fields, **E** and **H** (with **v** the velocity of the electron) entered. Evidently, this fact revealed, as Aharonov and Bohm concluded, 'an additional richness' and suggested that 'some further development of the theory is needed,' proposing in particular two directions, namely:

> First, we may try to formulate a nonlocal theory in which, for example, the electron could interact with a field that was a finite distance away. Then there would be no trouble in interpreting these results, but, as is well known, there are several difficulties in the way of doing this. Secondly, we may retain the present local theory and, instead, we may try to give a further new interpretation to the potential. In other words, we are led to regard $A_\mu(x)$ as a physical variable. This means that we must be able to define the physical difference between two quantum states which differ only by a gauge transformation. (Aharonov and Bohm, *loc. cit.*, pp. 490–491)

In a paper submitted after nearly two years, they analyzed the quantum-mechanical situation further, arriving at the definite conclusion:

> We see then that whether we treat the potentials as specified functions of space and time, or as dynamical variables furnishing a link between the sources and the electron, there is no way in the quantum theory to express the effect of a flux inside the solenoid on an electron outside in terms of a *localized* interaction, except with the aid of potentials. In no case does the theory ever contain any kind of interaction between the electron an the source, which does not go through the intermediary of potentials and, as we have seen, fields are not, in general, adequate for expressing all aspects of this intermediary role. (Aharonov and Bohm, 1961, p. 1522)

Meanwhile, several experimental investigations had confirmed the predicted dependence of the electron on electromagnetic potentials, the so-called 'Aharonov–Bohm effect.'[1253] Certain theoreticians discussed the possibility of avoiding the explicit use of potentials in quantum mechanics. Notably, Bryce DeWitt (1961) and Frederick Belinfante (1962) argued that the potentials did not belong to the 'observables' or Hermitean operators, whose eigenfunctions form a complete basis for the wave functions. Concerning this objection, Aharonov and Bohm answered simply that in quantum mechanics, waves and particles were also 'not observable' though they still made sense (Aharonov and Bohm, 1963, p. 1628). In a detailed discussion of gauge invariance and localizability—in fact, gauge invariance seemed to remove the electromagnetic potentials in favour of fields—they re-emphasized their previous conclusion 'to regard the potentials as *clues* to some new features of space, time and properties of charge'; in particular, they optimis-

[1253] Aharonov and Bohm referred in 1961, e.g., to the work of F. G. Werner and D. R. Brill (1960) and R. G. Chambers (1960). Of the later work, see Al'tschuler, Aronov, and Spivak (1981), who demonstrated an effect in disordered conductors, obtaining half the value of the ordinary Aharonov–Bohm phase.

tically hoped to 'perhaps obtain insights into the reasons why potentials seem to be "essential" for expressing the property of *localizability* in a simple and natural way' (Aharonov and Bohm, *loc. cit.*, p. 1632).[1254]

3.3 Further Interpretations and Experimental Confirmation of the Standard Quantum Mechanics (1957–1999)

The Course No. 49 of the 'International School of Physics "Enrico Fermi,"' held in Varenna, Italy, from 29 June to 11 July 1970, was devoted to the topic 'Foundations of Quantum Mechanics,' and a number of prominent representatives of the different views concerning the interpretation of quantum mechanics participated in it. Among these participants were Joseph M. Jauch, Günther Ludwig, G. M. Prosperi, and Eugene P. Wigner, of one interpretation camp, and Louis de Broglie, John Bell, and David Bohm, of the other. Bryce DeWitt of the University of North Carolina spoke about still another interpretation, which he called the 'Many-Universes Interpretation of Quantum Mechanics' (1971). In fact, quite a large number of physicists became active participants—during the last three decades of the twentieth century—in the field of interpretation of quantum mechanics, with perhaps the most important progress being achieved by experimentalists, as increasingly refined methods became available to follow the behaviour of individual microscopic particles and to analyze the occurrences in the various stages of the measurement process.[1255] In the following, we shall report about three aspects: first, the many-universes (worlds) interpretation and others; second, the experiments to test Bell's inequalities for a number of *EPR*-like experiments; and third, the most recent experiments establishing the details of the standard (orthodox) interpretation of quantum mechanics.

(a) The Many-World Interpretation and Other Proposals (1957–1973)

'The preceding paper puts the principles of quantum mechanics in a new form,' thus John Archibald Wheeler began a comment on the publication of his student Hugh Everett in issue No. 3 of *Review of Modern Physics*, published in July 1957, and briefly explained its contents as follows:

> Observations are treated as a special case of normal interactions that occur within the system, not as a new and different kind of process that takes place from without. The

[1254] In fact, it had been known for some time that quantum electrodynamics could be formulated in a formally local, covariant manner involving, however, the use of an indefinite metric in the Hilbert space (Gupta, 1951). It was the latter condition which did not fit into the concept of observables advocated by Belinfante and DeWitt, but Aharonov and Bohm did not bother too much, as the 'current physical and mathematical conceptions of quantum theory' might later have to be extended anyway (Aharonov and Bohm, 1963, p. 1630).

[1255] The 'Resource Letter IQM-2 on "Foundations of Quantum Mechanics since the Bell Inequalities"' (Ballentine, 1987) lists 140 items.

> conventional mathematical formulation with its well-known postulates about probabilities of observations is derived as a *consequence* of the new "*meta*" quantum mechanics.... In a new or "relative state formalism" this model associates with an isolated system a state function that obeys a linear equation. The theory deals with the totality of all possible ways in which this state function can be decomposed into the sum of products of state functions for subsystems of the overall system—and nothing more. (Wheeler, 1957, p. 463)

In the introduction of his paper, which he entitled '"Relative State" Formulation of Quantum Mechanics,' Everett himself characterized the goal and nature of his approach as follows:

> The aim is not to deny or contradict the conventional formulation of quantum theory ... but rather to supply a new, more general and complete formulation, from which the conventional interpretation can be *deduced*.
>
> The relationship of this new formulation to the older formulation is therefore that of a metatheory to a theory, that is, it is an underlying theory in which the nature and consistency, as well as the realm of applicability, of the older theory can be investigated and clarified. (Everett, 1957, p. 454)

This meant, especially, that he took over the standard scheme of quantum mechanics, but for the special postulates dealing with the formulation of observations, because he wanted to avoid the well-known difficulties and partly continuous and partly discontinuous changes in the theory. In fact, Everett and Wheeler were interested in an even more ambitious problem, namely, to quantize general relativity and to develop the corresponding 'interpretation of quantum mechanics when applied to so fundamental a structure as the space-time geometry itself' (Everett, *loc. cit.*, p. 454). In a closed universe, for example, one could not think of an outside observer (as in the standard quantum mechanics), since 'there is nothing outside it to produce transitions from one state to the other,' but rather a '*quantum mechanics* that is *internal to an isolated system*' (Everett, *loc. cit.*, p. 455).

In order to accomplish this task, Everett took as a first step the wave function 'as the basic entity with *no a priori interpretation*' and formulated 'abstract models for observers that can be treated within the theory itself as physical systems,' in order 'to consider isolated systems containing such model observers in interaction with other subsystems, to deduce the chances that occur in an observer as a consequence of the interaction with the surrounding subsystems, and to interpret the changes in the familiar language of experience' (Everett, *loc. cit.*). Mathematically, he introduced the concept of a 'relative state'; i.e., he assigned for any choice of a state ξ^{S_1} in one subsystem (S_1) uniquely a corresponding state ψ in another system S_2; thus,

$$\psi(S_2; \mathrm{rel}\ \xi_k S_1) = N_k \sum_j a_{kj} \eta_j^{S_2}, \tag{997}$$

with N_k a normalization constant and $\eta_j^{S_2}$ a complete orthogonal set of states in S_2. Consequently, '*it is meaningless to ask the absolute state of a subsystem—one can only ask the state relative to a given state of the remainder of the subsystem*' (Everett, *loc. cit.*, p. 456), and he concluded in particular about the measuring process the following:

> Von Neumann's example is only a special case of a more general situation. Consider any measuring apparatus interacting with any object system. As a result of the interaction the state of the measuring apparatus is no longer capable of independent definition. It can be defined only *relative* to a state of the object system. In other words, there exists only a correlation between the states of the two systems. (Everett, *loc. cit.*, p. 457)

A measurement in the new metatheory, therefore, required—as shown by Everett in a second step—the definition of an appropriate observer, e.g., an 'automatically functioning machine, possessing sensory apparatus and coupled to recording devices capable of registering past sensory data and machine configurations', thus 'its present actions shall be determined not only by its present sensory data, but by the contents of the memory as well' (Everett, *loc. cit.*). These observers would then be able, as Everett showed, to perform 'good' observations, in which the eigenstates of the systems remain unaltered, while the state of the observer changes in dependence on the different eigenfunctions. Everett derived two specific rules for the transformation of the states of an observed system and the total system, respectively (Everett, *loc. cit.*, p. 458). He finally arrived at the following picture of the process of measurement:

> Throughout all of a sequence of observation processes there is only one physical system representing the observer, yet there is no single unique *state* of the observer (which follows from the representations of interacting systems). Nevertheless there is a representation in terms of a *superposition*, each element of which contains a definite observer state and a corresponding system state. Thus with each succeeding observation (or interaction) the observer state "branches" into a number of different states. Each branch represents a different outcome of the measurement and the *corresponding* eigenstate for the object-system state. All branches exist simultaneously in the superposition after any given sequence of observations. (Everett, *loc. cit.*, p. 459)

In this manner, a 'trajectory' of the memory configuration of an observer performing a sequence of measurements would arise, corresponding to 'a branching tree, with all possible outcomes existing simultaneously in a final superposition with various coefficients in a mathematical model,' and all that remained to do was to determine the coefficients, i.e., the weighting of the elements in the final superposition (Everett, *loc. cit.*, p. 460). Everett calculated those in accordance with the additivity requirement of the superposition of quantum states—which also agreed with the weights given in the standard quantum-mechanical theory of discontinuous changes. He finally arrived at the conclusion that the new theory 'based on pure wave mechanics is a conceptually simple, causal theory, which gives predictions in accord with experience,' provides 'a framework in which one can investigate in detail, mathematically, and in a logically consistent manner a number of sometimes puzzling subjects' (like the measuring process and the *EPR* paradox) and 'ultimately justifies the use of the probabilistic interpretation as an aid to making practical predictions'; that is 'it forms a broader frame in which to understand the consistency of that interpretation ... a *metatheory* for the standard theory.' And finally, he argued that 'it may prove a fruitful framework for the quantization of general relativity.' (Everett, *loc. cit.*, p. 462)

In spite of great promises made, as reported above, not much happened with Everett's 'relative state' formulation of quantum mechanics until Bryce DeWitt picked it up in the 1960s. As he reported in a later essay published in *Physics Today*:

> What if we assert that the formalism [of quantum mechanics] is all, that nothing else is needed [in order to solve the measurement problem]. The answer is we can. The proof of the assertion was given in 1957 by Hugh Everett with encouragement of John Wheeler and has been subsequently elaborated by R. Neill Graham [in his Ph.D. thesis under the supervision of DeWitt]. It constitutes the third way of getting out of the crisis posed by the catastrophe of the infinite regression [besides the hidden-variable theory of David Bohm and von Neumann's collapse of the wave packet]. (DeWitt, 1970, p. 33)

According to DeWitt, 'Everett, Wheeler and Graham postulate that the real world or any isolated part of it one may wish for the moment to regard as *the* world is faithfully represented by the following mathematical objects: a vector in Hilbert space; a set of dynamical equations for a set of operators that act on the Hilbert space, and a set of commutation relations for the operators'; to these they added a 'postulate of complexity,' namely: 'The world must be sufficiently complicated that it may be decomposable into systems and apparatuses.' Thus, they succeeded in proving in particular: 'The mathematical formalism of the quantum theory is capable of yielding its own interpretation.' (DeWitt, *loc. cit.*)

In the interpretation pioneered by Hugh Everett, many universes were introduced, which might appear as a kind of horror vision to the scientist. However, this splitting of the atomic world into a large number of copies would not have consequences for the observer, as DeWitt continued in his article:

> The state vector at the end of the coupling interval [where the system and apparatus are treated together] again takes the form of a linear superposition of vectors, each of which represents the system observable as having assumed one of its possible values. Although the value varies from one element of the superposition to another, not only do two apparatuses within a given element observe the value appropriate to the element, but also, by straightforward communication, they agree that the results of the observation are identical. The splitting into branches is thus unobserved. (DeWitt, *loc. cit.*, p. 33)

DeWitt therefore advocated the 'many world' or 'many universes' interpretation in numerous talks, notably at the Varenna Summer School of 1970, where he defined more closely the meaning of 'system' and 'apparatus' by presenting detailed examples (DeWitt, 1971). However, several objections were raised against this approach in a set of letters, published in the April 1971 issue of *Physics Today*; for example, Leslie E. Ballentine pointed out that 'DeWitt's claim that the formalism by itself can generate an interpretation is misleading' because it rests on the assumption 'that the state vector provides a direct picture of the world (a world of many noninteracting branches), rather than, say, a statistical representation of an ensemble' (Ballentine, 1971, p. 37), and Joseph Gerver rejected 'DeWitt's statement that in a finite universe there are only a finite number of independent "realities"' (Gerver, 1971, p. 40). In a subsequent, detailed paper, entitled 'Can the Statistical Postulate of Quantum Theory be Derived? A Critique of the Many-Universes Interpretation' and published in *Foundations of Physics*, Ballentine argued in particular that the 'branches' of the universe in that interpretation depended upon the choice of representation and therefore became ambiguous, and concluded: 'Although the notion of a world splitting into many independent copies of itself seems fantastic, ... [it] is neither necessary nor sufficient for the derivation of the statistical postulate of quantum theory.' (Ballentine, 1973, p. 239)[1256]

As we have mentioned, the number of publications on the problem of the interpretation of quantum mechanics increased enormously since the late 1960s. Their contents ranged from discussing more cautiously the preparation of the initial state of systems in a given experiment (see Lamb, 1969: 'An Operational Interpretation of Nonrelativistic Quantum Mechanics'[1257]) to a particular two-vector reformulation of quantum mechanics, in which a violation of Lorentz

[1256] Indeed, it appears to be very difficult to derive the statistical behaviour of a quantum system without introducing any statistical hypothesis. One might, for example, think of Richard Feynman's path-integral method, who took all classical paths of a system into consideration; still, the probabilities for the individual path followed, not from classical dynamical considerations, but from quantum rules.

[1257] Since 1969, Willis E. Lamb, Jr., has consistently devoted much effort and many papers to the various aspects of the interpretation of quantum mechanics. In his latest preprint (22 September 1999), entitled 'Super Classical Quantum Mechanics: *The* Interpretation of Nonrelativistic Quantum Mechanics,' Lamb has pointed out that Newtonian classical mechanics (NCM) suffers from several kinds of chaotic indeterminacy, but 'these shortcomings can be repaired in a simple and obvious manner. The NCM theory is thereby transformed into a new (probabilistic) theory which is fully equivalent to the Non-Relativistic Quantum Mechanics of Heisenberg, Schrödinger, and Dirac with the Max Born probabilistic interpretation of the state function built in from the start. I call this new theory Super Classical Quantum Mechanics, (SCQM). With the help of Paul Ehrenfest's 1927 theorem, the *classical limit* of the new theory, SCQM, gives exactly the results expected of an improved Newtonian theory of Classical Mechanics.'

'This approach offers enormous advantages, not only for a physically reasonable interpretation of Quantum Mechanics, but also for its contribution to the Quantum Theory of Measurement, and for the avoidance of all the so-called paradoxes of traditional non-relativistic Quantum Mechanics.'

In a telephone conversation on 24 October 1999, Lamb repeated to J.M. that 'my work on the interpretation of quantum mechanics has given me even greater pleasure than the measurement of the fine structure of energy levels in hydrogen [in 1947]' (for which he was awarded the Nobel Prize for Physics in 1955). However, this is not the place to discuss all of Lamb's diverse contributions to the problems of the interpretation of quantum mechanics.

invariance in the infinitely fast collapse of the wave function (according to the standard interpretation) would be avoided (Aharonov, 1994). Historical books (such as Jammer, 1974; Cushing, 1994a) and annotated collections of papers (Wheeler and Zurek, 1983) have been devoted to the topic, as much as personal accounts.[1258] A considerable number of popular and pedagogical accounts appeared in newspapers and general scientific journals, especially in *American Journal of Physics*.[1259] After many years, even decades of silence, the younger generation of quantum physicists finally rediscovered the old concerns of Albert Einstein (the *EPR* paradox) or Erwin Schrödinger (the 'cat paradox'), and many of them started to have difficulties with what they thought to be '*the* Copenhagen interpretation' (often without knowing its detailed contents). On the other hand, the alternative causal theory of David Bohm and John Bell's inequalities deduced from it set the most skillful experimentalists into action, and they devised wonderful tests of the fundamental problems of the interpretation of quantum mechanics, which we shall discuss next.

(b) Tests of EPR-Type Gedankenexperiments: Hidden Variables or Nonlocality (1972–1986)

We mentioned earlier (in Section 3.2) that John F. Clauser and collaborators proposed in 1969 an experimental test of the generalized Bell inequality, Eq. (990), for the correlation of a pair of optical photons. Clauser also performed the first such experiment with Stuart J. Freedman (1972). In particular, they were interested in 'the linear polarization correlation of the photons emitted in an atomic cascade of calcium,' involving the transitions $J = 0 \rightarrow J = 1 \rightarrow J = 0$, and used the following setup and procedure:

> The decaying atoms were viewed by two symmetrically placed optical systems, each consisting of two lenses, a wavelength filter, a rotatable and removable polarizer, and a single-photon detector. The following quantities were measured: $R(\phi)$, the coincidence rate for two-photon detection, as a function of the angle ϕ between the planes of linear polarization defined by the orientation of the inserted polarizers; R_1 the coincidence rate with polarizer 2 removed; R_2, the coincidence rate with polarizer 1 removed; R_0, the coincidence rate with both polarizers removed. (Freedman and Clauser, 1972, p. 939)

A comparison of the predictions from the competing theories with the experimental results should decide the problem of interpretation.

[1258] A recent book by the elementary particle physicist Roland Omnès, in which the author announced an 'essentially fresh approach to the older interpretation we all owe to Bohr, though putting it on new and firmer foundations' (Omnès, 1994, p. xi), was criticized by a reviewer as 'differ[ing] in a very substantial way from the Measurement Postulate' in the Copenhagen interpretation (Gilmore, 1996, p. 72).

[1259] Let us refer, in this context, to the completely nontechnical essay of N. David Mermin, 'Bringing Home the Atomic World: Quantum Mysteries for Anybody' (1981).

Now, quantum mechanics predicted for the ratios $R(\phi)/R_0, R_1/R_0$ and R_2/R_0 the respective results

$$R(\phi)/R_0 = \frac{1}{4}(\varepsilon_M^1 + \varepsilon_m^1)(\varepsilon_M^2 + \varepsilon_m^2) + \frac{1}{4}(\varepsilon_M^1 - \varepsilon_m^1)(\varepsilon_M^2 - \varepsilon_m^2) \cdot F_1(\theta)\cos 2\phi,$$

$$R_1/R_0 = \frac{1}{2}(\varepsilon_M^1 + \varepsilon_m^1), \quad \text{and} \quad R_2/R_0 = (\varepsilon_M^2 + \varepsilon_m^2), \tag{998}$$

with $\varepsilon_M^i + \varepsilon_m^i$ the transmittance of polarizer i for light polarized parallel $(i = 1)$ or perpendicular $(i = 2)$ to the polarization axis, and $F_1(\theta)$ a function of the half-angle θ subtended by the primary lenses. In the case of a local hidden-variable theory, Clauser and Freedman derived the inequalities

$$0 \geq \Delta\phi \geq -1, \tag{999}$$

where

$$\Delta\phi = \frac{3R(\phi)}{R_0} - \frac{R(3\phi)}{R_0} - \frac{R_1 + R_2}{R_0}. \tag{999a}$$

For sufficiently small detector solid angles and highly efficient polarizers, Eqs. (999) would not be satisfied by the prediction of Eq. (997) for a range of values ϕ, especially not for $\phi = 22.5°$ (where $\Delta(\phi) > 0$) and $\phi = 67.5°$ (where $\Delta(\phi) < -1$). 'We observe no evidence for a deviation from the predictions of quantum mechanics,' Friedman and Clauser found; hence, they considered 'these results to be strong evidence against local hidden-variable theories' (Friedman and Clauser, *loc. cit.*, p. 940). After an experiment of other colleagues (who worked with cascade radiation of atomic mercury) had yielded a result contradicting quantum mechanics, Clauser also repeated this experiment, but obtained no such deviation (Clauser, 1976).[1260] Two years later, Clauser and Abner Shimony—in a review, entitled 'Bell's Theorem: Experimental Tests and Implications'—summarized the status of the four existing photon-polarization experiments and one experiment on the spin-correlation of proton pairs—the latter also did not contradict quantum mechanics—plus a few other, more indirect experimental tests:

> Although further experimental investigations of the family of theories governed by Bell's theorem are desirable, we are tentatively convinced that no theory of this kind can correctly describe the physical world.... Because of the evidence in favour of quantum mechanics from the experiments based upon Bell's theorem, we are forced either to abandon the strong version of *EPR*'s criterion of reality ... or else to accept some kind of action-at-a-distance. (Clauser and Shimony, 1978, p. 1921)

[1260] A little later, Edward S. Fry and Randall C. Thompson from Texas A. & M. University also confirmed Clauser's results (Fry and Randall, 1976).

In spite of this strong statement by an expert experimentalist, certain loopholes in his argument still had to be closed. In particular, Clauser had analyzed in a paper with Michael Horne what they called 'objective local theories,' i.e., local theories of the Bohm type, which they sharpened by adding an extra—though rather weak and obvious—'no enhancement assumption' stating that 'for every λ, the probability of a counter with a polarizer in place is less than or equal to the polarizability with the polarizer removed' (Clauser and Horne, 1974, p. 530). If this additional assumption were not added, they were able to construct a peculiar local model theory leading to identical results in the polarization experiments as obtained in quantum mechanics (Clauser and Home, *loc. cit.*, pp. 530–531).[1261] Finally, however, three experiments of Alain Aspect and collaborators at the *Université de Paris-Sud* in Orsay decided the case, for they removed essentially all possible supplementary conditions that had so far been assumed in the local theories of the Bohm–Bell type which had competed with quantum mechanics in the previous experimental tests.

In a first note, submitted to *Physical Review Letters* in March 1981, Aspect (with Philippe Grangier and Gérard Roger) reported the results of a measurement *à la* Freedman and Clauser on the polarization correlation of visible photons emitted in a $(J = 0) \to (J = 1) \to (J = 0)$ atomic radiative cascade of calcium; they obtained an excitation rate more than ten times greater than that of Fry and Thompson, which allowed them to achieve much higher statistics and also to carry out a great variety of additional tests and measurements over the entire 360° range of relative orientation of the polarizers—thus avoiding the extra assumption of rotational invariance (Aspect, Grangier, and Roger, 1981). They further stated in their letter:

> The experiment was performed for various distances between the source and the polarizers. For large separations, our results are able to rule out various hypotheses according to which a nonfactorizing pure state for two particles (such as a singlet state) evolves towards a mixture of factorizing states when the two particles separate. Accordingly, such a localization process then occurs over distances of the order of the coherence length of the wave packets associated with the emitted photons. (Aspect *et al.*, *loc. cit.*, p. 460)

Indeed, they arrived at the following result: While the generalized Bell theorem yielded for a 'realistic local theory' the specific inequalities

$$-1 \leq S = [R(\mathbf{a}, \mathbf{b}) - R(\mathbf{a}, \mathbf{b}') + R(\mathbf{a}', \mathbf{b}) + R(\mathbf{a}', \mathbf{b}') - R_1(\mathbf{a}') - R_2(\mathbf{b})]/R_0 \leq 0, \tag{1000}$$

[1261] A few years later, T. K. Lo and Abner Shimony proposed an experiment (1981), in which local theories not obeying the Clauser–Home no-enhancement assumption might be tested.

where $R(\mathbf{a}, \mathbf{b})$ was the rate of the coincidences with polarizer I in orientation **a** and polarizer II in orientation **b**, R (**a**, $\mathbf{b}'$), the corresponding rate for polarizer I in orientation **a** and polarizer II in orientation $\mathbf{b}'$, etc., $R_1(\mathbf{a}')$ and polarizer I in orientation $\mathbf{a}'$ [and suitably for $R_2(\mathbf{b})$], and R_0 was the coincidence rate with the two polarizers removed, quantum mechanics predicted the result of Eq. (998). Now for a particular set of orientations, Aspect *et al.* found experimentally

$$S_{\text{exp}} = 0.126 \pm 0.014, \tag{1001a}$$

'violating the inequality [(1000)] by 9 standard deviations and in good agreement with quantum mechanics (QM) prediction' yielding

$$S_{\text{QM}} = 0.118 \pm 0.005. \tag{1001b}$$

Hence, they concluded: 'Our results, in excellent agreement with quantum mechanics predictions, are to a high statistical accuracy a strong evidence against the whole class of realistic local theories,' and 'furthermore, no effect of the distances between measurements on the correlations was observed.' (Aspect, Grangier and Roger, 1981, p. 463)

The following letter by Aspect *et al.* reported on the results of an Einstein–Podolsky–Rosen *Gedanken*experiment in the peculiar version proposed earlier by David Bohm and Yakir Aharonov (1957)—discussed above—by saying:

> A source emits pairs of spin-$\frac{1}{2}$ particles [or photons] in a singlet state (or pairs of photons in a similar nonfactorizing state). After the particles have separated, one performs correlated experiments of their spin components along arbitrary directions **a** and **b**. Each measurement can yield two results, denoted ± 1; for photons, a measurement along **a** yields the result $+1$ if the polarization is found parallel to **a**, and -1 if the polarization is found perpendicular. For a singlet state, quantum mechanics predicts some correlation between such measurements on the two particles. Let us denote by $P_{++}(\mathbf{a}, \mathbf{b})$ the probabilities of obtaining the result ± 1 along **a** (particle1) and ± 1 along **b** (particle 2). The quantity
>
> $$E(\mathbf{a}, \mathbf{b}) = P_{++}(\mathbf{a}, \mathbf{b}) + P_{--}(\mathbf{a}, \mathbf{b}) - P_{+-}(\mathbf{a}, \mathbf{b}) - P_{-+}(\mathbf{a}, \mathbf{b}) \qquad [(1002)]$$
>
> is the correlation coefficient of the measurements on the two particles. (Aspect, Grangier and Roger, 1982, p. 91)

The return to the experiment considered by Bohm and Aharonov allowed one, in comparison with the variations of the *EPR* experiment explored so far, to avoid some of the earlier disadvantages, such as the low efficiency of the detection system, no direct measurement of the counting rates, extra measurements, and the additional 'non-enhancement' assumption. In particular, as Aspect *et al.* emphasized, 'true dichotomic polarization measurements on visible photons have been performed by replacing ordinary polarizers by two-channel polarizers, separating two orthogonal linear polarizations, followed by two multipliers,' and they described the procedure used as follows:

> Using a fourfold coincidence method, we measure in a single run the four coincidence rates $R_{\pm\pm}(\mathbf{a}, \mathbf{b})$ yielding directly the correlation coefficient for the measurement along $\mathbf{a}$ and $\mathbf{b}$:
>
> $$E(\mathbf{a}, \mathbf{b}) = \frac{R_{++}(\mathbf{a}, \mathbf{b}) + R_{--}(\mathbf{a}, \mathbf{b}) - R_{+-}(\mathbf{a}, \mathbf{b}) - R_{-+}(\mathbf{a}, \mathbf{b})}{R_{++}(\mathbf{a}, \mathbf{b}) + R_{--}(\mathbf{a}, \mathbf{b}) + R_{+-}(\mathbf{a}, \mathbf{b}) + R_{-+}(\mathbf{a}, \mathbf{b})}. \qquad [(1003)]$$
>
> It is then sufficient to repeat the same measurement for three other choices of orientations, and inequalities
>
> $$2 \leq S \leq 2, \qquad [(1004a)]$$
>
> where
>
> $$S = E(\mathbf{a}, \mathbf{b}) - E(\mathbf{a}, \mathbf{b}') + E(\mathbf{a}', \mathbf{b}) + E(\mathbf{a}', \mathbf{b}') \qquad [(1004b)]$$
>
> can directly be used to test the realistic local theories *versus* quantum theory. (Aspect, *et al.*, *loc. cit.*, p. 92)

This new experiment certainly satisfied many requirements, e.g., possible systematic errors could be largely suppressed and the experimental parameters (e.g., the counting rates of the photon-multipliers) were under direct control. By taking runs at each of the orientations $(\mathbf{a}, \mathbf{b}) = (\mathbf{b}, \mathbf{a}') = (\mathbf{a}', \mathbf{b}') = 22.5°$ and $(\mathbf{a}, \mathbf{b}') = 67.5°$, where the differences between the quantum-mechanical predictions and the inequalities [(1004)] were expected to be a maximum, Aspect, Grangier, and Roger observed

$$S_{\mathrm{exp}} = 2.697 \pm 0.015, \qquad (1005a)$$

in complete agreement with the calculated quantum-mechanical value

$$S_{\mathrm{QM}} = 2.70 \pm 0.05. \qquad (1005b)$$

The experiment therefore 'yields the strongest violation of Bell's inequalities ever achieved, and excellent agreement with quantum mechanics,' Aspect *et al.* expounded, but added more cautiously: 'We are thus led to the rejection of realistic local theories if we accept the assumption that there is no bias in the detected samples.' (Aspect *et al.*, *loc. cit.*, p. 94) However, they were pretty sure that they had applied some checks in order to exclude any bias; hence, only two points had to be investigated further: the role of the (low) efficiency of the detectors and, especially, the static character of the *EPR* experiments performed thus far. Therefore, Aspect, with Jean Dalibard and Gérard Roger, carried out a 'timing experiment' by applying time-varying analyzers switching fast between two orientations and presented the results in a note to *Physical Review Letters*, submitted in September 1982 (Aspect, Dalibard, and Roger, 1982). Again, they arrived at the definite conclusion that the generalized Bell inequality in this case, $S \leq 0$, was strongly violated by the experimental result, $S_{\mathrm{exp}} = 0.101 \pm 0.020$, which agreed perfectly with the quantum-mechanical prediction, $S_{\mathrm{QM}} = 0.112$ (Aspect, Dalibard, and Roger, *loc. cit.*, p. 1807).

The experiments in the 1970s and early 1980s stimulated lively debates among the interested theoreticians, who quickly recognized the role of Bell's inequalities and its consequences—'Bell's theorem'—for the problem of interpretation. Thus, Henry Stapp of Berkeley debated with Leslie E. Ballentine of British Columbia about the exact relation between Bell's conclusions and quantum mechanics (see, e.g., Ballentine, 1974, and Stapp, 1974). Stapp later reformulated 'Bell's theorem as a nonlocality property of quantum theory itself, with no explicit or implicit reference to determinism or hidden variables' (Stapp, 1982, p. 1470). That is, he argued that the theorem, 'which says that any theory compatible with the statistical predictions of quantum theory is nonlocal, provided the theory is a deterministic hidden variable theory' (Stapp, 1982, p. 1471), can also be generalized to theories which are neither deterministic nor contain hidden variables—even to quantum mechanics (either in the Copenhagen version or in the mathematically sharpened form given to it by von Neumann), or the many-world theory of Everett *et al.* (Stapp, *loc. cit.*, p. 1473). Stapp's Berkeley colleague, Philippe H. Eberhard, supported these points of view: On the one hand, the view that Bell's inequalities have 'to be equivalent with determinism,' on the other, 'that locality rather than determinism is the issue' in the problem of interpretation. In this context, he remarked:

> There are deterministic models and nondeterministic ones that violate Bell's inequalities. There are deterministic and nondeterministic ones that do not. These inequalities are not the consequences of determinism but of independence conditions between measurement results in $R_A(R_B)$ and apparatus setting $R_B(R_A)$. The predictions of quantum mechanics violate the independent conditions even when R_A and R_B are separated in space. Therefore they violate the corresponding definitions of locality. (Eberhard, 1982, pp. 1476–1477)

In a further article, Stapp distinguished between 'two different ideas of locality due to Einstein,' the first having been used by Einstein in the theory of relativity, and the second by Einstein, Podolsky, and Rosen in their paradox: 'Quantum theory is compatible with the first but not the second.' (Stapp, 1985, p. 973) Therefore, he pleaded in favour of abandoning the *EPR* locality in order to arrive at an 'adequate integral theory of quantum and classical phenomena' (Stapp, *loc. cit.*, p. 976). His opponent Ballentine recognized an even deeper gap between Bell's inequalities and quantum mechanics. Together with Jon P. Jarrett, he demonstrated 'that the locality principle needed for Bell's theorem is stronger than the simple locality that is needed to satisfy the demands of relativity and that quantum mechanics satisfies the latter' (Ballentine and Jarrett, 1987, p. 696). In greater detail, they sought to distinguish between two types of locality, the 'simple locality' and the 'strong locality'; and the latter locality, 'used to derive Bell's theorem and its generalization,' was 'logically equivalent to the conjunction of the simple locality and the predictive completeness of the state description.' Consequently, they ascribed 'the violation of Bell-type inequalities by quantum mechanics to the failure of predictive completeness' of the latter theory (Ballentine and Jarrett, *loc. cit.*, p. 700). But they went on to state further:

> The notion of completeness introduced by Einstein, Podolsky and Rosen is stronger than predictive completeness and implies it. Thus we have vindicated the *EPR* conclusion that the quantum-mechanical description of reality is not complete, and have done so independently of the details of their argument. But contrary to the belief of *EPR*, it is not merely the quantum-mechanical state description that is "incomplete" (in their sense, and in our more general sense). Rather it is the case that *any* state description that yields agreement with the statistical predictions of quantum mechanics, in particular those that violate Bell's inequalities must be "incomplete." Since the violation of Bell's inequalities has been confirmed by experiment, this "incompleteness" is, in some sense a property of nature. (Ballentine and Jarrett, *loc. cit.*, p. 700)

The whole discussion reported above reminds one of what Bohr said in his response to Einstein. 'In fact this new feature of natural philosophy [i.e., complementarity] means a radical revision of our attitude as regards physical reality, which may be paralleled with the fundamental modifications of all ideas regarding the absolute character of physical reality, brought about by the general theory of relativity.' (Bohr, 1935b, p. 702)

(c) The Process of Disentanglement of States and Schrödinger's Cat: An Experimental Demonstration (1981–1999)

'I have very little understanding of the position of Bohr,' wrote John Bell in an appendix to a talk entitled 'Bertelmann's Socks and the Nature of Reality,' and added: 'Yet most contemporary theorists have the impression that Bohr got the better of Einstein in the argument [on the *EPR* paradox] and are under the impression that they themselves share Bohr's views.' (Bell, 1981, p. 60) What Bell meant in this context was the *EPR* definition of 'physical reality,' notably: 'If, without in any way disturbing the system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.' (Einstein, Podolsky, and Rosen, 1935, p. 777), and Bohr's reply to it: 'The wording of the above-mentioned criterion ... contains an ambiguity as regards the meaning of the expression "without in any way disturbing the system" [as] there is in a case like that just considered [i.e., the *EPR* problem] no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure' (Bohr, 1935b, p. 699), and also the further statement that 'even at this stage there is essentially the question of an *influence on the very conditions which define the possible types of predictions regarding the future behavior of the systems*' (Bohr, *loc. cit.*). Bell was disturbed by this and wrote:

> Indeed I have very little idea what this means. I do not understand in what sense the word "mechanical" is used, in characterizing the disturbances which Bohr does not contemplate, as distinct from those which he does. I do not know what the italicized passage means.... Could it mean just that different experiments on the first system give different kinds of information about the second? But this was just one of the main points of *EPR*, who observed that one could learn either the position *or* the momentum of the second system. (Bell, 1981, p. 60)

Actually, Bell was not right in assuming that all theoreticians, but a very few (like Bohm), did not have their problems with Bohr's arguments in replying to *EPR* and the whole interpretation problem in quantum mechanics. Thus, for example, Rudolf Haag, who spent the year 1953 in Copenhagen, recalled:

> Niels Bohr told me: "I have again received a manuscript from Professor Bopp [i.e., probably the one on the 'Statistical Investigation of the Fundamental Processes in the Quantum Theory of Elementary Particles' (Bopp, 1953)]. I do not understand why one deals still with items which are perfectly clarified since decades. There are still so many [different] interesting problems to solve." My rash (*unüberlegte*) answer, "Perhaps things are not that clear," stimulated many discussions. I tried to argue that we do not know the root of the superposition principle and that the analysis of the mathematical structure may give hints regarding the further development of the theory. Bohr immediately replied: "But this is very stupid. There is no inspiration without reference to experiment." (Haag, 1999, p. 2)

During the forty-five years that elapsed since this encounter with Bohr, Haag reflected on the 'perfectly clarified item' and the whole question of the interpretation of quantum mechanics, both in the nonrelativistic and the relativistic schemes. A major clue which he derived from the mathematical structure of nonrelativistic theory consisted in the feature of 'entanglement,' which can be easily seen by looking at the product Hilbert space of two systems,

$$\mathscr{H} = \mathscr{H}_1 \otimes \mathscr{H}_2, \tag{1006}$$

in which the general vector Ψ assumes the form

$$\Psi = \Sigma c_{k\alpha} \Psi_k^{(1)} \otimes \Psi_\alpha^{(2)}, \tag{1007}$$

where $\Psi_k^{(1)}$ and $\Psi_\alpha^{(2)}$ denote orthogonal basis systems in the Hilbert spaces $\mathscr{H}_1$ and $\mathscr{H}_2$. Then, Ψ 'cannot be reduced to the form of a simple product [of two vectors, one in every Hilbert space] ... [which] exhibits a holistic feature of quantum theory,' or: 'The whole is more than the sum of its parts.' (Haag, *loc. cit.*, p. 7)

Thus, in the observation of events in the whole system 'correlations will occur between the events in the two partial systems which can be observed in coincidence measurements carried out in the partial systems,' and: 'If these deal with a conserved quantity of the partial systems (spin orientation, energy, momentum, charge) then the correlation remains in fact for long periods of time and over long distances.' (Haag, *loc. cit.*)

Actually, the situation which Haag sketched in these few lines had been described already in much greater, less-technical, detail by Erwin Schrödinger in his article in the *Naturwissenschaften* on '*Die gegenwärtige Situation in der Quantenmechanik* (The Present Situation in Quantum Mechanics)' arising from the debate on the *EPR* paradox (Schrödinger, 1935a).[1262] In order to illustrate the *EPR* situation, he had formulated his famous 'cat paradox' (in his Section 5) and then turned to a detailed discussion of the process of measurement in quantum mechanics (Sections 8–14). In dealing with the case of two systems which are brought into interaction, he spoke about the 'entanglement of predictions (*Verschränkung der Voraussagen*,' which he explained as follows:

> If two separated bodies, each by itself known maximally, enter a situation in which they influence each other, and separate again, then there occurs regularly that which I just have called *entanglement* of our knowledge of the two bodies. The combined expectation-catalogue consists initially of a logical sum of individual catalogues; during the process it develops causally in accord with known law (there is no question whatsoever of measurement here). The knowledge remains maximal, but at its end, if the bodies have again separated, it is not again split into a logical sum of knowledges about the individual bodies. What still remains of *that* may have become less maximal, even very strongly so. (Schrödinger, *loc. cit.*, p. 827; English translation, p. 161)

Exactly this situation now occurred in the quantum-mechanical measurement process, where the measured object and the measuring instrument represented the two systems to get entangled and disentangled again.

During the performance of the measurement process, continuous changes happen to the wave functions of the two systems, which evolve into a combined ψ-function of object and apparatus and further (as determined by the partial differential equation of the Schrödinger type); only when the measurement has been completed with the registration of the result, the well-known jump in the wave function of the object occurs. 'But it would not be quite right to say that the ψ-function of the object ... should *now* change leapwise because of the mental act [of registration],' Schrödinger wrote; he rather claimed that the previous wave function had disappeared, and:

> It is born anew, is reconstructed, as separated out from the entanglement knowledge that one has, through the act of perception, which as a matter of fact is not a physical

[1262] We have reported on this debate in Section IV.2; here, we just focus on the scenarios which Schrödinger developed for the processes of 'entanglement' and the 'resolution of the entanglement.'

effect on the measured object. From the form in which the ψ-function was last known, to the new in which it reappears, runs no continuous road—it ran indeed through annihilation. (Schrödinger, *loc. cit.*, p. 828; English translation, p. 162)

In truth, he continued, the quantum-mechanical interaction with the measuring device changed the original wave function of the microscopic object drastically, in that the wave functions of both systems form a space of higher dimensions—see Eq. (1007)—and 'as soon as the systems begin to influence each other, the combined function ceases to be a product and moreover does not again divide up, after they have again become separated into factors that can be assigned individually to the systems' (Schrödinger, *loc. cit.*, pp. 848–849; English translation, p. 167). The 'resolution of the entanglement' would occur only through an actual observation, which then would lead to von Neumann's reduction of the original wave packet (of the object), and Schrödinger concluded:

This is the reason that knowledge of the individual systems can decline to a scantiest, even to zero, while that of the combined system remains continually maximal. Best possible knowledge of a whole does *not* include best possible knowledge of its parts—and that is what keeps coming back to haunt us. (Schrödinger, *loc. cit.*, p. 849; English translation, p. 167)

While Rudolf Haag did not have difficulties to live with these consequences from the quantum-mechanical theory, John Bell shared Schrödinger's concern to accept them. In a paper presented at the London symposium to celebrate the centenary of Erwin Schrödinger's birth (held at the Imperial College from 31 March to 3 April 1987), Bell rather advocated a recent proposal of a 'Unified Dynamics for Microscopic and Macroscopic Systems' (Ghirardi, Rimini, and Weber, 1986), which suggested the modification of the Schrödinger equation in order to avoid quantum jumps (Bell, 1987). In particular, this modification allowed one to describe the macroscopic objects with the help of wave functions which occasionally undergo random, spontaneous jumps; these would occur extremely rarely with a probability per unit time of

$$P = \frac{N}{\tau}, \tag{1008}$$

where N is the number of arguments $\mathbf{r}$ in the wave function $\psi(t_p\mathbf{r}_1, \ldots, \mathbf{r}_N)$ of the object and τ is a new constant of nature of the order of magnitude 10^8 years. 'For myself, I see the *G R W* model as a very nice illustration of how quantum mechanics, to become rational, requires only a change which is very small,' Bell concluded (1987, p. 49).[1263]

[1263] Bell showed that the jumps assumed by Ghirardi *et al.* would not lead to the cat paradox, as: 'Quite generally any embarrassing macroscopic ambiguity in the usual theory is only momentary in the *GRW* theory. The cat is not both dead and alive for more than a split second.' (Bell, 1987, p. 44)

As Bohr had reminded Haag in 1953, there is no inspiration for a new theory without reference to experiment. In this respect, the last decade in particular has become very instructive. The experimentalists have not only succeeded in demonstrating the build-up of an interference pattern in a double-slit experiment with single electrons; notably, a Tokyo group observed, by detecting the final electrons one by one, how an interference pattern arose as the accumulation of electrons increased in time (Tonomura *et al.*, 1989, especially, p. 119). Similarly, experiments with single photons exhibited the quantum-mechanically predicted particle-wave duality (Grangier, Roger, and Aspect, 1986). Quite generally speaking, the enormous development during the 1970s and 1980s of the techniques in quantum optics provided the tools for new sensitive tests of Bell's inequalities, e.g., the 'Bell Inequality for Position and Time,' derived by J. D. Franson of Johns Hopkins University (1989). Based on Marlan O. Scully and Kai Drühl's concept of the 'quantum eraser' (1982)—a device designed to erase the path which a photon has taken in a slit experiment (see also Scully, Englert, and Walther, 1991)—Paul G. Kwiat, Aephraim M. Steinberg, and Raymond Y. Chiao of Berkeley carried out the double-slit experiment proposed by Franson, according to which 'sinusoidal fringes with visibilities greater than 70.7%, such as predicted by quantum theory, violate a Bell inequality'; they observed a 'visibility of $80.4 \pm 0.6\%$, implying a violation of the inequality by 16 standard deviations' (Kwiat, Steinberg, and Chiao, 1993, p. R2472). Kwiat *et al.* noted further that 'any classical field models describing separate beams in a Franson interferometer are limited to visibilities less than 50% and hence ruled out as well, without the need for any supplementary assumptions' (Kwiat *et al.*, *loc. cit.*). New techniques devised in the early 1990s involving single particle interference and path information opened even more sensitive checks of Bell's theorem. (See Anton Zeilinger's talk on 'Einstein-Podolsky-Rosen Interferometry,' 1996, delivered at the International Nathan Rosen Symposium in Haifa in March 1995.) Especially, they allowed one to study a generalization of Bell's inequalities in three- and four-particle arrangements, as proposed by Daniel Greenberger, Michael Horne, Abner Shimony, and Anton Zeilinger in a paper, entitled 'Bell's Theorem Without Inequalities,' and submitted in June 1990 to *American Journal of Physics* (and published in the December issue); the authors demonstrated that in this case the 'incompatibility with quantum mechanics is stronger than the one previously revealed for two-particle systems by Bell's inequality, where no contradiction arises at the level of perfect correlations' (Greenberger *et al.*, 1990, p. 1131).

By using the improved interferometric methods, especially a 'three-grating Mach Zehnder atom interferometer,' Michael S. Chapman and collaborators at MIT performed a scattering experiment with 'single photons from interfering de Broglie waves' and observed 'contrast loss and revivals as the separation of the paths at the point of scattering increased' (Chapman *et al.*, 1995, p. 3783). In addition, they found that the lost coherence might be recovered again by observing only atoms that are correlated with photons emitted in a limited angular range; hence, they concluded: 'The coherence was not truly destroyed, but only entangled with the final state of the [macroscopic] reservoir.' (Chapman *et al.*, *loc. cit.*, p. 3786) It was the detailed processes of coherence and decoherence that could thus be 'visualized' in the experiments of the mid-1990s, which ultimately helped to resolve Schrödinger's paradox by removing it from the realm of atomic physics. In a paper, entitled 'Observing the Progressive Decoherence of the "Meter" in a Quantum Measurement,' received in September 1996 by *Physical Review Letters* and published in the issue of 9 December, Serge Haroche with a group of his colleagues at the *École Normale Supérieure* in Paris succeeded in translating Schrödinger's *Gedanken*experiment into the latest experimental atomic physics and to give the final answer (Brune *et al.*, 1996).

Practically, the Paris team of investigators realized a model of Schrödinger's cat consisting of a single atom and an electromagnetic field in a cavity. In particular, they generated 'the mesoscopic state by sending a rubidium atom, prepared in a superposition of two circular Rydberg states e and g [having high quantum numbers 51 and 50 and a transition frequency of 51.099 GHz], across a high Q microwave cavity C storing a small coherent field $|\alpha\rangle$'; and they 'measured the coupling between the atom and the cavity by the "Rabi frequency" Ω' (Brune *et al.*, *loc. cit.*, pp. 4887–4888). Further, they chose the $e \to g$ transition and the cavity frequency slightly off-resonance (detuning δ); hence, a coupling atom-field was obtained during the time t which created an 'entangled state' $|\psi\rangle$ given by

$$|\psi\rangle = \frac{1}{\sqrt{2}}[|e, \alpha \exp(i\Phi)\rangle + |g, \alpha \exp(-i\Phi)\rangle, \tag{1009}$$

with $|\alpha| = \sqrt{n}$ (where n denoted the mean number of oscillatory quanta) and $\Phi = \Omega^2 t/\delta$. As the cavity, Brune *et al.* took a Fabry–Pérot resonator with its axis normal to the atomic trajectory, made of two superconducting niobium mirrors, having $\Omega/2\pi = 24$ kHz and being tunable by adjusting the mirror separation ($\delta/2\pi$ between 70 and 800 kHz; thus, $\Phi = 0.69$ for $\delta/\pi = 100$ kHz). The experimental setup consisted of a rubidium source (oven O), two diode lasers L_1 and L_1' to obtain atoms of a given velocity, and a box B to excite the atoms into the state e; the superposition of e and g states was achieved by a low-Q cavity R_1, and the atom then passed the cavity C in which a weak coherent field with average photon number n varying from 1 to 10 was injected, such that it existed only when crossed by the atom; after leaving C, each atom received a $\frac{\pi}{2}$ pulse in a cavity R_2 (made identical with R_1), and the atomic states e and g were finally counted with the help of two field-ionization detectors. 'With 50,000 events recorded in 10 minutes, the probability $P_g^{(1)}(\nu)$ to find an atom as a function of [the cavity frequency] ν is reconstructed,' Brune *et al.* reported (Brune *et al.*, *loc. cit.*, p. 4888) and analyzed the 'Ramsey fringes' of $P_g^{(1)}(\nu)$ as dependent on ν. The plots with oscillating curves (fringes) showed marked differences, when the tuning was altered from $\delta/2\pi = 712$ to 104 kHz in a cavity field given by $n = 9.5$ and

$|\alpha| = 3.1$. In particular, Brune *et al.* noticed two striking features, namely, 'when δ is reduced, the contrast of the fringes decreases and their phase is shifted,' which they interpreted as clear evidence for 'the separation of the field state into two components' (Brune *et al.*, *loc. cit.*, p. 4889).

The translation of these rather technical results into a more visual language, understandable to the nonexpert, was given by Serge Haroche in an interview to Gary Taubes, who then wrote a report on the Paris experiment in the *Science* issue of 6 December 1996:

> To detect this strange state [i.e., a cat being, according to Schrödinger, in a superposition of two states, alive and dead], says Haroche, you make a small hole in a box [containing the arrangement of a cat and a radioactive pellet triggering a device to kill the cat] and send in a mouse.... He adds, however, that such an experiment will never work with such macroscopic systems as cats or mice. An ubiquitous process known as decoherence will instantly destroy the quantum superposition, making the cat either dead or alive and washing out the quantum interference between the two outcomes. (Taubes, 1996, p. 1615)

As Taubes reported further, in the experiment, the cat consisted of a few microwave photons in an indeterminate state, and the mouse of an atom prepared so that it could react to the dead and alive state of the cat. Actually, both in generating the cat and the mouse, Haroche *et al.* used a rubidium atom in the high Rydberg states; they first created the cat field by one atom (demonstrating the collapse of the mixed wave functions) and then sent in a second atom to interact with the state of the cat:

> "The first atom prepares this strange state," says Haroche, "and the second atom goes across the cavity and interacts with this strange state, again by shifting its phase, and then it goes out and you detect it" and compare its state with the final state of the first atom. By repeating the experiment many times, the physicist can measure the probability that the second atom emerges in a state relative to the first atom. This "conditional probability" has a measurable quantum interference if the electromagnetic field is in a quantum superposition when the second atom passes through. (Taubes, *loc. cit.*)

The experiment of Haroche *et al.* in actually realizing Schrödinger's *Gedanken*-experiment thus consisted of two parts: In Part One, the Paris group determined the decay time of the fields into one state or the other by changing the delay time between the two rubidium atoms, because the longer this delay time the more the coherence has decayed and the second atom would not detect its size. In Part Two, they measured how the lifetime of the cat-field superposition changed with its size by injecting more microwave photons into the cavity or increasing the phase difference between the two states—by both procedures, the cat would become more macroscopic and the coherence would decrease. That is, the decay of the coherent state became faster and faster, as the size of the cat grew; hence:

> This size effect may be the explanation for why even Schrödinger's mouse would never be able to detect a full-grown Schrödinger's cat. "If you had a real Schrödinger's cat in the box," says Haroche, "you would never see the superposition, because the decoherence time is so short for big systems." (Taubes, *loc. cit.*)

The team of Hagley *et al.* at the *École Normale Supérieure* in Paris also performed *EPR* experiments with rubidium atoms entangled in a superposition of two different Rydberg states which they separated then by distances of the order of 1 cm (Hagley *et al.*, 1997). A different experiment of the Paris group concerned another phenomenon arising in quantum mechanics: In a letter to *Nature*, published in the issue of 17 May 1999, under the title 'Seeing a Single Photon Without Destroying It,' G. Nogue *et al.* used 'atomic interferometry to measure the phase shift in an atomic wave function, caused by a cycle of photon absorption and emission,' that is, they verified 'a restricted quantum non-demolition measurement' (G. Nogue *et al.* 1999, p. 239). Such measurements avoided the 'back-action effect' which previously had been explained as being due to the uncertainty relations. Vladimir Braginskii and Yu I. Vorontsov of Moscow had first suggested in 1974 the possibility of a nondestructive recording of the *n*-quantum state in an article on 'Quantum-Mechanical Limitations in Macroscopic Experiments and Modern Experimental Technique' (Braginskii and Vorontsov, 1975, especially, pp. 648–649). The main point of the procedure was 'to devise measurement schemes in which the back-action noise is kept entirely within unwanted observables, without being coupled back to the quantity of interest' and 'this quantity remains uncontaminated by the measurement process, allowing repeated measurements to be performed with arbitrary high accuracy' (Grangier, Levinson, and Poizat, 1998, p. 537). While these experiments do not violate the uncertainty relations—contrary to opposite claims in the literature—they may open the way for applications 'such as the noise-free information tapping in optical telecommunications' (Grangier *et al.*, *loc. cit.*) or 'to quantum logic gates based on cavity quantum electrodynamics, and multi-atom entanglement.' (G. Nogue *et al.*, 1999, p. 239)[1264]

The recent, advanced optical techniques permitted experiments rendering visible the detailed structure of the processes occurring in atomic physics. The 'reduction of the wave packet' in the measurement process, whether expressed in the mathematical language of von Neumann or in the resolution of the cat paradox of Schrödinger, as presented above, may be considered a particularly spectacular example. Thus far, quantum mechanics (and even its orthodox Copenhagen interpretation, perhaps with suitable extensions) has withstood all critical tests. All modifications of either the nonrelativistic theory of atoms, molecules, and solids, or the relativistic theory of elementary particles, have been shown to contradict some experiments. The fundamental theory, inaugurated by Max Planck at the

[1264] Another application of Braginskii's 'quantum non-demolition' seemed to be the measurement of a weak classical force coupled to a quantum-mechanical oscillator, especially gravitational forces (Caves *et al.*, 1980).

beginning of the 20th century and brought into its final form nearly seventy-five years ago, has been brilliantly confirmed by so many experimental and theoretical investigations, and also by the outcome of the debates on its interpretation.

Shortly before his death, David Bohm—together with D. L. Schumacher—wrote an essay 'On the Failure of Communication Between Bohr and Einstein' concerning their different interpretations of quantum mechanics and relativity theory. They finally concluded:[1265]

> Communications between Einstein and Bohr could have been opened up if each had become aware of his implicit judgments of relevance, and if both had, thus, gone out to explore new concepts in which neither relativity nor quantum theory would be considered to be basically relevant. Such communications would have been creative, rather than merely a means of conveying each point of view to the other. In such communications one is not talking "about" quantum theory or "about" relativity.

At the end of the 20th century, it is difficult to see clearly as to what Einstein and Bohr could have talked about more than fifty years ago, other than the great theoretical schemes to which they contributed so much. Although their dates of birth, 1879 and 1885, respectively, were separated only by a few years, Einstein had made his reputation primarily by completing the classical theories of statistical mechanics and the electrodynamics of moving bodies without really abandoning the goals and concepts of classical physics, while Bohr had fully entered into the challenges provided by the new quantum theory, which (in his opinion, and the opinion of most other creative quantum physicists) required different concepts or a 'different level of the description of reality' (see Heisenberg, 1984b, especially, pp. 233–236). Bohr's interpretation of the atomic level of reality rested on the two principles of complementarity *and* correspondence, of which the latter has often been underestimated in the debate on the interpretation of quantum mechanics during the past decades. Whatever might be the interpretation of a future description of microphysics, a return to the concepts of classical physics, even if they were generalized, would contradict the spirit of the entire development of 20th-century physics.

[1265] D. Bohm and D. L. Schumacher, unpublished manuscript, Birkbeck College, London, p. 8.

Conclusion: Four Generations of Quantum Physicists

Looking back now on one hundred years of quantum theory, we recognize that the foundations were laid essentially by four generations of pioneers. The first one consists of Max Planck (born 1858), Arnold Sommerfeld (born 1868), and the younger Albert Einstein (born 1879), who—though deeply rooted in the concepts of the 19th century—accomplished the transition from the classical to the quantum-theoretical description of a set of new physical phenomena. The second generation began with Max Born (born 1882) and included Niels Bohr (born 1885), Erwin Schrödinger (born 1887), and finally Louis de Broglie (born 1892), who built on the already existing quantum concepts and applied them to various fields of physics, notably, atomic theory. Although not in age, but rather through his crucial contribution, Schrödinger broke into the third generation, younger by about 15 years—consisting of Wolfgang Pauli (born 1900), Werner Heisenberg (born 1901), Paul Dirac (born 1902), Eugene Wigner (born 1902), and John von Neumann (born 1903)—which created quantum and wave mechanics. The fourth generation we have in mind enlarged the use of quantum mechanics and extended the formalism to treat adequately the new levels of physical phenomena. This generation also extended the limits beyond the European (especially Central European) continent to the whole scientific world; it included the Japanese Hideki Yukawa and Sin-itiro Tomonaga (both born in 1907), the Russians Lev Landau and Nikolai Bogoliubov (both born in 1908), the Indian Subrahmanyan Chandrasekhar (born 1910), and finally the Americans John Bardeen (born in 1908), Julian Schwinger, and Richard Feynman (both born in 1918). The lifetimes and activities of these four generations spanned nearly the entire century of quantum theory, as can be seen from the many contributions of the above-mentioned pioneers, their collaborators and disciples, discussed in Volumes 1–6. It is therefore fitting to end the last volume of this series by completing the stories of their lives and activities.

Let us begin with Max Planck, who initiated the whole development in 1900 by introducing the quantum of action h and thus laid the foundation of an entirely new description of nature. After retiring in October 1926 from his chair of theoretical physics at the University of Berlin—and leaving it eventually to Erwin Schrödinger—he took over in 1930 (besides his position as Permanent Secretary of the Prussian Academy of Sciences, which he had held since 1912) the Presidency of the *Kaiser Wilhelm-Gesellschaft (KWG)*, the society established in 1911 to house the most important German institutes for pure research. Thus, he advanced to becoming one of the highest representatives of scientific culture in Germany; as such, he had to handle—when in 1933 the dictatorial regime of the Nazis took over the political power in his country (and created the *Third Reich*)—very serious, if not dangerous, problems (see Heilbron, 1986); eventually, he was driven out from both of his offices (1937: the KWG, and 1938: the Prussian Academy). In World War II, Planck not only lost his home in Berlin (by the air raids of the Allies) but also his son Erwin (hanged in January 1945 by the Nazis). After the war, he took refuge in Göttingen and helped to reestablish the *KWG* under the name of *Max Planck-Gesellschaft (MPG)*. He died on 4 October 1947, in Göttingen.

Both of his former colleagues and friends, Arnold Sommerfeld and Albert Einstein, also ran into great troubles with the *Third Reich* authorities. Sommerfeld first lost, because of the Nazi racial laws, some of his best students and collaborators (e.g., Hans Bethe), and he himself was denounced later on as the main representative of the "degenerate," "international," and "non-Aryan" modern physics. His famous school of atomic theory in Munich ceased to exist (also because he was denied from taking on Werner Heisenberg as his successor); this school could not be reestablished after the war when the old Sommerfeld took up his chair over again (he died on 26 April 1951, in Munich, after suffering from an automobile accident). Einstein, on the other hand, had not returned in 1933 to Germany from his visit abroad, but he had already resigned his position in Berlin. He finally settled for the rest of his life in Princeton, New Jersey, to work at the Institute for Advanced Study, mainly on a generalized field theory of matter, always battling against the modern version which quantum theory had proclaimed since 1925. Although he warned in August 1939 U. S. President Roosevelt of the danger that the German government might succeed in developing an atomic bomb, he did not participate actively in the Manhattan Project and, after World War II, again became a pacifist and showed great sympathy for the new Jewish State of Israel. He died at Princeton on 18 April 1955, nearly fifty years after he had expounded the theory of relativity and light-quanta.

In spite of the fact that he was only three years younger than Einstein, Max Born belonged already to the next generation of quantum physicists who picked up the fruits of the pioneering work of the first generation of quantum theory. Also because of the Nazi racial laws, he was driven away in 1933 from his chair in Göttingen. He first went to England (Cambridge), then joined C. V. Raman in Bangalore, India, for about six months before being appointed in fall 1936 to the Tait Professorship of Natural Philosophy at the University of Edinburgh (as successor to Charles Galton Darwin). There, he assembled again a fair number of talented collaborators and students (from Klaus Fuchs to Herbert S. Green), investigating especially the problems of solid-state physics and kinetic theory. After his retirement at the end of 1953, he returned to Germany and settled with is wife, Hedwig, at Bad Pyrmont near Göttingen. Born then played an influential role as an elder statesman of atomic physics in postwar Germany and, in particular, joined the movement against the nuclear arms race ('*The Göttingen 18*,' together with Otto Hahn, Werner Heisenberg, and others). He died on 5 January 1970, in Göttingen.

Niels Bohr, who escaped from the German occupied Denmark at the end of September 1943 via England to the United States, was brought to Los Alamos for work on the American–British atomic bomb; at the same time, however, he also pleaded for the international control of the new weapon. After his return home in August 1945, he continued to investigate quantum-theoretical problems. Besides, he played a central role in the establishment of the European Centre for Nuclear Research (*CERN*). When the preparatory groups moved from Copenhagen to Geneva, the final seat of *CERN,* the Nordic Institute for Theoretical Physics (*NORDITA*) assumed its work in his institute at the Blegdamsvej. Bohr travelled widely and succeeded in reestablishing many prewar scientific relations, also with physicists in the Soviet Union and the Eastern European countries. He died on 18 November 1962 in Copenhagen.

Bohr's first opponent in the question of the interpretation of quantum mechanics, Erwin Schrödinger, left Berlin in summer 1933 and, after spending three years in Oxford, accepted a professorship in Graz until being dismissed in spring 1938 (as a consequence of the *Anschluß*—annexation—of Austria into the *Third Reich*). Via Rome, Oxford, and Belgium (University of Ghent, from the end of 1938 to September 1939), he arrived in Ireland where Eamon de Valera had installed him as Director of the School of Theoretical Physics at the Dublin Institute for Advanced Studies. There, he worked until 1956, mainly on field-theoretical and relativistic problems. Then, he returned to the free, unified Austria, where a distinguished professorship had been established for him at the University of Vienna through the good offices of his friend Hans Thirring. Having received many national and foreign honors, he died on 4 January 1961, in the city of his birth. The man, on whose idea of matter waves Schrödinger had built his wave mechanics, Louis de Broglie, lived a more quiet life in Paris until his death on 19 March 1987. He taught and worked undisturbed since 1929 at the *Institut Henri Poincaré* (created with the support of the Rockefeller Foundation); from 1932 to 1962, he also held a chair in theoretical physics at the University of Paris. He devoted the last decades of his life, as mentioned above in Section 3.2, to the interpretation of wave mechanics on the basis of his old (1927) idea of the 'double solution.'

While the influence of the second generation of quantum theorists weakened in the 1950s, the members of the third generation continued to play an active role, although two of them, John von Neumann and Wolfgang Pauli, died already in this decade. Pauli stayed in Princeton until summer 1946; then (as an American citizen—he became a Swiss citizen only in May 1949), he returned to his chair at the *ETH* in Zurich and tried to build a new school there, supported by a number of assistants beginning with Res Jost. Many foreign visitors, like Gunnar Källén from Sweden. Joaquin Luttinger from the United States, Walter Thirring from Austria, and Jan von Weyssenhoff from Poland joined him in Zurich, where fine Swiss disciples, such as Felix Villars, Robert Schafroth, Charles Enz, and Armin Thellung emerged from the Pauli school. He himself participated quite actively in the discussion of two topics: the new, renormalized quantum electrodynamics and the theory of superconductivity. In particular, he contributed to the analysis of discrete symmetries in elementary particle physics (leading to the *TCP*-theorem, the final version of his old spin-statistics theorem). Besides visiting regularly the Institute for Advanced Study in Princeton (where he was made a member from 1950 to 1955), he often travelled to meet foreign colleagues at institutions in Europe, America, and India. During the last years of his life, he again collaborated with Heisenberg on their common dream of a unified theory of elementary particles ('nonlinear spinor theory'); but once they had arrived at a possible scheme, he soon became dissatisfied with the solution (while Heisenberg carried on for years). He died on 15 December 1958, in Zurich in a room bearing the ominous number 137, the fine-structure constant introduced by his teacher Arnold Sommerfeld, which he had wished to derive from a consistent, final theory.

Even more than Pauli, Heisenberg became involved in the postwar reconstruction of scientific work, first in Germany and later on in Western Europe. After his return in January 1946 from an internment at Farm Hall, near Cambridge, England, Heisenberg reopened the *KWI für Physik* (formerly in Berlin and partly transferred during the war to Hechingen) in Göttingen as the *Max Planck-Institut für Physik*; he directed its working programme toward (mostly) the investigation of elementary-particle and astrophysical problems, since the access to nuclear physics and instrumentation was restricted in Germany for the next decade. Nearly from the very beginning, he eagerly supported and joined the West European activities toward establishing *CERN*, and he became the chairman of its Scientific Policy Committee. He played a decisive role in West Germany's science policy (in various capacities, as president of the *Deutsche Forschungstrat* and other advisory committees of the government), including the quest for (civilian use of) nuclear energy, both via fission and controlled fusion; on the other hand, he remained strictly opposed to nuclear rearmament of Germany. With respect to scientific work, he proposed after 1946 an unsuccessful approach to the problem of superconductivity, then he extended earlier ideas of multiparticle production; and finally he developed (partly in collaboration with Pauli) the unified field theory of elementary particles, based on symmetry (though not the later-used SU(3)-symmetry) and pioneering the idea of a degenerate vacuum. When he died on 1 February 1976, in Munich (he had been instrumental in moving his institute to the Bavarian capital in 1958—which had been extended to the *MPI für Physik und Astrophysik*, from which soon other Max Planck-Institutes for special physical fields, such as plasma physics, would split off)—the nonlinear spinor theory had not won much approval from the experts. However, the German and European sciences, in general, owed much to his early efforts in closing the gap with the leading centres of research in modern physics, in the United States and the Soviet Union.

Paul Dirac did not share the active role of his friends Heisenberg and Pauli on the stage of science politics—his only involvement in such matters had occurred already in the 1930s when he negotiated the British (Cavendish Laboratory) deal with the Soviet authorities for the transfer of Peter Kapitza's low-temperature laboratory to, and its installment in, the USSR. However, he remained very active in pure science and did not become much involved in war work (though he participated in some investigations on isotope separation), always retaining his independence both politically and privately. After the war, he travelled widely, to Canada, USA, India, and Western and Eastern Europe, to meet many friends again as well as to discuss his new ideas in quantum and relativity theories—which deviated increasingly from the mainstream of development, especially in particle physics (like renormalization theory). When in 1967 he retired from the Lucasian Professorship of Mathematics at Cambridge University (which he had occupied since 1932), Dirac joined the Center for Theoretical Studies at Coral Gables, Miami, for several months per year; after 1971, he remained for good at the Florida State University in Tallahassee. He died in Tallahassee on 20 October 1984.

The two Hungarian-born quantum pioneers, on the other hand, quite eagerly participated in the Manhattan Project in the USA during World War II. Especially, Eugene Wigner's work on constructing the plutonium reactors at Hanford contributed vitally to the nuclear-bomb program. After the war, he directed further research on nuclear power at the Oak Ridge National Laboratory (1946–1947), before he got back to pure research in theoretical physics in the chair of theoretical physics at Princeton University (which he had held since 1938); he now contributed especially to the group-theoretical treatment of nuclear and elementary particles problems (for which he shared the 1963 Nobel Prize in Physics), and he further concerned himself with the interpretation of quantum mechanics. Politically, Wigner took a strong stand as a stout anti-communist, and he believed strongly in the deterrent power of nuclear weapons. Still, he lived to see the dissolution of the Eastern Block, prior to his death on 1 January 1995, at Princeton. His friend since youth and colleague in Princeton, John von Neumann, who had shifted his attention in the 1930s to a variety of mathematical and physical problems, pioneered after 1945 the planning and construction of electronic computers and their application. Unfortunately, he died already on 8 February 1957 (in Washington), before the great age of computers really got started.

Once the final quantum theory governing the domain of atomic, molecular, and nuclear physics had been established, the new, fourth generation of quantum pioneers participated effectively and finally took over the main task of further applying and extending quantum mechanics. These were notably two Japanese, Yukawa and Tomonaga; two Russians, Landau and Bogoliubov; the Indian-born Chandrasekhar; and three Americans, Bardeen, Schwinger, and Feynman, who together characterized perfectly the trend of the internationalization of quantum theory since the 1930s, as described especially in Chapter IV and the Epilogue. When his meson-theory of nuclear forces (which would win him the 1949 Nobel Prize in Physics) gained general approval, Hideki Yukawa was appointed to the chair of theoretical physics at the Kyoto Imperial University. After World War II, he spent the years 1948 to 1953 in the United States as visiting professor at the Institute for Advanced Study in Princeton, and as a professor at Columbia University. Then, he was appointed head of the Institute for Fundamental Research in Kyoto (until 1977), where he died on 8 September 1981. Just as certain other senior pioneers of quantum theory (Dirac, Heisenberg, and Pauli, in particular) who had never accepted the renormalized quantum electrodynamics, regarding it as a purely calculational device, Yukawa never did accept the renormalized quantum electrodynamics but preferred a nonlocal quantum-field theory of elementary particles having internal structure. His friend Tomonaga, co-inventor of the renormalized *QED* (and co-winner of the 1965 Nobel Prize in Physics), also spent a year (in 1949–1950) at the Institute for Advanced Study in Princeton. Upon returning to Japan, he assumed several administrative positions, e.g., in the Science Council of Japan (succeeding Yoshio Nishina) and as President of Tokyo University of Education, and he engaged himself in the movement against the development of nuclear weapons. Having a weak physical constitution, he passed away in Tokyo on 8 July 1979.

Yukawa and Tomonaga (like Heisenberg) played a vital role in restructuring science in their home countries, which had been defeated in and impoverished by the war. Lev Landau and Nikolai Bogoliubov, on the other hand, became famous leaders of important schools of quantum theory in the victorious, powerful, and flourishing postwar Soviet Union. We have already related the continuing sick life, after his 1962 automobile accident, of Landau until his premature death in 1968. His colleague Bogoliubov, who was born on 21 August 1909, at Nishny Novgorod, Russia, and had joined the seminar of the well-known mathematician Nikolai M. Krylov (of the Soviet Academy of Sciences) in Kiev already at the age of 13, and obtained there his doctor's degree in 1930. From 1936 to 1950, he served as professor at Kiev and Moscow universities. Working first on developing the new field of nonlinear mechanics, he shifted his attention in the 1940s to the kinetic theory of fluids and the quantum description of superfluidity. From 1951 onward, he led the laboratory of theoretical physics at the Joint Institute for Nuclear Research in Dubna, which he would eventually direct from 1965 to 1989. During the 1950s, he formulated with his collaborators a successful axiomatic formulation of quantum field theory. When he died on 13 February 1992, the great Soviet Union was disintegrating. But both Landau and Bogoliubov had trained a large number of talented disciples, such as Evgeny Lifshitz and Dmitri Shirkov, who carried out research at many places in the Soviet Union, as well as in the new states emerging from and even beyond in the scientific world.

In the 1930s, quantum theory won strong support in the United States, partly supported by the exchange and input from Europe. Thus, well-reputed refugees enjoyed the freedom offered in the New World and created, together with a growing number of native-born scholars—many of whom had been trained by J. Robert Oppenheimer, who administered the Los Alamos Laboratory of the Manhattan Project during World War II and, after the war, headed the Insitute for Advanced Study in Princeton until he died on 20 February 1967—a most prosperous period for extending the domain of quantum theory. For example, S. Chandrasekhar, the young Indian-born pioneer of astrophysics, accepted a position in the astronomy department of the University of Chicago in 1936, where he continued to widen his research into further astronomical and physical problems (such as stellar dynamics, Brownian motion, and magnetohydrodynamic stability). He also turned into an influential teacher of modern subjects: once he counted two later Nobel Prize winners among his students, T. D. Lee and C. N. Yang. From 1952 to 1971, he edited the prestigious *Astrophysical Journal*, and in 1983, he received the Nobel Prize in Physics 'for his theoretical studies of the physical processes of importance to the structure and evolution of stars.' Until his death on 21 August 1995, in Chicago, he continued to write books on various themes (such as *Hydrodynamic and Hydromagnetic Stability* and *The Mathematical Theory of Black Holes*), to the latter of which he had turned his devoted interest.

Similarly productive at an early age as Chandrasekhar were the two, perhaps the most ingenious, American quantum physicists. Julian Schwinger had excelled in mathematical brilliance already in his teens and obtained his Ph.D. at the age of 20 from Columbia University. During the war, he contributed quite effectively to the U.S. radar project at the MIT Radiation Laboratory at Cambridge, Massachusetts, and since 1945, he belonged as professor in the faculty of Harvard University, which he would leave only in 1971 to join the University of California at Los Angeles (becoming *emeritus* in 1988). After deepening the pioneering work on *QED in* the 1950s, he turned in the 1960s to a new approach in elementary particles physics, the so-called 'Source Theory,' which should provide the problematic quantum field theory a sound basis. He was a brilliant lecturer and prolific author, and he attracted a large number of excellent doctoral students. When he died on 16 July 1994, at Los Angeles, he had survived Richard Feynman, his colleague and competitor in developing *QED* (who had passed away on 15 February 1988, also at Los Angeles) by over six years. The latter had moved already much earlier (in 1950) to California to accept a professorship at Caltech. In contrast to Schwinger, after the breakthrough in *QED*, he had turned to a variety of other fields in quantum physics, especially the theory of superfluidity, the theory of weak interactions, and other topics of particle theory. As we have seen above, he scored quite a few successes in these fields, notably, the (*V*-*A*) theory of weak interactions (which he modestly considered the only law of physics he had ever discovered) and the parton-structure of hadrons.

Through his intuitive genius and his pedagogical presentation of difficult physical problems (say, in his famous *Feynman Lectures* or on the Presidential Commission for the Challenger Catastrophe), Feynman became one of the most publicly known representatives of science in the second half of the 20th century. His fellow countryman John Bardeen, older by ten years, exhibited a less-outgoing, even shy personality. Still, he was remembered by students and collaborators as a warm-hearted man and great teacher, and by the world of science as an outstanding contributor to the quantum theory of solids; the first invention, in which he became involved, the transistor, did drastically change technology and life on earth; and the second, the *BCS* theory of superconductivity, led to a wealth of new, fundamental applications in the developing field of (microscopic) many-body physics—thus, the second Nobel Prize for Physics awarded to him was fully justified. In contrast to the two younger colleagues, Bardeen did not start out as an infant-prodigy; he only graduated in 1928, after receiving a sound education at the University of Wisconsin, Madison, with a B.S. degree in electrical engineering. Then, during the next three years, he worked at the Gulf Research Laboratories in Pittsburgh, Pennsylvania, on developing physical methods for prospecting oil. In 1933, he resumed studies in mathematical physics at Princeton University, completing (in 1936) a Ph.D. thesis on solid-state theory under Eugene Wigner. Afterwards, he went as a Junior Fellow to the Society of Fellows at Harvard University, where he collaborated with John H. Van Vleck and Percy Bridgman, and finally started an academic career at the University of Minnesota, which was interrupted by military service at the Naval Ordnance Laboratory in Washington, D.C. In 1946, he decided to take up a well-paid position (because he had to support a family) in the newly created solid-state research group under William Shockley at Bell Labs; already, the first project, the use of solid-state amplifiers, led to the invention of the transistor. However, in 1951, he went back to university life, accepting a dual professorship in physics and electrical engineering at the University of Illinois in Urbana. There, he returned to one of his prewar problems, the riddle of superconductivity, which he had treated unsuccessfully in the late 1930, but also the second attempt in the early 1950s seemed to lead astray; however, the third attempt—after writing the *Handbuch* article on the subject—with Leon N. Cooper and John R. Schrieffer led to the final breakthrough. He stayed at Urbana (until he died there on 30 January 1991) and had the pleasure of seeing much of the further progress stimulated by the *BCS* theory.

From the generations of quantum physicists, which we have pursued in even a more rapid sequence after these pioneers, many personalities stick out, and it becomes more and more difficult to select the most important ones. Certainly, in the field of particle physics, which appeared to be the most fundamental one, physicists like the Americans Murray Gell-Mann (born 1929) and Steven Weinberg (born 1933), the Japanese Yoichiro Nambu (born 1924), or the Pakistani Abdus Salam (1926–1996) exerted a great influence on the development of our present views, similar to the American Philip W. Anderson (born 1929) in solid-state theory. Numerous younger researchers have picked up the torch of quantum theory and carried it further to confirm it as *the* outstanding physical scheme of the 20th century. Does this mean that now, at the end of this glorious period of the scientific enterprize, all the secrets of nature are known? And further, have we come closer to the dream of some of the greatest natural philosophers of all time, to achieve a single unified theory of matter? Friedrich Hund, himself one of the great pioneers of quantum theory and also a most conscientious witness and historian of his times, stated a few years before his death (on 31 March 1997, when he was more than 101 years old) in Göttingen (where he had returned in 1957 as a later successor to his teacher Born): Nature is ‘intelligible (*begreifbar*)’ though not necessarily on a ‘unified foundation’ or theory. Perhaps the whole question for a ‘theory of everything,’ even if formulated essentially with the help of quantum theory, will remain an illusion, in spite of all the wonderful achievements of quantum theory. Whatever the future may bring, quantum theory will certainly continue to stand out as an intellectual triumph of man to achieve an adequate description of natural phenomena, which yielded with an unprecedented accuracy the deepest insights thus far into the structure of matter.

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Note: We have used the following abbreviations for journals and periodicals. Journals and periodicals not mentioned here are cited by their full titles. In the list of references, we have tried to give, if available, dates of submission or receipt of the papers (letters) and of the issues in which the papers were published—except of the *Sitzungsberichte* or *Comptes rendus.* Of some important papers in other languages, English translations are also given. Although we did so in the other volumes, here we do not refer to reprints of the papers in most cases, especially when collected works of the authors have been published (which are referred to as well).

The references are arranged generally in alphabetical order according to the names of the authors, and generally in the time-order of the publications by letters added to the dates; the time-order letters (a, b, c, ...) refer to all papers of an author cited in Volumes 1 to 6.

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As an example, consider the entry 'Reid, Alexander **1:**626, **5:**866, **6:**246, 375–*378*, 583'. References to Alexander Reid occur in Volume 1 on page 626, in Volume 5 on page 866, and in Volume 6 on pages 246, 375–378, and 583. Biographical information is given in Volume 6 on page 378.

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