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Summary

Heisenberg, in constructing quantum mechanics, explicitly followed certain principles exemplified, as he believed, in Einstein's construction of the special theory of relativity which for him was the paradigm for radical theoretic change in physics. These were the principles of (i) scientific realism, (ii) stability of background knowledge, (iii) E-observability, (iv) contextual re-interpretation, (v) pragmatic continuity, (vi) model continuity, (vii) simplicity. Fifty years later, in retrospect, Heisenberg added the following two: (viii) a principle of non-proliferation of competing theories — scientific revolutions are not a legitimate goal of physics — and (ix) a principle of tenacity — existing theories are to be conserved as far as possible. The conservative as well as the revolutionary potential of these principles is then discussed. A more penetrating philosophical criticism of these principles is postponed.

I wish to address myself to the question of rational research-guiding principles during episodes of radical theoretic change. I assume that radical theoretic changes occur in science and that the discovery of relativity (special and general) and of quantum mechanics were instancers of such changes. I do not wish, however, to endorse any particular account or explanation of those changes; rather my purpose is to attempt to criticize and improve the accounts and explanations that are at present available. I shall occasionally use, when convenient, terminology borrowed from contemporary literature on the topic of growth and development of scientific knowlegde, as, for example, Kuhn's distinction between 'revolutionary' and 'normal' science, or Radnitzky's 'territory', 'internal' or 'external steering field', etc., but in so doing I wish merely to refer to certain identifiable phenomena (to the extent that this can be done) without endorsing the theories of which these terms may also be a part.

Among those who admit in some sense the existence of radical theoretic change in science are, on the one hand, those who adhere to Reichenbach's distinction between the context of discovery and the context of justification: rational principles, they would say, apply only to the latter. Among this group, for example, are most of the adherents of the Received View in the philosophy of science[1], as well as K. Popper, T. S. Kuhn, P.

[1] The term "Received View", coined by H. Putnam, has gained considerable circulation for the tradition of the philosophy of science inspired by the Vienna Circle and Reichenbach's Berlin School. For an excellent summary of criticisms of the Received View and for the existing available alternatives, see the introduction by F. Suppe, in F. Suppe (ed.). *The Structure of Scientific Theories*, pp. 3—243 (Urbana, Ill., University of Illinois Press, 1974).

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Feyerabend, I. Lakatos and pragmatists and historicists in general[2]. All would deny that the process of discovery of radically new theories is something to which rational research-guiding principles apply: the facts and circumstances of discovery are, they would say, of interest only to psychologists, historians and sociologists. On the other hand, there are those adherents of the Received View, like I. Scheffler, and many historically-oriented philosophers of sience, like D. Shapere, R. N. Hanson, as well as praxiologists of sience, notably G. Radnitzky, and systems theorists of research, such as H. Tornebohm and E. Laszlo,[3] who affirm the existence of rational research-guiding principles for growth in knowledge, even for instances of radical theoretic change.

Using Kuhn's[3a] distinction between 'normal' and 'revolutionary' science, there exists, he says, for normal science a disciplinary matrix and exemplar instances of research that together control the growth of and impose their form of rationality on the process and product of change: this paradigm is precisely a mode for the progressive and rational acquisition of (antecedently categorized) knowledge. For revolutionary science, however, defined as radical theoretic change with a resulting change of paradigm, there is the twofold question of (a) rationality (or lawfulness) of the process of change and, (b) the rationality of (or warrant for) the product of change. The following are the three possibilities: (i) there are no rules for (a), only for (b); (ii) there are some necessary, perhaps sufficient, principles for (a), but not sufficient for (b); (iii) there are a sufficient set for (a) which are also sufficient for (b).

The case history that will be studied in this paper is that of Heisenberg's discovery of quantum mechanics, which it will be assumed is an instance of radical theoretic change in physics. It is clear from the record that Heisenberg sought guidance in the model of theoretic change brought about in physics by Einstein, also assumed by me to be an instance of radical change. It is clear that for him the rational warrant of quantum mechanics — the product of change — included as necessary conditions, fulfillment of the principles of the model. It is arguable, however, whether the principles of the model were or were not in his view sufficient to guarantee of themselves the acceptability of the product of

[2] K. Popper, *Objective Knowledge* (London, Oxford Univ. Press, 1972); T. S. Kuhn, *Structure of Scientific Revolutions* (Chicago, Univ. Press, 1962); I. Lakatos and A. Musgrave, *Criticism and Growth of Knowledge* (Cambridge Univ. Press, London, 1970); I. Lakatos, "History of Science and its rational reconstruction", *Boston Studies in the Philosophy of Science*, vol. 3, ed. by M. Wartofsky and R. S. Cohen (New York, Humanities Press, 1967), pp. 91—136; P. Feyerabend, "Problems of Empiricism, I" in Colodny, R. (ed.) *Beyond the Edge of Certainty* (Pittsburgh, Pittsburgh Univ. Press, 1965), pp. 145—260, "Problems of Empiricism II", in Colodny, R. (ed.), *Nature and Function of Scientific Theories* (Pittsburgh, Univ. of Pittsburgh Press, 1970), pp. 275—355.

[3] An excellent bibliography of the mentioned authors is contained in F. Suppe (ed.), *The Structure of Scientific Theories, op. cit.*, and in a review article by T. Kisiel with G. Johnson, "New Philosophies of Science in the USA: Selective Survey", *Zeit. f. Allgemeine Wissenschaftstheorie, 5* (1974), pp. 138—191.

[3a] T. S. Kuhn, *Structure of Scientific Revolutions, op. cit.*

change. The fact is that Heisenberg at the time he was fashioning quantum mechanics was led by certain research-guiding principles which we shall attempt to formulate below. To this list will be added certain others which he came to affirm in the retrospect of fifty years later but which were not part — as far as one can judge — of the original set. It will be held by some that these facts are of interest only to psychologists and others, but not to methodologists and philosophers of science. Since it is my claim, however, that they are indeed of interest to the philosopher and methodologists of science, I shall attempt in another paper to study these principles critically and show to what extent they may possibly lay claim to universal validity for radical theoretic change in science.

Historical Context

In the years just preceding 1925, physics was in a state of crisis. There were major significant unresolved anomalies, which strongly suggested that a breakthrough, when it occurred, would result in radical changes in the edifice of physics. Heisenberg was one of those convinced that a scientific revolution had to take place and, looking for a model of such a revolution, he found one already at hand in Einstein's work on relativity, particularly on special relativity, still at that time the focus of residual controversy[4]. For example, that none but observables must go into a

[4] Published and unpublished sources will be used in this study. The unpublished sources are in the *Archive for the History of Quantum Physics* (hereafter referred to as AHQP) compiled and maintained by a Joint Committee of the *American Physical Society* and the *American Philosophical Society*, Philadelphia, on the History of Theoretical Physics in the Twentieth Century. The *Archive* is deposited, in original or duplicate form, at the Library of the American Philosophical Society, Philadelphia, at the Bancroft Library of the University of California, Berkeley, at the Niels Bohr Library, American Institute of Physics, New York, and at the Universitets Institut for Teoretisk Fysik, Copenhagen, Denmark. The Archive contains documents on the history of quantum physics and taped interviews conducted by T. S. Kuhn, J. L. Heilbron and others with Heisenberg, Niels Bohr and other quantum physicists. All the interviews cited or referred to were with T. S. Kuhn. Reference and citation will be by date of interview. The writer was enabled to consult this material by permission of the Joint Committee referred to above through the courtesy of Dr. Charles Weiner, formerly Director of the *Center for the History and Philosophy of Physics*, a division of the AIP, New York, and Mrs Joan Warnow, presently Acting Director of the *Center*. Permission to quote from the unpublished material was kindly given by Professor Werner Heisenberg and the Joint Committee. The principal published sources for the biographical material are
W. Heisenberg, "Erinnerungen an die Zeit der Entwicklung der Quantum Mechanik", in *Theoretical Physics in the Twentieth Century: Memorial Volume to Wolfgang Pauli*, ed. by M. Fierz and J. F. Weisskopf (New York, Interscience, 1960), pp. 40—7; "The Development of the Interpretation of Physics", in *Niels Bvhr and the Developmeut of Physics*, ed. by W. Pauli (New York, McGraw-Hill, 1955), pp. 12—29; *Physics and Philosophy* (New York, Harper and Row, World Perspectives Series, vol. 19, 1958) — hereafter referred to as PP; *Physics and Beyond*, (Harper and Row, World Perspectives Series, vol. 42, 1971) — hereafter referred to as PB; *Across the Frontiers*, trans., from the German by Peter Heath (Harper und Row, World Perspectives, Series, vol. 48, 1974) — hereafter referred to as AF.

physical theory, was an insight he derived from Einstein[5]. Likewise, his strategy of using the formalism of quantum mechanics to re-interpret the kinematical variables instead of accepting their traditional sense from classical physics, was derived from Einstein[6]. A comparison between relativity and quantum mechanics as they relate to classical physics brings out additional resemblances between the two great scientific revolutions of this century. In this paper, I shall try to formulate the principles for a scientific revolution that Heisenberg read into his study of relativity, and used in the discovery of quantum mechanics.

Let us first focus on the revolutionary strategy used by Einstein. At the turn of the century, the four main theoretical divisions of physics were mechanics, electromagnetics, thermodynamics and gravitational theory. Each theory described a certain kind of system in space and time, and gave mathematical laws for its dynamical development. Mechanics dealt with mass particles and continuous ponderable media. Electromagnetics dealt with electric charges, magnetic dipoles and electromagnetic fields. Einstein noted the puzzling fact that mechanical and electromagnetic laws have different space-time properties. Mechanical laws, on the one hand, are covariant[7] relative to Galilean frames[8]; electromagnetic laws, on the other hand, are covariant relative to

[5] AHQP, 15 February, 1963: Heisenberg said that the importance of observables in physics came to him from relativity, reflecting how Einstein converted "apparent time" into "real time". This turning of the picture by saying that the real things are those which you observe and everything else is nothing, was, he asserts, in the minds of the people at Göttingen at that time.

[6] AHQP, 25 February 1963: Heisenberg said about the discovery of the Uncertainty Principle (1927) "I tried to say what space meant and what velocity meant and so on. I just tried to turn around the question according to the example of Einstein. You know Einstein just reversed the question by saying 'We do not ask how we describe nature by mathematical structures, but we say that nature always makes so that the mathematical scheme can be fitted to it. That is, you find in nature only situations which can be described by means of the Lorentz transformation. Therefore, I just suggested to 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 the situations in which there was this Uncertainty Relation between p and q".

[7] Equations are *covariant* relative to a certain group of transformations if they are form-invariant with respect to transformations of the group. For a study of the covariances of Newtonian and relativistic physics, see M. Strauss, "Einstein's Theories and the Critics of Newton", *Synthese*, xviii (1968), 251—84.

[8] The Galilean transformation group comprises (i) spatial and temporal displacements of the form: $x_i \to x_i' = x_i + a_i$ ($i = 1, 2, 3$), $t \to t' = t + t_0$ (ii) three-dimensional spatial orthogonal rotations of the form $x_i \to x_i' = c_{ij}x_j$ ($i = 1, 2, 3$: the dummy index is summed); (iii) uniform motions in a straight line: $x_j \to x_i' = x_i' + v_i t$, $t \to t' = t$. Every transformation of the spatial or temporal frame can be regarded either passively or actively, cf. E. Wigner, *Progr. Theor. Phys.*, ii (1954), p. 437, and his *Symmetries and Reflections* (Bloomington: Univ. of Indiana Press, 1967), p. 45. I am concerned with the passive interpretation of space-time transformations.

Lorentz frames[9]. But a Galilean frame is not usually a Lorentz frame, except in the instances where both coincide with (different coordinatizations of) absolute space and absolute time, the unique objective containers of physical objects and events.

Absolute space and time, however, eluded all experimental attempts to determine their relation to the frames actually used in science and everyday life. They remained hidden. To explain this puzzling phenomenon, two quite different strategies were used, one by Lorentz and Fitzgerald and the other by Einstein. The conservative strategy proposed by Lorentz and Fitzgerald was to explain the negative results of these experiments by a universal contraction of rods and a universal dilation of time intervals in inertial frames other than that of absolute space and time[10]. This enabled the traditional relationships between space, time and mechanics to be maintained. A revolutionary strategy, however, was proposed by Einstein[11]. It had four parts: (1) he denied reality-status to absolute space and absolute time; (2) in the order of logic (semantics) and meaning, he re-interpreted the state variables (coordinates and momenta) as logically (semantically) relative to members of a unique set of local frames of reference; (3) in the order of the ontology of space and time, he proposed to locate their reality in a unique group structure of local spatio-temporal frames of reference, which group he

[9] The inhomogeneious Lorentz or Poincare group comprises (i) spacetime displacements of the form, $x_i \rightarrow x'_i = x_i + a_i$ ($i = 1, 2, 3, 4$: where x_4 is the time coordinate); (ii) three-dimensional spatial orthogonal rotations; (iii) four-dimensional space-time orthogonal rotations, i.e., real linear transformation which leave invariant the squared space-time interval $(a_1 - b_1)^2 + (a_2 - b_2)^2 + (a_3 - b_3)^2 - c^2(a_4 - b_4)^2$ separating two events whose space-time coordinates are (a_1, a_2, a_3, a_4) and (b_1, b_2, b_3, b_4). These last transformations relate two space-time frames of such a kind that the spatial part of one is moving with uniform relative velocity in a straight line relative to the spatial part of the other. I am concerned only with the passive form of the transformations.

[10] H. A. Lorentz, "Electromagnetic Phenomena in a System Moving with any Velocity less than that of Light", *Proc. Acad. Sci. Amsterdam*, vi (1904), reprinted in *The Principle of Relativity* by H. A. Lorentz, A. Einstein, H. Minkowski and H. Weyl (Dover, New York), pp. 10—36.

[11] A. Einstein, "On the Electrodynamics of Moving Bodies" *Annalen der Physik* xvii (1905), translated and reprinted in *The Principle of Relativity, op. cit.*, pp. 37—65. The language of the paper is more phenomenological than ontological, due perhaps to the influence of Mach's "economy of thought" at this period of Einstein's career. It is clear from his "Autobiographical Notes", published in *Albert Einstein: Philosopher-Scientist*, ed. by P. Schilpp (Library of Living Philosophers, Edmonton, Ill., 1949), pp. 53—63, and other of his writings that Einstein later at least (and certainly before 1915 and general relativity) interpreted the principle of relativity in a realistic way, as asserting something about nature. He also foresaw special relativity giving way to a unified field theory in which there would be no longer any priviliged space-time frames or arbitrary constants of nature. Such a transposition, he held, would nevertheless preserve special relativity as a limiting case of the unified theory. Heisenberg's view of Einstein's accomplishment is given in his essay "The Scientific Work of Albert Einstein", AF, pp. 1—7. Heisenberg stressed here the re-interpretation of space-time variables and the consequent change in the ontology of space-time.

took to be that of the inhomogeneous Lorentz group; (4) in the order of the ontology of nature, he proposed to locate physical objects, events and structures in and only in the contents of physical theories that were covariant relative to the inhomogeneous Lorentz group. The outcome of these proposals, specifically of (4), was a change in the laws of mechanics.

Einstein's proposal was a revolutionary strategy chiefly for three reasons: it made changes in the laws of mechanics, laws thought to have been established immutably by Newton; it gave a semantical re-interpretation to the basic kinematical variables of mechanics, and it changed the notion of scientific objectivity. Heisenberg later spoke of Einstein's "stroke of wizardry" resolving all difficulties by simply stating that the "apparent time of Lorentz's formulae is actually the real time"[12]. The revision of the ontology of the relativistic domain was itself only the consequence of a process of theory change that incorporated, and presumably was intended to incorporate, the principles which give warrant to the process and hence to the product of change. Let me try to formulate these principles — principles for the rational revolutionary development of physics as Heisenberg saw them exemplified in the relativity model of radical theoretical change.

The *terminus a quo* is classical physics. Let L_N be the language in which is embodied the descriptive ontology of classical mechanics. Fortunately, L_N is a formal language: its basic entities and predicates are clearly designated in its mathematical form and their mutual relationships are implicitly defined through the mathematical equations of motion.

The ontology of classical mechanics is what Heisenberg called "the ontology of materialism"[13]. This is the ontology of objects that possess, he says, a special kind of objectivity[14], independence of the conditions under which they come to be known by particular observers[15]. Let me

[12] AF, p. 2. The terms 'appearance' and 'reality' have various usages, and it is not clear which of them Heisenberg intended. Among the usages; for example, (i) phenomenon (for Kant) as opposed to noumenon, (ii) phenomenon as perceptually given as opposed to the same phenomenon as scientifically explained; (iii) experience described in an unendorsed (or obsolete or uncritical) frame as opposed to experience described in an endorsed (or currently acceptable or critical) frame, etc. According to sense (iii), the length contraction would have been real for Lorentz but, according to Einstein, uncritically so: only from the relativity perspective, would the Lorentz contraction be seen as a mere appearance of what is not really the way it appears. Alternatively, one might use sense (ii): then the Lorentz contraction is taken as observationally given (truly described) in some way antecedent to the scientific explanation that accrues to it from relativity, Cf. the author's "Horizon, Objectivity and Reality in the Physical Sciences", *Internat. Philos. Qrtly*, vii (1967), pp. 375—412 for a discussion of *reality* and *appearance*.

[13] PP, pp. 81—2.

[14] The word "object" is ambiguous: it can refer to the entity that is represented or to the representation of that entity by a knower. Objectivity characterizes the former only in virtue of certain features of the latter. Objectivity primarily applies to the objects of knowledge *within acts of knowledge*.

[15] PP, p. 81. "We 'objectivate' a statement if we claim that its contents does not depend on the conditions under which it can be verified". Cf. also p. 130.

call these *objectifiable* objects[16]. The physical systems of classical physics — localized exactly in absolute Euclidean 3-space independently of all observers — are paradigm cases of objectifiable objects.

Although the state variables of a classical system are defined with respect to absolute space and time, local inertial coordinates relative, say, to a laboratory, are as good, since they too satisfy the dynamical equations. A local inertial frame, unlike absolute space, is recognizable; it is generally embodied concretely in the environment surrounding the object. This environment, like the laboratory, is the physical context with respect to which the relative state variables are defined. For Einstein, it was sufficient that a relative state variable referred *notionally* to the physical context which concretely embodied an inertial frame of reference. For Heisenberg, the relationship between object and frame of reference had to be *real*, founded on a measuring process, which is a special kind of interaction that leaves a macroscopic trace on a measuring instrument[17].

[16] An object comes to be known when two conditions are fulfilled: (1) when it is conceptually categorized (or becomes subject to clear semantical description); (2) when it is recognized by particular observers in particular instances. An objectifiable object then is one which has a two fold independence from observers: (i) a *logical* (or *semantical*) independence, and (ii) a real independence. An object is *logically* (or *semantically*) independent of observers if its notion does not logically involve any set of observers (whether these be instrumental contexts or human perceivers) or if its descriptive framework does not refer to a class of observers or speakers (instrumental or perceptual). Absolute position and velocity would fulfil this condition (but not relative position and velocity); also properties invariant for all observers, such as for example, rest mass or the velocity of light. An object is *really independent* of observers if it does not depend for the fact of its existence on the activity, for example, of measuring or observing, of any class of observers or speakers. There are two ways in which the fact of existence of an object can come to depend on the activity of an observer: first of all, when the object is the real product of that activity, as, for example, in participant-observer situations in the social sciences, or in the physical sciences, for such properties as are the product of an interaction with a standard set-up that acts simultaneously as a measurement frame. According to one (re-)interpretation, *position* is the product of a localizing (inter)action with (one of a class of) macroscopic instrumental reference frames; that is, in so far as it is relative, position is according to this (re-)interpretation not a merely *notional* relation but a *real* relation founded on the action of the reference frame on it. A second kind of existential dependence on observers arises when the object has existence only in virtue of being recognized by the observer, such as, for example, warm-as-felt, or red-as-sensed, etc.; according to one interpretation of the reduction of the wave packet, quantum mechanical variables also belong to this class. Cf. for example, F. London and E. Bauer, *La théorie de l'observation dans la physique quantique* (Paris, Hermann, 1939), and Eugene Wigner, "Remarks on the Mind-Body Problem", in *The Scientist Speculates*, ed. by I. J. Good (London, 1962), pp. 284—301. The physical systems of classical physics are paradigm cases of *objectifiable objects*. Relativity and quantum mechanics were to change, not merely the kinds of objects admitted to be real, but also the nature of scientific objectivity.

[17] Cf. Patrick Heelan, *Quantum Mechanics and Objectivity: A Study of the Physical Philosophy of Werner Heisenberg* (The Hague, Nijhoff, 1965) — hereafter referred to as QMO — pp. 64—71. In relativity theory, Einstein only went so far as to re-define *position* as *conceptually* relative to frames of reference. M. Sachs notes: "it was tacitly assumed that an *outside* observer will always have at its disposal a set of measuring rods and clocks — to probe the properties of the universe (as closely as he pleases!) . . . [but] these investiga-

Since local environments embody inertial frames, it becomes important to learn to categorize them as similar or dissimilar for the purposes of estimating relative state variables. Do we need descriptive frameworks other than L_N for this purpose? At first sight, yes! since what has to be determined are precisely the conditions under which L_N is applicable. Let us call the knowledge systems required for this task, *stable background knowledge relative to the applicability of* L_N. But might not L_N be part of its own stable background knowledge system? Paradoxically, the answer is yes!, provided L_N can serve as an observational language, since an observational language — one used directly and immediately of experience — does not need the mediation of any other language for its valid use. But is L_N an observation language? Einstein and Heisenberg, following the Kantian (or Neo-Kantian) tradition, took the position that L_N is both in fact and in principle an unmediated observational language, a refinement of natural language and the one and only language appropriate for the experience of all everyday physical objects[18].

I should like to compare this with another view, the *pragmatic theory of observation* which holds that, through use and familiarity, a theoretical language framework like L_N can become appropriated for observational use[19]. The reasons in favor of some version of the pragmatic theory of observation are based on the analysis of the subject-object cut for embodied observers like human perceivers[20]. To point up the philosophical problem of a stable background observational language, let me consider

tions did not attempt to explicitly incorporate the measuring processes", p. 59 in "The Elementarity of Maesurement in Relativity", in *Boston Studies in the Philosophy of Science*, Vol. iii, ed. by R. S. Cohen and M. Wartofsky (New York, Humanities Press, 1968), pp. 56—80. Cf. QMO, pp. 73, 76—111.

[18] Heisenberg writes: "The concepts of classical physics will remain the basis of any exact and objective science. Because we demand of the results of science that they can be objectively proved (i.e., by measurements registered on suitable apparatus) we are forced to express these results in the language of classical physics ...; Thus while the *laws* of classical physics ... appear only as limiting cases of more general and abstract connections the concepts associated with these laws remain an indispensable part of the language of science without which it would not be possible even to speak of scientific results", *Philosophic Problems of Nuclear Science* (London, Faber and Faber, 1952), p. 45. The same idea is expressed in Heisenberg, *Physical Principles of the Quantum Theory* (New York, Dover), pp. 1—4, 11, 62—4 and passim, where it is supposed that the descriptive categories of classical physics are identical with those of everyday language; also in PP, pp. 44, 144; PB, pp. 64, 130. See also QMO, chap. iv.

[19] For a recent exposition of this viewpoint, see P. Feyerabend, "Problems of Empiricism I", in *Beyond the Edge of Certainty* ed. by R. Colodny (Pittsburgh Univ. Press, 1965), pp. 145—260.

[20] The analysis of the subject-object cut especially in acts of observation, leads to an epistemology that permits the displacement of the S—O cut so that the measuring instrument becomes functionally part of the observing subject. Observational languages then are context-dependent: where the context of observation depends on the location of the S—O cut. Cf. the author's "Hermeneutics of Experimental Science in the Context of the Life-World", *Philosophia Mathematica*, (1972), pp. 101—44 and the commentary by T. Kisiel in *Zeit. f. Allgem. Wissenschaftstheorie* 5 (1974), pp. 124—134 where this topic is discussed.

the paradigm case of the transformation in everyday language effected by the Copernican revolution.

The ontological commitments of pre-Copernican man were carried in his observational language which I shall call, Aristotelian language, L_A. In L_A, the observer takes the soil on which he stands as fixed and immobile. We find echoes of this pre-Copernican frame in such common expressions as, "The sun rises in the morning and sets in the evening". In L_A, the sun's motion is real. If asked to interpret this expression today, we merely say that we are using a conventional phrase to mean that the sun *merely appears* to rise and set. The sun's motion, real in the earth-centered frame, L_A, becomes merely an appearance of motion in the heliocentric frame of L_N, in which the sun is the fixed and immobile center of the solar system. A radical change in the ontological commitments of everyday language relative to motion followed the replacement of L_A by L_N as the language of everyday observation. Was this the conversion of L_N from a theoretical to an observational language? Or was it the discovery of the uniquely correct observational language for perceptual experience? My view is the former, Heisenberg's the latter.

Whence comes the warrant for the ontological commitments of language? Is it a matter merely of taste or convention, as some would hold? Or is it the product merely of historical and sociological factors, authorities and the like? We are concerned not with such reasons, but with possible philosophical grounds for rejecting the Aristotelian usage and adopting in its place the modern usage incorporating the Copernican viewpoint.

Scientific Realism

Einstein believed that philosophical principles were involved in the choice of an ontology. Ontological assertions about Nature, he held, are not validly made in any pre-theoretical language, but only within the theoretical (mathematically explanatory) linguistic framework of a physical theory[21]. Of the variety of conventionally sanctioned frameworks actually used to describe the world, only one in his view described what was really the case: that was the scientific frame. The Aristotelian linguistic frame, or what Wilfred Sellars calls[22], the "manifest image of the world", spoke for him, as for Sellars, merely of a phenomenal realm that was destined to give way to the reality to be revealed by the correct theoretical description. Heisenberg's view was more complex: from Pythagoras, Plato and Democritus came the impulse to explain immediate and direct perceptual qualities by the geometrical and other mathe-

[21] A. Einstein, "Physics and Reality", (1936) reprinted in *Out of My Later Years* (New York, Philosophical Library, 1950) pp. 59—97; "Autobiographical Notes", *op. cit.* pp. 20—21, 48—9.

[22] Wilfred Sellars, "Philosophy and the Scientific Image", in *Science, Perception and Reality* (London, Routledge and Kegan Paul, 1963), pp. 1—40.

matical properties of fundamental non-perceptual entities, atoms and the like:[23] the history of modern science is the story of the successful growth of a mathematical explanation of nature. Contrasting mathematical methods with perception-theoretic methods (such as, for example Goethe's) he asserts, "the attempt to prove impossible a perception-theoretic understanding... and to prove mathematical analysis the only possible way, appears to me as unwise as the opposite assertion that the understanding of nature can be achieved in a philosophical way without the knowledge of its formal laws"...[24] Scientific language then though formal, does not replace ordinary language; rather is it to be considered as the "natural extension of ordinary language"[25]. To the mathematical symbols of the theory are given names that refer to experiments or (measurement-) processes describable in ordinary language. Classical physics, moreover, is a precise rendering of the world as given in ordinary perceptual experience, "The words of [our] language represent the concepts of daily life which in the scientific language of physics may be refined to the concepts of classical physics"[26].

For Heisenberg then the scientific image of the world is certainly real, but it is not exhaustive of reality — there are, as he asserts, values, the works of the human spirit, religion, none of which enter into physics[27]. But in the philosophy of physical nature, the scientific image has primacy[28]. He applied to Max Planck his description of the perfect scientist, "[his] gaze ... was directed to the still distant summit of atomic theory whence it would be possible to discern not only the existence of elementary particles and all the atomic products composed of them, but also indirectly the physical interconnections of the world in general, as a consequence of a simple mathematical structure"[29]. That was the goal he shared with Einstein and Planck grounded on scientific realism[30].

[23] W. Heisenberg, *Philosophic Problems, op. cit.*, p. 29.

[24] Ibid., p. 40. [25] PP, p. 173. [26] PP, p. 144.

[27] AF, chaps. xiii and xvi; PB, chaps. vii and xvii.

[28] For Heisenberg, the mathematical structure of a physical theory tells how nature really is: this he learned from Einstein. Language, however, is tied to everyday experience, since it is the product of conventions and historical process. At first (1925—6), he tried to force on the language of physics a semantical re-interpretation to make it conform to the quantum mechanical formalism; but Bohr convinced him (early 1927) that language need not be so re-interpreted; its old usages could be retained provided it was understood that the meaning of the old terms was sufficiently vague and imprecise. Besides language (or words), there were *intuitions* or *pictures*, roughly intuitive classical models; language uses these pictures realistically, of the macroscopic everyday world, but in quantum mechanics, a variety of complementary pictures is used to adapt ordinary language to the purposes of scientific expression; the reality expressed, however, is isomorphic, not with the picture or the language, but with the mathematical schemes, (or meaning); cf. Heisenberg, *Physical Principles, op. cit.*, p. 11, and notes 44, 61, 65 and 68 below.

[29] AF, p. 14.

[30] Heisenberg emphatically believes that physics, though necessarily expressed in terms of mathematical structures, depends in an important way on physical intuitions into possible experiments. He criticizes Born, Wightman, Symanski and others for being too mathematically oriented. "First solve the physics", he says echoing Bohr, "and then find

Stable Background Knowledge

For Einstein[31], however, as for Heisenberg, there was — despite revolutionary changes in physics — a certain unrevisability about L_N, in so far as, they believed, L_N is in principle a priori to all subsequent theories in physics[32]. L_N was, in their view, the only correct language in principle for everyday macroscopic non-relativistic phenomena, the phenomena of daily life. Everyday language then, for them, incorporated L_N immutably for the domain of macroscopic non-relativistic phenomena, such as the instruments and signals that constitute the everyday face of all the phenomena large and small, scientific and non-scientific[33]. It should be noted that everyday language is neutral to the transposition from L_N to L_R (the language of relativity) in the domain of relativistic phenomena only, not in the realm of everyday phenomena, where L_N is assured of a permanent place in principle. The reason given for the unrevisability of L_N is the alleged necessity — for the purposes of scientific inquiry — of L_N to describe the process and results of experimentation.

Heisenberg goes further in specifying the background knowledge for scientific inquiry. As a field of scientific inquiry develops, every phase of the inquiry rests on the stable past accomplishments of the field, particularly on what he calls, *Closed Theories*, that is, theories closed-off from internal development by reason of the fact that they have achieved axiomatic form[34]. Such closed theories have a non-empty domain of

the mathematical tools". (AHQP, 22 February 1963). "Such terms as 'the stability of the atom' or 'the quantum condition' give a different style to physics ... more difficult ...; forgetting about mathematical schemes, one comes to a kind of substance of things which one is inclined to forget if one works in the mathematics alone ... [However] it is difficult to describe physics without having the logical connection [of mathematics]. Still by doing so, one is forced to think very carefully about what will happen in ... experiments". (AHQP, 28 February 1965).

[31] Heisenberg reports that Einstein held that the valid applicability of the old physics is a necessary condition for the observation of facts in the new physics; PB, pp. 63—4.

[32] AHQP, 17 February 1963: Heisenberg said, "Quantum theory ... certainly includes Newtonian physics in some way ... The concepts for quantum mechanics can only be explained by already knowing the Newtonian concepts; ... I would say that *Newtonian mechanics is a kind of a priori for quantum theory* ... in that sense that it is that language which enables us to say what we observe. If we have not the language of classical physics, I don't know how we should speak about our experiences" (italics inserted). See note 18 for other references.

[33] Nevertheless, Heisenberg says that L_N is the "product of the historical process" (AHQP, 17 February, 1963). Such a comment reveals, I believe, the Neo-Kantian influence on Heisenberg's thinking. This would have come from Hermann Weyl, Einstein and the contemporary academic milieu in Germany. Kuhn forces some of the conflicting elements in Heisenberg's thinking into the open in interviews of 17 and 28 February, 1963, AHQP.

[34] Heisenberg's belief in the unrevisable cumulative character of scientific knowledge is expressed in his notion of *closed theory* or *abgeschlossene Theorie*. For an explicit analysis of this notion, see his "Recent Changes in the Foundation of Exact Science", (1934), *Philosophic Problems, op. cit.*, pp. 11—26; "The Notion of 'Closed Theory' in Modern Science" (1948), in AF, pp. 39—46. The notion is adumbrated in *Zeit. f. Physik*, xviii (1927), p. 172.

applicability, but the extent of that domain, he holds, can never be anything but indefinite: future research then cannot falsify a Closed Theory; it can only discover restrictions on its domain. For Heisenberg then, radical theoretic change presupposes a fund of stable background knowledge — observational and theoretical — that comprises the language and concepts of classical physics (L_N) and the set of Closed Theories that constitute the existing stage of the permanent achievement of science.

Whatever about the apriori character of L_N, whatever about the value of what he calls *Closed Theories*, Heisenberg has indeed argued plausibly that all experimentation takes place only on the supposition that there is a fund of stable background knowledge, both pre-theoretical (or observational) and theoretical, that permits the construction of essentially similar pieces of apparatus and the recognition of essentially similar signals. This I call the *principle of stable background knowledge.*

E-Observability

A second principle Einstein used at least in the development of the special theory was a principle of observability. Under the influence of Ernst Mach's principle of thought economy, Einstein rejected the existence of absolute space and time because they were in some sense unobservable. In what sense observability counted and why, was never fully and critically formulated by Einstein. He came, moreover, to change his view about observability during the period between special relativity (1905) and general relativity (1915). What is relevant to our discussion, however, is what Heisenberg in 1925 thought Einstein meant by observability and how he fashioned a principle of observability derived as he believed from Einstein. This principle I shall call *the principle of E-observability*[34a].

In the *Physicist's Conception of Nature* (London, Hutchinson, 1958), he writes that a closed theory "is valid for the entire cosmos and cannot be changed or improved" (pp. 27—8). Closed theories are unrevisable because, as he says, they are "idealizations" of reality (AF, pp. 184—191.) The evolution of physics, for Heisenberg, takes place through the dialectic of "idealization" (or formalization making a "closed theory") and research into "domains of applicability" of theories that are "closed". The domain of a closed theory is always to some extent indefinite. New research discovers areas of inapplicability of the old theory; this necessitates a new "idealization" (subject to principles of growth and continuity) and so on; cf. *Physical Principles, op. cit.*, pp. 1—4. About the domain of applicability of a closed theory, he says, "When you axiomatize a theory, as for instance, Newton [did] ... then from this moment on you do not know whether this whole scheme has anything at all to do with nature because then it is closed. You may be lucky and it may actually fit to nature in a very large number of observations: all right, but you never can say how much it fits. I mean in some way, you have lost contact with nature". (AHQP, 5 July, 1963).

[34a] Though Einstein seemingly did not subscribe to this principle at the time of the discovery of special relativity, it does express what he later came to hold.

Heisenberg launched his theory of quantum mechanics with the explanation that his basic principle was to consider only relations between *observable magnitudes*[35]. The rhetoric echoed Einstein's of 1905; the message was revolutionary since it seemed to attack the entrenched rationalism of the German academic scene. The principle sounded like the rallying-cry of young inconclastic positivists, "Clean out from physics the remnants of metaphysics, religion and mysticism!" A closer inspection, however, reveals that Heisenberg's basic insight was far less revolutionary than it sounded. Heisenberg had no wish to deny the three hundred years of physics based upon the mathematization of qualities *as measured* in order to return to a pre-Galilean or Aristotelian physics based upon qualities *as sensed*. What stimulated Heisenberg's insight was the recognition that certain variables, like the intra-atomic electron orbits appearing in the old quantum theory were not *measurable*. Energy levels, intra-atomic electrons, spin and a host of other entities are not observable in a positivist sense; but what distinguishes them from the orbits of intra-atomic electron is that they are measurable, and to that extent, they are, what I call, *E-observable*.

The principle of E-observability can be formulated in the following way: A scientific descriptive language L, to yield true ontological descriptions, must be such that every descriptive predicate in it has observable consequences; that is for every predicate F(—) in L, there exists some warranted empirical fact, the description of which in L, namely, S(—, F(a), —) uses F(a) essentially. (A sentence F(a) is used *essentially* in S(—, F(a), —), if and only if, when F(a) is replaced by its negative ∼F(a), the truth of S(—, F(a), —) implies the falsity of S(—, ∼F(a), —)). Such an empirical fact, S(—, F(a), —) is equivalent to a measurement of F(a), since it provides an empirical criterion to discriminate between the two cases F(a) and ∼F(a) (a kind of yes/no test for F(a)!): it is assumed of course that the two facts S(—,F(a),—) and S(—, ∼F(a), —) have identical conditions except for F(a) or ∼F(a).

It is possible to read Heisenberg's philosophical development as the passage from an early empiricist phase to a later rationalist, even idealist, phase[36]. Within this perspective, one would point out in his earlier writings, the blurring of the distinction between the observable *signifier* of the scientific fact, e.g., the track on the photographic plate, and the scientific fact *signified* by (and made observable through) the signifier, e.g., the electrons that made the tracks. The signifier (taken as an independent object) is a fact in the everyday world: the signified object is a scientific fact for which the predicates of the everyday world are — generally at least — neither applicable nor appropriate; except in so far as

[35] As Heisenberg wrote to W. Pauli on 24 June, 1925, "Grundsatz ist bei der Berechnung von irgenwelchen Größen, wie Energie, Frequenz, usw., dürfen wir nur Beziehungen zwischen prinzipiell beobachtbaren Größen vorkommen", cited in Heisenberg, "Erinnerungen", *op. cit.*; see also QMO, pp. 30—2 and passim.

[36] As the author did in QMO.

everyday language is reductively scientific, as would be the case if it were normed, as Heisenberg thought it was, by the language of classical physics[37].

It is possible, however, to put a different construction on Heisenberg's writings and to read these as gradually expressing recognition of the fact that the revolutionary empiricist language he had borrowed from Mach, Bohr and the early Einstein, did not express the true complexity of his position, since it left out something of which he was always at least dimly aware, namely, the theory-laden character of observables. In this paper, I am adopting the latter perspective which I now believe to be preferable. In confirmation, we have Heisenberg's report (admittedly written many years later!) of a conversation with Einstein in Berlin in the spring of 1926 about the role of observables in physics. The impression Heisenberg gives us is that Einstein's strong dislike of what he heard was occasioned by the vocabulary Heisenberg used with its echoes of positivism, rather than by the substance of what was said with Einstein seemingly failed to grasp[38]. "It is the theory which tells us what we can observe", Einstein peremptorily told a bewildered Heisenberg, articulating what Heisenberg always meant but perhaps has not been able to express[39].

The principle of E-observability so expressed makes the observable character of a predicate a necessary but not, however, a sufficient condition of the descriptive status of a language. Consequently, the principle does not prove theories but refutes conjectures. Since all the predicates of a well-formulated theory constitute an implicit meaning circle, the conjecture that is refuted is probably not the existence of a single unobserved predicate, but most likely the acceptability of the theory as a whole[40]. So the unobservability of electron orbits, led Heisenberg to attempt to change the very substance of the theory by re-defining the very meaning of state variables. Through a change in the mathematical form of the theory and then — as we shall see in the next section — in the semantic re-interpretation of the theoretical symbols. The same implicit meaning circle also implies that for him there is no occurrence of a fact or observation of a predicate that does not suppose at least the tentative acceptance of the linguistic framework of the theory[41]. Hence, the kind of observation-event that warrants a predicate as observable is for him a post-theoretical fact, not a pre-theoretical fact. Despite the apparent disagreement ex-

[37] Evidence for this, for example, is found in *Physical Principles*, *op. cit.*, pp. 1, 65.

[38] PB, chap. v, "Quantum Mechanics and a Talk with Einstein (1925—6)", pp. 58—69.

[39] PB, p. 63. Einstein had by that time come to reject vehemently the spirit of positivism in science, cf. "Autobiographical Notes", op. cit., p. 49.

[40] In practice then, Heisenberg's Principle of Observability functioned negatively, much like falsification in Karl Popper's philosophy of science; cf. K. Popper, *The Logic of Scientific Discovery* (London, Hutchinson, 1959), *Conjectures and Refutations* (London, Routledge and Kegan Paul, 1963), *Objective Knowledge* (New York, Oxford Univ. Press 1972).

[41] This point has been made by Popper, S. Toulmin, R. N. Hanson, P. Feyerabend and others.

pressed by Einstein in his conversation with Heisenberg already mentioned, neither of them supported the view characteristic of empiricism that a scientific theory represents either a mere generalization of pre-theoretical experience or a second- (or third-) level systematization removed from ontological description[42].

Contextual Re-interpretation of the Formalism

About the failure of every attempt to observe absolute motion, Heisenberg wrote, "Here Einstein took a hand, and with one stroke of wizardry resolved all the difficulties. He assumed that the bodies really do contract in the direction of motion, and that the apparent time of Lorentz's formulae is actually the true time and that these formulae therefore convey a new understanding of space and time themselves. He thereby created the basis of the theory of relativity"[43]. The Einsteinian revolution that followed was a revolution in descriptive ontology introduced by a transformation of linguistic usage[44]. The transformation of linguistic usage was the result of a semantical re-interpretation of the kinematical variables making them contextually dependent on a unique class of observers (instrumental reference frames), those that constitute the manifold of inhomogeneous Lorentz group. This transformation was effected by what I call the *principle of contextual re-interpretation of the formalism*. For Einstein, contextualization was *merely notional*; it meant no more than a conceptual relativity to members of a class of observer-frames. For Heisenberg, however, contextualization was *real*! it made the measurement process itself the basis for a real relativity between the quantum mechanical observable and the class of its observer-frames[45].

[42] Heisenberg's later explicit rejection of positivism, empiricism and pragmatism is contained in PB, "Positivism, Metaphysics and Religion (1952)," pp. 205—17 and "Atomic Physics and Pragmatism (1929)", pp. 93—102.

[43] AF, p. 2; cf. also *ibid.*, p. 161, "If this statement [that the Lorentz transformation tells the structure of space and time] is correct, the words 'space' and 'time' mean something other than they do in Newtonian physics".

[44] AHQP, 5 July 1963: Heisenberg narrated how late in 1926 as he ran in Faelled Park near Bohr's Institute in Copenhagen, the insight came to him, "Why not simply say that only those things occur in nature which fit our mathematical scheme!" This thought, clearly inspired by Einstein, led him to consider the gamma-ray microscope, etc. The problem of expressing in language what occurred in nature as revealed by quantum mechanics was a real one: on the one hand, Bohr held that language was a given, consecrated by historical process, convention, everyday usage and the "customary forms of perception"; on the other hand, Heisenberg believed that the experience of relativity showed the flexibility of language to contextual re-interpretation forced by changes in the mathematical formalism. Heisenberg eventually came to accept the view (Bohr's) that it was not necessary to re-interpret the language of physics (classical language), but that the old language could remain provided limitations were imposed on its use in quantum mechanics, principally a certain vagueness of meaning; cf. note 68.

[45] As I have shown in QMO, chap. ii, Heisenberg's original intention was to re-interpret the kinematical variables within the context of the measuring process. The title of his revolutionary paper proclaims this intention, "Über quantentheoretische Umdeutung

Pragmatic Continuity

Two other principles of rational development are involved in what is called *Korrespondenzdenken,* that is, in the belief that there is a smooth passage between classical physics, and relativity and quantum mechanics. It is supposed that if L_R is a rational development or extension of L_N, then the domain of applicability (Radnitzky's *territory*[46]) D_R of L_R contains the domain of applicability D_N of L_N; that is, $D_R > D_N$ (D_R is greater in extension than D_N).

What does *applicability* mean? When is a theory *applicable*? What can be derived about the relative power of two theories when $D_R > D_N$, and what can be said about the relation between the theoretical concepts of L_R and L_N if both theories are applicable in a common domain?

A scientific theory is *applicable* over a domain D, if and only if it fulfills the systematic and predictive goals of science for that domain. The systematic goals of science provide objective understanding of the domain: the predictive goals permit control and manipulation of the events and processes of the domain[47]. But are the two goals different? Is objective understanding different from the power to manipulate predict and control? There are some who say that for the sciences, understanding is none other than the knowhow to predict and control. No so for Einstein and Heisenberg: for them, scientific understanding is oriented towards ontology, the way things are and act; and this is intimately involved with mathematical representations of nature — the scientific image. Prediction and control may or may not follow depending on the subject matter: for satellites and space stations, yes! for general cosmology, certainly, no!

If two theories such as L_N and L_R are applicable to a common domain such as D_N, both provide understanding and to the extent possible,

kinematischer und mechanischer Beziehungen", ("On quantum theoretical re-interpretation of kinematic and mechanical relations"), *Zeit. f. Physik,* 33 (1925), pp. 879—893. Bohr was more cautious, as we have seen (note 44). Heisenberg came to accept complementarity in March 1927, as he told Kuhn (AHQP, 25 February 1963). Heisenberg's explicit adoption of Bohr's philosophy is announced in the Preface to his *Physical Principles, op. cit.,* but internal evidence in the text shows a considerable difference of viewpoint; cf. QMO, chaps. ii and iii. See below, especially note 68, for further comments on the differences between Bohr and Heisenberg.

[46] Gerard Radnitzky, "Philosophie de la recherche scientifique" *Archives de Philosopie,* 37 (1974), pp. 5—76. "Towards a 'Praxiological' Theory of Research", "*Systematics,* x (1972), pp. 129—185; "Philosophy of Science in a New Key", *Methodology and Science,* vi (1973), pp. 134—178; *Preconceptions in Research* (London, Human Context Books, 1974).

[47] In PB, Heisenberg speaks of the goals of science: "'Understanding' in Modern Physics (1920—22)", pp. 27—42, "Atomic Physics and Pragmatism (1929)", pp. 93—102, "Positivism, Metaphysics and Religion (1952)", pp. 205—17. Predictive ability, he says, is not enough, because even Ptolemy could achieve this (pp. 31, 212). Exact science moves towards more and more comprehensive theoretical syntheses, expressed by simple mathematical formulae (p. 99). The beauty and simplicity of these formulae witness to their truth as expressing the real course of nature (p. 212; also AF, p. 172).

prediction and control over the events and processes of D_N. Leaving out of account the question of what constitutes understanding, or whether or under what conditions L_N and L_R could provide a common understandingover D_N, we can speak about the predictive power of the two theories in their common domain. If L_R is to be the rational extension of L_N, then at least it must be true that to every problem of prediction and control in the domain D_N that L_N solves correctly (to a specified degree of accuracy), L_R must also provide a correct solution (to the same specified degree of accuracy). That condition, I call, *the principle of pragmatic continuity*: It represents one aspect of *Korrespondenzdenken*, and it purports to be a principle of rational growth of knowledge[48].

Model Continuity

If in addition to pragmatic continuity, we add the condition that L_R is a rational extension of L_N, then something more is said — that the understanding (as well as predictive control) given by L_R over D_N is the same as that given by L_N over D_N. Since scientific understanding for Einstein and Heisenberg is essentially of the implicit correlations between scientific terms given by the mathematical equations of a theory[49], if the understandings conveyed by L_R and L_N are to be the same, they must have formally isomorphic mathematical models over the domain D_N (to the degree of accuracy that is significant)[50]. Model continuity is, for Einstein and Heisenberg, a guarantee of *meaning-invariance* over theory change. By the mathematical model of a theory, I mean the set of symbols, syntactical rules of formation, postulates and equations that,

[48] See for example, Heisenberg, *Physical Principles*, *op. cit.* pp. 66, 105, 107; *Philosophic Problems*, op. cit., p. 24, where pragmatic continuity is implied. In noting the variety of formalisms developed for the quantum theory — by Schrödinger, Dirac and himself — and the variety of interpretations of the formalisms — by Bohr, Schrödinger, Born and himself — he consoles himself with the thought that, after all, they all give pragmatically the same experimental results; cf., PB, p. 77. In AF, he writes, that the success and fruitfulness of a new theory is reason why scientists come to accept it; this he calls a "pragmatic criterion of value" (p. 163).

[49] AHQP, 27 February 1963: Heisenberg said, "When you have a number of axioms as Newton had in ... *Principia Mathematica*, then the words are not only defined by the customary use of the language, but they are *defined by their connections* ... That is, you cannot change one word without ruining the whole thing". (italics inserted).

[50] AF, pp. 187, 189. Enough has been said to prove that for Einstein and Heisenberg the mathematical formulae contained the relationships essential to a scientific understanding of phenomena. Both demanded model continuity as one passed beyond the domain of applicability of a Closed Theory to the more extended domain of the new theory. It does not follow, of course, that model continuity was in fact achieved; in the case of quantum mechanics, it was not achieved as Bohr and Heisenberg knew well. It is not always the case that $h \to 0$ and/or masses or quantum numbers become very large, that the classical formula is obtained. Cf. QMO, pp. 114—5. The same point is made by P. Feyerabend in "Problems of Empiricism II" p. 296—300 in *Nature and Function of Scientific Theories*, ed by R. G. Colodny (Univ. of Pittsburgh Press, 1970).

when given a particular semantical interpretation, becomes a scientific language over the domain in question.

Applying this condition of model continuity to L_R and L_N, it follows with Einstein that L_R is a rational development and extension of L_N only if, in the limiting case of the non-relativistic domain (using an appropriate limiting process, e.g., $v/c \to 0$), L_R and L_N present a formal isomorphism of mathematical theoretical structure; that is, when one passes to the limiting case, both kinematical and dynamical variables in L_R must have the same theoretical relationships to one another that they have to L_N[51]. This is the syntactic aspect of *Korrespondenzdenken*. I shall call this the *principle of model continuity*. It too purports to be a principle of rational growth of knowledge[52].

Simplicity and Beauty

The list of principles of rational theory change would be incomplete without mention of the properties of simplicity and beauty. It is a common view of scientists shared by both Einstein[53] and Heisenberg[54], that all theories are simple and beautiful that truly mirror Nature. But what is *simplicity*? How do we judge the *beauty* of a scientific theory? To formulate criteria for these is as difficult as it is to say why one art object hits it off and another does not. Einstein intended one thing by these terms; Heisenberg apparently intended something else, and Schrodinger something different again. All we can say with certainty is that each claimed to be able to recognize simplicity and beauty and that nevertheless they disagreed profoundly in their concrete judgements about particular theories; for example, about Heisenberg's Matrix Mechanics. However, the fact of disagreement does not imply that criteria do not exist, only that there are different sensibilities, different "esthetic" styles in science as in art. Criteria of this kind belong to the transcendental (or non-objective) conditions of possibility of theoretical scientific rationality. Whether or not these conditions are fixed is a matter for dispute. Nevertheless, theories, old and new, are subject to a *principle of simplicity* (of objective form) on account of which they are seen to possess the kind of beauty that is the splendor of scientific truth and a manifest of its presence.

[51] For example, Heisenberg, *Physical Principles*, *op. cit.*, pp. 37—8, 83, 89, 101, 116 where continuity of formalism is used as a criterion or is affirmed.

[52] Model Continuity and Contextual Re-interpretation result in the kind of problem about meaning-invariance in theory-change that Feyerabend articulates in his "Problems of Empiricism I", *op. cit.* The author has given his analysis of the conditions of continuity in development and change in his "Hermeneutics of Experimental Science in the Context of the Life-World", *op. cit.*, and "Logic of Framework Transpositions", *Internat. Philos. Qrtly*, xi (1971) p. 314—34.

[53] Einstein, "Autobiographical Notes", *op. cit.*, pp. 23—63.

[54] PB, p. 68, AF, pp. 172, 174—5.

Heisenberg had then before him in 1925; explicitly or at least implicitly, seven principles which he believed radical theoretic change should obey[55]: the principles of (i) scientific realism; (ii) stability of background knowledge; (iii) E-observability; (iv) contextual re-interpretation; (v) pragmatic continuity; (vi) model continuity; (vii) simplicity.

In his latest book written about fifty years after the great achievement of quantum mechanics[56], Heisenberg addressed himself in a lecture entitled "The End of Physics?" to the question of revolutionary change in physics. "How does one make a [scientific] revolution?" he asks. "By trying to change as little as possible", he replies[57]. Revolutions have occurred, he admits, and these have altered the edifice of physics down to its foundations, but "never in its history has there been a desire for any radical reconstruction of the edifice of physics", he asserts, "it is precisely the wish to change as little as possible which demonstrates that the introduction of novelty is a matter of being compelled ... by nature itself and not by human authorities of any kind". To demand a radical revision of physics would "run the risk of wanting uncritically to change things even where change is made impossible for all time by the laws of nature".

What Heisenberg is saying in summary adds to the original set of principles the following: (viii) a scientific revolution, though affecting a particular social group — the scientific community — has never been the express goal of that group, nor *ought* it ever to become an express goal (*principle of non-proliferation of competing theories*), instead, (ix) scientists *ought* to direct their efforts towards resolving residual problems while conserving existing theories as far as possible (*principle of tenacity*)[58]; for only in this way will the mistake be avoided of trying to make changes in theories that are unrevisable.

Within the context of the two extremes of *theory-monism* and *theory-pluralism*, Heisenberg is today certainly not to be counted among the latter[59]. Neither is he, however a monist-reductionist. He believes

[55] The extent and consequences of the use by Heisenberg of these research-guiding principles in the construction of quantum mechanics, will be considered in another paper.

[56] AF, *op. cit.*

[57] AF, chap. 12, "Changes of Thought Patterns in the Progress of Science", pp. 154—65. The citations are on pp. 163—5. This paper was given during the student unrest of the late '60's and perhaps its rhetorical form was influenced by Heisenberg's rejection of what he took to be the use of political means to transform the disciplines. He does admit, however, that there are social components to a scientific revolution, for, as he says, echoing Wolfflin, not everything is possible in every epoch (p. 158).

[58] Nevertheless, Heisenberg said that he is in agreement with Kuhn's theory of scientific revolutions. He notes that our inquiries are endless, our theories are subject to being overturned, and that mystical and religious criteria may sometimes be operative in scientific change; for the last-mentioned, he cites the views of Pauli and Kepler. "There is no solid bottom ... one can never hope that these fundamentals (reached at any time) will rest forever", he asserts (AHQP, 17 Feb., 1963).

[59] *Theory-monism* is a research-guiding principle that advocates staying within the paradigm of tested contemporary research: *theory-pluralism*, on the other hand, advocates

physics is cumulative and that revolutions are essentially conservative ending in the unification by transformation of the established domains of physics subject to the principles of continuity contained in the list enuntiated above[60].

Age tends to make people more conservative and gradualist in their views, and Heisenberg is no exception. It is therefore instructive to learn that his present attitude towards continuity and change does not negate any of the former principles which would still apply, we would suppose, in the future to any conceivable radical theoretic change. He would now however, explicitly enjoin physicists against seeking deliberately to bring about radical change (viii and ix). Nevertheless, the original seven principles contain important conservative and gradualist elements: for example, the use of an accredited model even in change, and of certain continuity principles, viz., (ii), (v) and (vi). Principle (i) is a statement of scientific realism from which he has never moved — despite his acceptance of complementarity. The other three principles contain revolutionary potential.

These last three potentially revolutionary principles can be expressed as research-guiding injunctions: (a) Test for E-observational adequacy (principle iii)! (b) Preserve continuity of mathematical formalism but be open to the need for semantical re-interpretation of the formalism (Principle iv)! (c) Look for theories that unify and simplify the existing multiplicity of theories (Principle vii)!

All three of these injunctions have a certain mutual independence: the thrust for observational adequacy, the search for a better, perhaps revolutionary semantical interpretation of the formalism and, the desire for theoretical unity and simplicity in science touch on different and independent kinds of research endeavor. The testing for observational adequacy enjoined by (a) used experimental methods based on the accepted paradigm for the theory. The search for better semantical interpretation of a formalism enjoined by (b) uses a kind of critical eidetic reflection on the functioning of the paradigm in order to correct the interpretation of the model or to open it up to new possibilities linked to the same mathematical model by giving the terms and equations of the model new semantical meanings[61]. Finally the search for theoretical

the construction of a variety of different paradigms which are then tested against one another. The principle advocates of theory-pluralism today are Popper and Feyerabend. For an excellent review of the problems and its literature, see Gerard Radnitzky, *Preconceptions in Research, op. cit.*

[60] AF, p. 189. The theory of elementary particles that will unify the existing domains of physics will be a Closed Theory, but it will not close physics; this must grow in the direction of biology and other disciplines, where new concepts, such as *life*, appear that do not appear in physics.

[61] The method used can be compared with Husserlian eidetic intuition into the sense of a given (in this case of the givenness of scientific observables): one aims, by a type of eidetic phenomenological reduction, at the intuitive essence of what the theory says (should or could say) about the World; of the author's "Hermeneutics of Experimental Science", *op.*

unity and simplicity enjoined by (c) is a work principally for creative mathematical inventiveness and involves methods different again both from experimental methods and from critical eidetic reflection.

For the early Heisenberg, E-observational adequacy was the primary research-directive that led him to quantum mechanics. Despite the principles of continuity more talked about than evident in quantum mechanics, the first impression his contemporaries got of his version of quantum mechanics was of a theory that contradicted the central principles of continuity and causality that constituted the intelligible core of all science to that date. Einstein, Planck and others were quite unable to accept the new bewildering perspective that was envisioned. Schrodinger was revolted by it[62]. Such changes as quantum mechanics proposed affected not merely the content of physics but the very notion of science itself. Heisenberg may well now defend in retrospect the claim that after all "he changed as little as possible", but it makes about as much sense of the quantum mechanical revolution to say that its instigator tried to change as little as possible, as for a surgeon to console a patient whose leg he had amputated, by saying that he made as few incisions as possible in the circumstances.

The revolutionary theoretical form of quantum mechanics made it necessary to find an adequate acceptable semantical interpretation of the new formalism. Since the new formalism was to be continuous with the old (principle of model continuity) the semantical interpretation of the new formalism would possibly effect retroactively a re-interpretation of the old. Heisenberg believed that quantum mechanics did in fact revolutionize the sense of the kinematical variables by contextualizing them with respect to processes of measurement[63]. To these new (or newly interpreted) variables, following the principle of scientific realism, he attributed descriptive force. Bohr, however, differed from Heisenberg[64],

cit. For eidetic phenomenological reduction, see Edmund Husserl, *Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie*, I, II and III. Husserliana, vols. I, III, IV and V (1952) (The Hague, Nijhoff). Vol. I trans. by W. R. Boyce Gibson as *Ideas* (London, Allen and Unwin, 1931).

[62] Heisenberg's interesting account of the debate between himself and Bohr on one side and Schrödinger on the other is found in PB, pp. 70—76. Schrödinger said of quantum mechanics that it was, "von abschreckender ja abstoßender Unanschaulichkeit und Abstraktheit" (quoted by Heisenberg in *Zeit. f. Physik*, xliii, 1927, p. 195, note).

[63] He did not follow out the logic of his principle, however, to determine how such a re-interpretation would affect the sense of classical physics; and subsequent events, particularly his acceptance of complementarity, aborted such an inquiry.

[64] About Heisenberg's disagreements with Bohr, see PB, pp. 76—81, and AHQP, 11, 13, 15, 19, 25 and 27 February and 5 July 1963. About Bohr, Heisenberg said, "I have really in this whole period (1925—27) been in real disagreement with Bohr and the most serious disagreement was at the time of the Uncertainty Relations", (15 February 1963). Bohr wanted to start with "intuitions of how nature was and worked", he asserted, "Bohr was not a mathematically-minded man . . . he was Faraday but not Maxwell". Bohr insisted on the experimental inadequacies of matrix mechanics; Heisenberg was less worried about these, trusting in the consistency of the mathematical formalism (25 February 1963). Bohr wanted to use both wave and particle pictures jointly to give intuitive sense of how nature is

opposing both the realist sense and the semantical significance given by him to the kinematical variables. Instead he chose what he called the "complementarity" of particle and wave "pictures", these were not in his view true models of atomic phenomena, but only complementary "pictures" of how these phenomena appeared when interacting with macroscopic experimental contexts[65], and guided our use of classical language about quantum phenomena. The semantical sense of the classical variables was preserved at the expense of blurring the distinction between *signifier* and *signified*. Heisenberg's capitulation to the philosophy (or semantics) of complementarity in March, 1927[66] quelled dispute between the two, but it did not solve the underlying problem[67]. We are left to

and works; Heisenberg wanted to use the mathematical formalism as guide to what nature is really like (27 February 1963, cf. also note 68).

[65] About Bohr's philosophy, see Aage Petersen's *Quantum Physics and the Philosophical Tradition* (M. I. T. Press, Camb. Mass., 1968): QMO, pp. 44—56 and the author's "Complementarity, Context-dependence and Quantum Logic", *Foundations of Physics* 1 (1970), particularly, pp. 108—9. Bohr's philosophy has been described both by Heisenberg and Petersen as a preoccupation with the possibilities of unambiguous communication through language. Bohr saw quantum mechanics as revealing certain limitations on the possibilities of human discourse arising out of (i) the inseparability of objective content and the observing subject and (ii) the fact that the partition between the actor and the audience can be moved at will so that what was part of the audience becomes in a new context part of what is being observed on the stage. The reason, he says is the "coupling between the phenomena and the agency by which it is observed". This condition "forces us to adopt a new method of description designated as *complementary* in the sense that any given application of classical concepts precludes the simultaneous use of the classical concepts which in a different connection are equally necessary for the elucidation of the phenomena", *Atomic Theory and the Description of Nature* (Cambridge Univ. Press, London, 1934), pp. 10—11. Bohr held that all communicable knowledge about the world is necessarily expressed in terms of the "customary forms of perception" of which the categories of classical physics are a clear and precise expression; cf. *ibid.*, pp. 1, 5, 15—9, 22, 90—3, 103, 111; and "Discussions with Einstein on Epistemological Problems in Atomic Theory", in *Albert Einstein: Philosopher-Scientist, op. cit.*, p. 209. As was pointed out above (note 28), Heisenberg uses the distinction between *language*, *picture*, and *mathematical scheme* (or *meaning*) in describing Bohr's philosophy, "Bohr was from his youth interested in our ways of expression, the limitations of word, the problem of talking about things where one knows that the words do not really get hold of the things... Bohr tried to keep the picture while at the same time omitting classical mechanics. He tried to keep the words and the pictures without keeping the meaning of the words and pictures", So what do you do? he asks. He rejects Sommerfeld's "escape into mathematics" and endorses Bohr's perception that there was a philosophical problem to solve (AHQP, 11 February 1963).

[66] In the preface to the *Physical Principles of the Quantum Theory*, op. cit., Heisenberg identifies himself with the *Kopenhagener Geist der Quantentheorie*. Later (in 1955), he wrote "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", p. 16 in "The Development of the Interpretation of the Quantum Theory", in *Niels Bohr and the Development of Physics, op. cit.*

[67] To judge by the unending stream of argument and counterargument about the meaning and significance of complementarity during the past forty years, we are forced to conclude that complementary was no solution, or at least, not a clear and convincing solution to the many physicists, philosophers and logicians that have since tried to master quantum mechanical theory.

conclude that the acceptance of complementarity implied Heisenberg's temporary abandonment of the principle of contextual reinterpretation dominant in his early view[68]. The evidence shows that he did not reject the principle, but once he subscribed to the philosophy (or semantics) of complementarity it was difficult to re-instate its claims systematically in quantum mechanics. Since 1925, we have seldom seen the explicit use of critical eidetic methods in physics — attempts by critical eidetic reflection to correct the existing use or interpretation of the mathematical formalism of a theory; physics may be the poorer for lack of such a tool. However, though little in evidence, it is still not forgotten among those who share a scientific realism, especially if this has a strong conceptualist component as it has in Heisenberg and Einstein[69].

The main thrust of Heisenberg's research today is guided by the principle of simplicity: it is directed towards the unification and simplification of the existing theoretical divisions of physics in one universal matter equation, that would exhibit all the symmetries of nature and from which would be derived as limiting cases all the existing "closed theories" of physics[70]. Principles (viii) and (ix) follow from his belief in the cumulative character of physics and in the essential unrevisability of

[68] AHQP, Heisenberg said that at the time he wrote the paper on the Uncertainty Relations, he did not realize that the words "position", etc. could still be used in the old sense, but with limitations: Bohr made him realize this a few months later (27 February 1963). In fact, referring to his original intention, he said that he learnt from Bohr that "the thing I in some way attempted could not be done ... one has to talk about, e.g., the diffraction pattern (as a wave phenomenon) while holding the indivisible character of the electron (as a particle phenomenon)... [for this] one needs a language... taken from the historical process [which in our case] is a classical language [L_N]... thus one cannot avoid the tension between classical precise language and its limits (17 February 1963). Nevertheless, the experience of relativity showed that his original intention was viable. Comparing the quantum mechanical and the relativistic revolutions: in relativity, he said, actual language has adjusted to the mathematical scheme... In quantum theory, language has never adjusted to it... The mathematicians have shown that it could adjust to it by changing the Aristotelian logic... So far nobody has been willing to pay that price. Now that was not clear at that time. But still it was clear that probably the only sensible thing to do was to use the old words and always remember their limitations" (27 February 1963). Various non-Aristotelian logics have been proposed, starting with the paper of G. Birkhoff and J. von Neumann, "The Logic of Quantum Mechanics", *Ann. of Math.*, 37 (1961), pp. 155—184; of the author's "Classical Logic and Quantum Logic: Their Respective Roles", *Synthese*, 21 (1970), pp. 2—33.

[69] The relative invisibility of eidetic methods in modern physics arises from the fact that there seems to be no systematic place for it or for the kind of evidence it produces in the "received" views, both empiricist and rationalist, of scientific inquiry. Some results in elementary particle physics, as, e.g., those obtained by Kosta Gavroglu in "Semiweak interactions and the non-leptonic weak decays", *Nuovo Cimento 16A* (1973), p. 61, were produced by the use of eidetic methods, according to a verbal report given to the author by Gavroglu. Gavroglu is now engaged in a study of these methods in physics, particularly as applied to elementary particle and quantum gravitational theory. Gavroglu is at SUNY at Stony Brook.

[70] For Heisenberg's work on unified field theory, see for example, his „Entwicklung der einheitlichen Feldtheorie der Elementarteilchen", *Naturwissen.*, 50 (1963), pp. 3—7, and his *Unified Theory of Elementary Particles* (London, Interscience, 1966).

closed theories — closed-off, that is from internal development and complete in their axiomatic form. Heisenberg's conservatism today envisages only what we might call, echoing his own metaphor, the "central edifice" of physics to which, he believes, quantum theory belongs. Future developments in physics, he asserts, will lie in building connections to other disciplines, for example, to chemistry and biology[71]. But the central edifice is nearly complete and to touch it is to risk "wanting uncritically to change things even where change is made impossible for all time by the laws of nature"[72].

I shall take up the critique of the research-guiding principles formulated above in another study.

[71] AF, p. 164.
[72] *Ibid.*

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