Complementarity and Scientific Rationality

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Abstract: Bohr's interpretation of quantum mechanics has been criticized as incoherent and opportunistic, and based on doubtful philosophical premises. If so Bohr's influence, in the pre-war period of 1927-1939, is the harder to explain, and the acceptance of his approach to quantum mechanics over de Broglie's had no reasonable foundation. But Bohr's interpretation changed little from the time of its first appearance, and stood independent of any philosophical presuppositions. The principle of complementarity is itself best read as a conjecture of unusually wide scope, on the nature and future course of explanations in the sciences (and not only the physical sciences). If it must be judged a failure today, it is not because of any internal inconsistency.

Despite the expenditure of much effort, I have been unable to obtain a clear understanding of Bohr's principle of complementarity (Einstein).

Bohr's point - and the central point of quantum mechanics - is that no elementary phenomenon is a phenomenon until it is a registered (observed) phenomenon (Wheeler).

1 A QUESTION POSED

It was only in the last quarter of the twentieth century that alternatives to quantum mechanics finally came of age: only then was it agreed, by majority (if not quite unanimous) consent, that de Broglie's pilot-wave theory was indeed empirically equivalent to the non-relativistic theory; and by the late 1980s that state reduction too might be employed at ground level in a precise and universal formalism, empirically equivalent to the standard one.

Recognition of these facts was hard-won, and, in the case of the state-reduction theory, built on hard labour. But it was otherwise with pilot-wave theory. The essential ideas for this were laid down by de Broglie in the 1920s. When Bohm independently discovered the formalism in 1952 he added only new applications (in particular to the measurement process itself). Why was it so neglected?

This question becomes more troubling when it is recognized that the view that did prevail in the inter-war years, Bohr's interpretation of quantum theory, was flatly in contradiction to the pilot-wave approach. Embarrassment turns to scandal when it is added that Bohr's view was wedded to idealism: to the view that there *is* no quantum reality, that it is *the observer* who brings about the result of the measurement. It must then be explained how the physics community, to an extraordinary degree of uniformity, could have bought into such an extreme and unnecessary philosophical position, in support of a theory to which the pilot-wave theory was a clear counter-example.

The view that Bohr's philosophy *was* so committed - if not to a Berkelerian idealism, then to anti-realism or to neo-Kantianism - is now very prevalent (see e.g. [23][27]). There is plenty of evidence that Bohr was drawn to neo-Kantianism; there is no doubt that he saw complementarity as a theory of importance to philosophy as well as physics. Given which, the view that Bohr's success can only be explained, if explained at all, by *sociological* considerations, of fortuitous coincidences of philosophical movements, of institutional and collaborative influences, becomes extremely persuasive: Bohr did stand at the centre of most of the major discoveries leading to the matrix mechanics; he was revered by the younger generation of physicists, including Kramers, Pauli, and Heisenberg (and even Dirac). Cushing was the first to press the question I am concerned with and was led to just this conclusion [16]; others have since followed him [1].

Or something has gone wrong with our standard of rationality. One can confront the challenge head-on. One can simply deny that it was reasonable to buy into pilot-wave theory, at least in the inter-war years. After all it faces serious difficulties when it comes to relativity,[1] and not all is at is seems when it comes to its account of the observable phenomena (I shall come back to this in due course). Just as important, almost no-one at the time (not even de Broglie) was prepared to believe that the pilot-wave - a complex-valued function on configuration space, so a space of enormously high dimension - could be taken as physically real. And it gets much worse when one takes on board the implications of extending this idea to the macroscopic level, as one must, if pilot-wave theory is to get rid of state-reduction. De Broglie was never prepared to take this step. Although he was perfectly clear that one could drop non-overlapping "empty" waves from the guidance equation - so providing for an "effective" state-reduction - he could not countenance the same treatment when it came to the measurement apparatus and the macroscopic world (not even *after* reading Bohm's papers [17][18, p.178]). The pilot-wave in that case is nothing less than a wave-function to the universe. It was not Bohm in 1952 who was the first to explore the meaning of that concept, but Everett, in 1957. Who says it was rational to buy into this picture of reality?[2]

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[1] I have argued elsewhere that the only feasible option here is the Dirac hole theory (although whether this is really credible, from a realist point of view, is another matter). Working with field-configurations instead, as recommended by Valentini [35], is of no use in making contact with phenomenology, and worse, if these are the beables, provides no guarantee that objects will be localized at the macroscopic level, where they need to be [32].

[2] The argument that pilot-wave theory incorporates the same ontology as Everett's has been made by Brown and Wallace [15]. I would only add that if it is to be resisted, it is at the price of denying the intelligibility of the state-reduction approach altogether (so this too faces a "problem of rationality", according to the pilot-wave theory).

Hindsight does not always yield clear vision. Nevertheless, believable or not, pilot-wave theory is clearly a causal, spacetime theory, of the sort that Bohr denied was possible. There is undoubtedly a severe tension, if not outright contradiction, between Bohr's account of quantum mechanics and de Broglie's. And embarrassment at the failure to expose it still turns to scandal if, as is alleged, Bohr's philosophy was itself incoherent or philosophically extreme. It is this more limited question that I am concerned with in the sequel.

2 HOW TO DEFEND COMPLEMENTARITY

There have been plenty of attempts in recent years to make sense of Bohr's writings as a coherent and systematic interpretation of quantum mechanics, but no account of this sort that I know of is directed to the choices that were made by the wider community of physicists in the '20s and '30s.[3] That limits the options. From that perspective, if it is to be defended at all, Bohr's principle of complementarity, that stands at the heart of his interpretation, has to make sense as a principle that was plausible and accessible to his readers at the time. It will not do to read it as based on esoteric philosophical principles; it will not do to justify it by mathematical or physical arguments that were then unfamiliar. The answer had better lie more on the surface of his writings.

What is wrong with complementarity as based on philosophical principles? The answer is not only that it would then have hardly had much appeal among physicists; it is that it would have had too broad a scope, and pilot-wave theory refutes it. As we shall see, the principle needs to be read as anchored in the phenomena if it is to stand any hope of escaping this objection. Only on one point was it reasonable to commit to any *a priori* (or dogmatic) principle, in the absence (at the time) of any worked out view to the contrary, and that was the irreplaceability of classical concepts (agreed on by Bohr, Einstein and de Broglie). The old quantum theory was of course also based on classical concepts; Bohr was as much concerned to make sense of the old quantum theory as the new.

Broadly philosophical presuppositions can be allowed, so long as they were genuinely community-wide. Anti-realism and idealism are not in this category, but a mildly operationalist outlook is.[4] Bohr was concerned with experiments and with the operational definitions of concepts, as were most of his contemporaries. A broad and shallow form of operationalism is perfectly compatible with realism ("realism with a cautious face").

Finally, since our concern is with Bohr's influence on the wider scientific community in the inter-war period, it is only his views and writings at this time that matter. Three stand out as especially important. The first is his

[3]Howard's [26] is a possible exception, but as he himself says, his account is a reconstruction rather than an exegesis of Bohr's interpretation.

[4]The notion of "the observer" as a central concept of special relativity was also common ground to Bohr and his readers. Bohr often drew attention to it in his discusions of quantum mechanics. But the parallel was not essential to Bohr's arguments (it was the notion of a frame of rerence that played the greater role), and I shall not consider it here.

address to the International Congress of Physics in Como, Italy, in 1927 (the *Como lecture*), published in *Nature* the following year as "The quantum postulate and the recent development of atomic theory" (and, in German, in *Die Naturwisssenschaften*). It was the centre piece of his collection *Atomtheorie und Naturbeschreibung* published in 1931, translated as *Atomic Theory and Human Knowledge* in 1934 [2]. In the Como lecture Bohr first set out his theory of complementarity, following two years of almost complete public silence (the two years that covered the explosive discovery of the new mechanics). The second is the preface to this collection, the "Introductory Survey" [2, pp.1-24], first published in 1929 (in Danish) as an accompaniment to the Danish translations of these articles. In the German and English translations this was read almost as widely as the Como lecture itself. And the third is Bohr's response to Einstein, Podolski, and Rosen (the EPR argument), published in 1935 [5]; that followed several years of debate with Einstein over foundations and in effect marked their conclusion. Any account of Bohr's interpretation of quantum mechanics that is not clearly embodied in these three texts is worthless for our purposes.

Better still is an account consistent with these and with Bohr's own evaluation of his thinking in this period. That we have in his contribution to the Schilpp volume in the *Library of Living Philosophers* devoted to Einstein, published in 1949 [8]; there Bohr reappraised both the Como lecture and his discussions with Einstein. In this spirit one other article is worth special mention, "Quantum physics and philosophy - causality and complementarity" published in 1958 [9]. It was Bohr's last presentation of complementarity. According to his son and executor Aage Bohr, he felt that there he expressed some of its key concepts more clearly and concisely than he had elsewhere. Bohr died in 1962.

My claim is that in these writings Bohr is most clearly and consistently read as a realist, albeit of an operational persuasion; that his goal was to present a framework in which quantum phenomena were to be analyzed in classical terms; and that he argued for this framework in terms of principles, and specifically the principle of complementarity, as *scientific* principles, broadly empirical in scope, rather than philosophical ones, that stood independent of idealism or neo-Kantianism (or any other school in philosophy). And in these respects I maintain he was largely successful.

None of this is to say that it was reasonable, *circa* the late 1920s and '30s, to embrace Bohr's views and to reject de Broglie's and Einstein's; but it is a step in the right direction. I shall have a further comment to make on this at the end.

3 THE COMO LECTURE

In 1927 Bohr's point of departure was the *quantum postulate*. A principle of that name had long been familiar to the old quantum theory, as defined by the Bohr-Sommerfeld quantization rules (the principle that any change in action, with the units of angular momentum, must be an integral multiple of Planck's constant h). Of quantum mechanics, he began:

> Its essence may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's constant of action. [2, p.53]

Bohr immediately went on to say that the quantum postulate "implies a renunciation as regards the causal spacetime coordination of atomic processes".

Both claims were by then contentious, given that Schrödinger had deduced the quantization rule as a consequence of boundary conditions for the solutions of a continuous wave equation only the previous year. It may be that Bohr was convinced, with the experience of the failure of the Bohr, Kramers and Slater theory just behind him, that if energy and momentum were conserved in individual processes then quantum jumps were just as unavoidable in wave mechanics as in matrix mechanics (Schrödinger's wave function and Slater's virtual radiation field were closely allied). Later on in the Como lecture Bohr spoke of wave mechanics as "a symbolic transcription" that is "only to be interpreted by an explicit use of the quantum postulate". On two occasions he spoke of the postulate as "irrational" (as in "...we meet...the inevitability of the feature of irrationality characterizing the quantum postulate"). This makes the abandonment of causal spacetime descriptions look like an assumption from the very beginning.

But the argument that followed was more circumspect. There was a special sense in which the causal, spacetime idea of explanation was to be weakened. Here is the argument on its first appearance:

> Now, the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation.

Bohr argues from the quantum postulate, understood as implying an ineliminable interaction on observation, to a "no-separation" principle - that the object of observation is inseparably bound up with the experimental context. Similar claims may be found in all his subsequent writings on quantum mechanics.

Bohr continued:

> After all, the concept of observation is in so far arbitrary as it depends upon which objects are included in the subsystem to be observed. Ultimately, every observation can, of course, be reduced to our sense perceptions. The circumstance, however, that in interpreting observations use has always to be made of theoretical notions entails that for every particular case it is a question of convenience at which point the concept of observation involving the quantum postulate with its inherent "irrationality" is brought in.

Bohr here and subsequently is certainly preoccupied with "observation", with experiments - on what can be said of the microscopic realm on the basis of experiments. To this extent his philosophy was broadly operationalist. On the other hand his position is far from positivist, in Mach's sense: Bohr is clear that observations, whether or not they are reducible to sense impressions (as it happens he grants that they are), must be expressed in terms of concepts - they are *interpreted* - and in this precisely where one puts the boundary between the observed system and the context of the observation is somewhat arbitrary. Bohr repeatedly spoke of "measurement" ("agencies of measurement") in the sequel: the boundary at issue for Bohr as much marks the distinction between the context of the experiment and the object under investigation, as between "the observer" and "the observed". In his later writings it was the former that was increasingly to the fore.[5]

He immediately continues:

> This situation has far-reaching consequences. On 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 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 co-ordination and the claim of causality, the union of which characterizes the classical theories, as *complementary* but exclusive features of the description, symbolizing the idealization of observation and definition respectively. [2, p.54, emphasis original]

This is the first time that the word "complementary" was used by Bohr.

The simplest reading of this passage is more strongly operationalist: the concepts of space and time have no meaning independent of a context of observation. As a philosophical doctrine it will therefore apply equally to classical physics. The difference, for microscopic quantum phenomena, is that the act of observation cannot under any circumstances be neglected. Because of the quantum postulate, there is an irreducible coupling between apparatus and measured system, that cannot be made arbitrarily small.[6] A microscopic system to which spacetime coordinates can be assigned can therefore never be considered a closed system, not even as an idealization. So equations of motion in the customary

[5] For a typical example: "We must recognize that a measurement can mean nothing else than the unambiguous comparison of some property of the object under investigation with a corresponding property of another system, serving as a measuring instrument, and for which this property is directly determinable according to its definition in everyday language or in the terminology of classical physics."[7, p.19]

[6] Understood as a statement about energy or momentum transfers, this point is not entirely general. What is entirely general is that, for a given basis, the presence or absence of *entanglement* can never be neglected. Much hangs on this distinction.

form are not available; what equations may be found for it will not conserve energy and momentum; no "causal" description is possible. In this sense space-time coordination and causality cannot be combined. Further, at least in a number of important examples, the reciprocal nature of this limitation can be quantified by means of the uncertainty relations.

This reading is consistent with the rest of the Como lecture. There and in his later writings Bohr repeatedly gave examples to show that the attempt to give an operational meaning to the spatiotemporal coordinates of a phenomenon leads to an uncontrollable flow of momentum and energy into and out of the system, of just such an amount as to satisfy the uncertainty relations. As of the Como lecture the foundation of the latter was the de Broglie relations: in them the "fundamental contrast between the quantum of action and the classical concepts is immediately apparent". Momentum and energy (the basis of a "causal" description) is thereby related to wavelength and frequency; with that the uncertainty relations (or near neighbours of them) follow immediately:

> At the same time, the possibility of identifying the velocity of the particle with the group-velocity indicates the field of application of space-time pictures in the quantum theory. Here the complementary character of the description appears, since the use of wave-groups is necessarily accompanied by a lack of sharpness in the definition of period and wave-length, and hence also in the definition of the corresponding energy and momentum as given by [the de Broglie relations]. [2, p.59]

Bohr's derivation of the uncertainty relations depended only on the de Broglie relations and the concepts of elementary (linear) wave theory.

This point is worth emphasizing, for whilst Bohr, as of the time of the Como lecture, had finally taken on board what he had always regarded as the most profound paradox of the quantum theory - the wave-particle duality - he had yet to absorb even quite superficial features of the new mechanics. As he apologetically prefaced his address:

> [A]mong the audience there will be several who, due to their participation in the remarkable recent development, will surely be more conversant with details of the highly developed formalism than I am. Still, I shall try, by making use only of simple considerations and without going into any details of the technical mathematical character, to describe to you a certain general point of view *which I believe is suited to give an impression of the general trend of the development of the theory from its very beginning* and which I hope will be helpful in order to harmonize the apparently conflicting views taken by different scientists. [2, p.52, emphasis mine]

It is most unlikely that Bohr meant by the "very beginning" the beginning of the *new* quantum mechanics (no more than two years old). Evidently, he was as much concerned, in the Como lecture, with the trend of development of the *old* quantum theory, as with the new.

Our reading of Bohr to this point is relatively uncontroversial, but the initial steps of Bohr's argument remain obscure. What, precisely, is the "quantum postulate", and why does it lead to what I am calling the "no-separation" principle - the doctrine that neither the agency of observation nor the object observed can be ascribed "independent reality in the ordinary physical sense"? Bohr presented this conclusion as an immediate consequence of the impossibility of neglecting the measurement interaction, but it is not clear if this is a reference to the "individuality" of an atomic process - whatever, precisely, that may mean - or its "essential discontinuity". But either way, Bohr is evidently taking it as an "external disturbance" to a system (for only then does it imply that on measurement a system *cannot* be free of any external disturbance). It is tempting to go on to take Bohr to mean "disturbance" in its normal sense, as a causal physical process. With that one is led very quickly to simple-minded *disturbance theory of measurement*. The observed system is *disturbed* by its interaction with the measuring apparatus, so it can no longer be treated as isolated (and the energy-momentum conservation laws will no longer hold for it, so "causality" is violated).

The disturbance theory of measurement had the virtues of simplicity and clarity, and it was certainly popular; it figured in many of the early texts in quantum mechanics; but it falls foul to obvious objections. Why not seek to correct for the disturbance, as one does classically, in cases where the measurement interaction is not in fact negligible? (obviating the no-separation principle). Why not, to this end, include the measuring apparatus in with the measured system, and model the interaction between the two directly, in quantum mechanical terms? We know of course of one answer to this question: that if we apply the unitary equations of motion to the apparatus as well as to the system measured, we are led to a description of no *one* definite outcome at all - apparently, to nonsense. But Bohr did not begin with the abstract formalism; he did not acknowledge the problem of measurement as such. His point was not: here is the problem of measurement, to solve it we have to insist on X. Bohr's point is: here is the quantum postulate and some philosophical or physical principles; deduce X (and from X, one might hope, solve or dissolve the problem of measurement). So what, according to Bohr, is wrong with applying the ordinary equations (suitable for a closed system) to the apparatus and observed system taken together? Bohr can continue to insist that that description, if cast in terms of space and time coordinates, must itself be observed by some agency (to give "sense" to the coordinations), but why an *outside* agency? Cannot the universe be observed from within? Is he bound by some neo-Kantian injunction or what-not against global descriptions?

On this reading Bohr is on awkward ground. His thesis is in danger of becoming overtly philosophical, and hostage to philosophical arguments that may take unforeseen directions. It is hardly what he intends. He is trying to do justice "to the general trend of the development of the theory" (including the old quantum theory) as recognized by physicists, not in accordance with arcane metaphysical principles. To insist on clear operational meanings to physical concepts is one thing; on Kantian bounds of sense is quite another.

All of this flows from the simple-minded picture of the experiment as introducing a disturbance in the object measured. If this were the whole story, Bohr, on review of Heisenberg's operational analysis of the uncertainty relations in terms of a disturbance on measurement, would not have continued as he did:

> 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 coordinates, 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, as will be clear from the proceeding analysis, 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 coordinates of the particle are sufficiently small. [2, p.63, emphasis mine]

Bohr went on to speak of "the complementarity of the possibilities of *definition*", emphasizing that "the agreement between the possibilities of *observation* and those of *definition* can be directly shown" (emphasis mine); that sets clear limits to any positivist elements in Bohr's operationalism. But the most decisive reason to reject the assimilation of complementarity as of this point to a disturbance theory of measurement is given, not by Bohr, but by Heisenberg, in a note added in proof to his paper on the uncertainty relations:

> Bohr has brought to my attention that I have overlooked essential points in the course of several discussions in this paper. Above all, 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. [25, p.198]

That is precisely how Bohr went on to illustrate complementarity at the beginning of the Como lecture, first in terms of the wave-particle duality for light, and then for matter:

> Just as in the case of light, we have consequently in the question of the nature of matter, so far as we adhere to classical concepts, to face an inevitable dilemma which has to be regarded as the very expression of experimental evidence. In fact, here again we are not dealing with contradictory but with complementary pictures of the

phenomena, which only together offer a natural generalization of the classical mode of description. [2, p.56]

Complementarity, on its first appearance, was thus a thesis concerning the contextuality of the phenomenon to the experiment, as expressed by classical concepts, under a reciprocal latitude in definition as follows from the de Broglie relations; and of the agreement between this and a corresponding reciprocity in their simultaneous measurability. He never changed his views on these matters.

But not all of Bohr's assumptions were properly in evidence in the Como lecture. Bohr spoke of adhering to the classical concepts - but why should we? The challenge was made shortly after by Schrödinger in correspondence: that interesting as the limitations of the classical concepts were, as subject to the uncertainty relations

> [I]t seems to me imperative to demand the introduction of new concepts, in which this limitation no longer occurs. Since what is unobservable in principle should not at all be contained in our conceptual scheme, it should not be representable in terms of the latter. In the adequate conceptual scheme it ought no more to seem that our possibilities of experience are restricted through unfavourable circumstances [11, p.465]

Bohr could not have been more cool in his reply:

> I am scarcely in complete agreement with your stress on the necessity of developing "new" concepts. Not only, as far as I can see, have we up to now no clues for such a re-arrangement, but the "old" experiential concepts seem to me to be inseparably connected with the foundation of man's power of visualising [11, p.465]

He is on shaky ground, however. It was not so long before that Euclidean geometry was supposed to be the only visualizable geometry, the existence of mathematical schemes for non-Euclidean geometries notwithstanding. Bohr's position, at this point, is dogmatic.

If a point of dogma, better state it at the beginning of any argument for complementarity, and better free it from any reliance on dubious empirical claims about our "powers of visualization". It came in the very first paragraph of the "Introductory Survey" to his *Atomic Theory and the Description of Nature*, written in 1929:

> Only by experience itself do we come to recognize those laws which grant us a comprehensive view of the diversity of phenomena. As our knowledge becomes wider, we must always be prepared, therefore, to expect alterations in the points of view best suited for the ordering of our experience. In this connection we must remember, above all, that, as a matter of course, all new experience makes its appearance within the frame of our customary points of view and forms of perception. [2, p.1]

Later on in the same survey the point was made again, this time with reference to "our customary ideas or their direct verbal expressions". This commitment to classical concepts on the basis of their role in ordinary language figured repeatedly in Bohr's subsequent writings. It was, for Bohr, the rational for a far more sweeping commitment. He immediately continued:

> No more is it likely that the fundamental concepts of the classical theories will ever become superfluous for the description of physical experience. The recognition of the indivisibility of the quantum of action, and the determination of its magnitude, not only depend on an analysis of measurements based on classical concepts, but it continues to be the application of these concepts alone that makes it possible to relate the symbolism of the quantum theory to the data of experience. [2, p.16]

The jump from the necessity and unrevisability of the concepts of *everyday experience* to that of the fundamental concepts of *classical theories* was unsubstantiated, however.[7] But almost no-one apart from Schrödinger saw this as a weakness of Bohr's interpretation; and certainly not Einstein, his principal critic.

4 BOHR'S RESPONSE TO THE EPR ARGUMENT

The early 1930s were the must crucial years for the interpretation of quantum mechanics. They followed much publicized and visible encounters between Einstein and Bohr on the subject of foundations (in particular at the 6th Solvay Conference of 1930). With the appearance of the first comprehensive introductions to the subject, by Pauli, Kramers, Jordan and Dirac, and with time to assimilate the new formalism, a deeper appreciation of its paradoxes was in the air. Things came to a head in 1935: in that year a number of criticisms of

[7] One might wonder if, far from exhausting the concepts available in the description of our experience, the use of classical concepts is even so much as consistent with quantum mechanics. This question, of whether classical concepts, taken individually, could so much as be employed in the quantum domain, had guided Heisenberg in his discovery of the uncertainty relations; his conclusion was that they could (and that only their *simultaneous* deployment was circumscribed) so Bohr's piecemeal use of them did at least have Heisenberg's sanction. In Heisenberg's words: "*All concepts which can be 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.*" [25, p.68, emphasis original]. (In point of fact, the claim at this level of generality runs very quickly into trouble. Shortly after, Jordan and Dirac both noted the difficulties of giving any meaning to the time or phase as self-adjoint operators obeying canonical commutation relations with the energy, if the latter is to have a point spectrum. Mathematically, the difficulty is that no quantization procedure has been found in which the full symmetry group of classical phase space, the symplectic group, can be implemented as a group of unitary transformations on a Hilbert space.)

the new mechanics were published, by von Laue, Schrödinger, and (with Podolski and Rosen) Einstein. It was Einstein's parting shot: he left Germany for America in 1933, never to return.

The view is very widely held that Bohr, without admitting it, shifted his position markedly in the face of these developments, and above all the EPR argument. That would be a damaging admission, if true; for not only would Bohr stand revealed as an opportunist, it would show that the community embraced quite distinct orthodoxies without even realizing it. Fortunately, however, whilst there undoubtedly were shifts in Bohr's position, they effected his argumentative strategy more than its substance. On substance the changes were subtle.

This claim needs to be justified. Bohr frequently remarked on the value he placed on his discussions with Einstein, almost all of which took place before Einstein's departure for America. Here is a lesson he said he learned early from them:

> The extent to which renunciation of the visualization of atomic phenomena is imposed upon us by the impossibility of their subdivision is strikingly illustrated by the following example to which Einstein very early called attention and often has reverted. If a semi-reflecting mirror is placed in the way of a photon, having two possibilities for its direction of propagation, the photon may either be recorded on one, and only one, of two photographic plates situated at great distances in the two directions in question, or else we may, by replacing the plates by mirrors, observe effects exhibiting an interference between the two reflected wave-trains. In any attempt of a pictorial representation of the behaviour of the photon 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. [8, p.221]

It is an early example of a *delayed-choice* experiment. One must change the description of a system in the past, needed to explain a measurement, depending on which of two measurements one chooses to make later on.

In one respect this is worse than any non-locality in space, as was shortly to be demonstrated by the EPR argument [22]: it is an action of the present on the past.[8] It is a case in which the phenomenon is contextualized to the experimental conditions; it illustrates Bohr's "no-separation" principle. Bohr had, moreover, already met with attempts by Einstein to extend this to non-locality in a predictive sense (I shall come back to these in a moment). If the paper of Einstein *et al* really came as an "onslaught...as a bolt from the blue", as Rosenfeld later said [29, p.142], the ideas were by no means entirely unfamiliar to Bohr (which does not of course mean that he had anticipated

[8]However it was widely accepted that retrodictions have a rather different status from predictions in quantum mechanics, in view of the fact that - say from successive measurements of position of arbitrary accuracy - one can defeat the uncertainty relations. According to Bohr in the Como lecture, in such cases we deal with an "abstraction, from which no unambiguous information can be obtained".

the argument). Nor did it take him long to respond to it, by his standards - little over a month - in a short note in *Nature* [4]; this, almost verbatim, was the core of the much longer reply he published in the *Physical Review* near the end of the year [5]. In the latter he began with well-known experiments that he had already used as examples of complementarity. According to Bohr, the EPR argument "does not actually involve any greater intricacies than the simple examples discussed above". If Bohr saw anything new in the EPR argument, he did not acknowledge it. Yet for Einstein it was conclusive proof that quantum mechanics was incomplete, a view that he held to the end of his life.

The EPR argument, recall, rested on a sufficiency condition for a quantity to be counted an "element of reality". The condition was that the quantity can be predicted with certainty "without in any way disturbing the system". Depending on which of two experiments was performed on one system, and on the outcome obtained, the value of one or other of two non-commuting quantities associated with a second system could be predicted with certainty. Since this is so even in the absence of any interaction between the two systems, the sufficiency condition is satisfied; so *both* must be elements of reality. But they could not both be represented as such by any single quantum state: quantum mechanical description is therefore incomplete.

The argument turns on the key concepts of "disturbance" and "interaction" in the very context - what Einstein *et al* called "reduction of the wave packet" - that was so critical to Bohr's interpretation (bringing in "the quantum postulate"). Naturally, therefore, according to Bohr of the Como lecture, it is a context in which "an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation." To complete the line of thought he expressed then, this is because the quantum postulate implies that *there is* an interaction between agency of observation and object (one that is *not to be neglected*). What is needed, then, if Bohr is to be consistent with his creed, is to bite the bullet and admit there is still some kind of an interaction even if it is not of the usual sort (and, he might have added, even if it acts at a distance[9]). That is just what he said:

> Of course there is in a case like that just considered no question of a mechanical disturbance or the system under investigation during the last critical phase of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argumentation of the mentioned authors does not justify

[9]There were special difficulties in treating the EPR state dynamically (in contrast to Bohm's later version in terms of corelated spins). Neither in the EPR paper nor in Bohr's reply was any mention made of locality. But the potential non-local character of the influence Bohr spoke of must have been obvious, given Einstein's previous criticisms of quantum mechanics.

> their conclusion that quantum-mechanical description is essentially incomplete. [5, p.700, emphasis original]

(In his earlier one-page reply, Bohr used the phrase "...no question of a direct mechanical interaction....".)

It is a wordier version of Bohr's no-separation principle, but now quite clearly *divorced* from the disturbance picture of measurement. It is the principle that any physically real phenomenon must be specified under definite experimental conditions, so any change in the latter must lead to a change in the former, even if no ordinary interaction is involved. To put it in spacetime terms (which neither Bohr nor Einstein *et al* had, given that the EPR state was defined at only a single instant of time), it is not as though one can hold a part of the phenomenon, the remote part, constant, whilst varying the experimental conditions of the local part of the phenomenon - this would be to try to visualize the phenomenon in accordance with causal spacetime concepts; it would be to ignore the "individual" nature of a quantum phenomenon.

At this point the strain of not interpreting Bohr's non-mechanical interaction as entanglement, and the quantum postulate as state-reduction, becomes well-nigh intolerable, but still it should be resisted. It is not only that he never accepted these identifications, it was that for Bohr, formal concepts like entanglement and state reduction could *never* have been explanatory (the formalism was *only* an abstract calculus). To put the quantum postulate in terms of state reduction is to look at Bohr's theory from the wrong direction.

If we stay with Bohr's own terms, there is not a great deal to add in reply to Einstein *et al* - unless it is to illustrate how Bohr's no-separation principle had functioned all along, in the familiar cases he had already analyzed in his Como lecture. That is precisely what the bulk of his reply in the *Physical Review* contained. But in the course of it he did refine his position in certain respects, and he took the opportunity, naturally enough, to put the matter more as he had done in his Introductory Survey of 1929:

> While, however, in classical physics the distinction between object and measuring agencies does not entail any difference in the character of the description of the phenomena concerned, its fundamental importance in quantum theory, as we have seen, has its root in the indispensable use of classical concepts in the interpretation of all proper measurements, even though the classical theories do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics. In accordance with this situation there can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow us to predict the results to be obtained by a given experimental arrangement described in a totally classical way. [5, p.701]

Here Bohr is crystal-clear. His interpretation is of the phenomena in terms of classical *concepts*, even though no classical *theory* can account for such regularities. This was the heart of what was really innovative about Bohr's principle of complementarity: how could one describe regularities classically, when they could not be described by any classical theory?

Before coming to that, we should take note of what is genuinely new in Bohr's statement over and above that in the "Introductory Survey": it is his insistence that the experimental arrangement be described in a *totally* classical way, meaning there was no reciprocal latitude needed in any of the classical concepts involved (the uncertainty relations do not apply).[10] Use of constraints on the possible "latitudes of definition" characterizes rather the "quantum mechanical description". Thus if, in an experiment to measure the position of a particle (using a rigidly mounted diaphragm), one wishes instead to control for the momentum of the diaphragm, then it must, "as regards its position relative to the rest of the apparatus, be treated, like the particle traversing the slit, as an object of investigation, in the sense that the quantum mechanical uncertainty relations regarding its position and momentum must be taken explicitly into account." ([5, p.698]).

One can read the shift as reflecting the need for the von Neumann "cut", but again this is to put it in terms quite foreign to Bohr. It is rather a matter of recognizing that eventually one must make use of an apparatus whose reaction to the process of measurement cannot itself be controlled. There must always come a point where it is impossible to keep track of any energy and momentum flows between the apparatus and the object that is measured (as the change in momentum of the apparatus, let alone the change in position - relative to what? - become totally inaccessible). One way of making the point is by insisting that the uncertainty relations are not to be applied to the apparatus.[11] Along the way, insofar as Bohr has to draw a definite classical-quantum distinction, it is a convenient tidying-up exercise: why not draw it at the same place as the apparatus-object distinction?[12]

But it is not really needed. We can replace it, as an expression of our pragmatic situation, by the stipulation that the conditions of an experiment must ultimately involve rigid connections to bodies of arbitrarily large mass. In that case the uncertainty relations, for the latter bodies, become irrelevant.[13] Bohr admitted as much when he remarked that the freedom of choice in the divide between quantum and classical was restricted to "a region where the quantum mechanical description of the process concerned is effectively equivalent with the classical description" [5, p.701], and later, when he said that the requirements of unambiguous description of the apparatus "is secured by the use, as measuring instruments, or rigid bodies sufficiently heavy to allow a completely classical account of their relative positions and velocities" [9, p.3].

[10] In the Como lecture Bohr did suggest on at least one clear occasion that the agency of measurement could partake of quantum mechanical uncertainties ([2, p.66]).

[11] See Diósi [21], for a technical treatment along these lines.

[12] Howard calls this the "coincidence interpretation", and goes on to question it, suggesting, even, that subatomic particles might have been counted by Bohr as measuring instruments, so long as they are assigned the right spectrum of classical properties [26]. (My disagreement with him should be clear from the sequel.)

[13] But see Dickson [20], for a rather different view of the matter.

Another tidying-up operation came shortly after his reply to EPR, and was more explicitly terminological. Recognizing, as Bohr may not have appreciated before the EPR argument, that the contextualizing of the phenomenon embodied in the no-separation principle had to be freed much more explicitly from any causal concepts, it would be handy to devise a terminology in terms of which the choice of experimental arrangement strictly does *not* disturb the phenomenon. But that is quite easy to do. As he later reported his proposal, made at Warsaw in 1938:

> I warned especially against phrases, often found in the physics literature, such as "disturbing of phenomena by observation", or "creating physical attributes to atomic objects by measurement". Such phrases, which may serve to remind us of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like "phenomena" and "observations", just as "attributes" and "measurements", are used in a way hardly compatible with common language and practical definition.
>
> As a more appropriate way of expression I advocated the application of the word *phenomenon* exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement. [8, p.237-38, emphasis original]

With that talk of any disturbing of the phenomenon by a change in experimental context becomes *literally* false, in fact a logical contradiction. If the kind of experiment is changed we have a completely different phenomenon.

Does it follow that there is no longer a role for the idea that quantum measurements disturb the system measured? Not in the least: it is essential, to get the whole doctrine of complementarity off the ground, that there are indeed mutually exclusive experimental arrangements. We are not talking of logical incompatibility; the point is not that if one performs a two-slit experiment one cannot at the same time perform a diffraction-grating experiment (a contradiction in terms, even though the observables that are measured commute); the incompatibility rather derives from a physical principle that implies an experiment to measure one classical quantity thereby excludes the possibility of simultaneously measuring another. Here the notion of an irreducible disturbance works perfectly well as limited to a purely local action.[14] As Bohr went on to explain, in defending the completeness of the quantum mechanical description:

> On the contrary this description,[15] as appears from the preceding discussion, may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measurements in the field of quantum theory. [5, p.700]

[14] In fact, by microcausality, this is guaranteed.

[15] Here, for once, we should take Bohr as talking about the quantum state (the EPR state), understanding its decomposition with respect to the position basis or the momentum basis as illustrating the complementary descriptions that can be given of the system.

There would be no difficulty in measuring the position of a shutter as well as its momentum, so performing an inclusive measurement, were it not for this local notion of an uncontrollable disturbance on measurement. Quite distinct from this is the no-separation principle, the non-local sense in which a phenomenon is defined relative to one or another of such mutually exclusive experiments.

All this being so, why did Bohr have any difficulty with the EPR paper? Rosenfeld reported that it was not all plain sailing, or not for the first few days anyway [29]. The argument surely required a different answer to the one Bohr had presumably found to Einstein's earlier attempt to draw out the non-local import of quantum mechanics, as also reported by Rosenfeld[16]; or the answer he had found to Einstein's "photon box" thought experiment, another precursor to the EPR argument, at the 6th Solvay conference. And the mathematical example given in the EPR paper, making use of Dirac delta-function normalization and Fourier transforms of two-particle wave functions, was hardly physically transparent. It made no reference to dynamics, and it was not interpreted in terms of any actual or possible experiment. Bohr undoubtedly had to struggle to find an experimental model for it.

There is one last and crucial component to Bohr's reasoning. I have mentioned a puzzle, following his insistence on the use only of classical concepts in interpreting experiments. How after all does he get beyond classical theory? How to express what cannot be expressed classically? For Bohr, the quantum formalism itself has entirely disappeared from view.[17] In what sense, then, was there any possibility of genuinely non-classical laws? The answer, in a word, is *complementarity*. Bohr immediately continued:

> In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complimentary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic descriptions of science. It is just this entirely new situation as regards the description of physical phenomena that the notion of *complementarity* aims at characterizing. [5, p.700, emphasis original]

Bohr was scarcely under pressure to explain how this was to be done (how to make use of this "new room"); he had showed it by example in the Como lecture and in the discussion just concluded in reply to the EPR argument. It is the idea that this amounted to a *general* new method in the sciences that is the crucial one.

[16]According to Rosenfeld [28], Einstein posed an outline of the EPR argument in Bruxelles in 1933, apparently only to "illustrate the unfamiliar features of quantum phenomena". (In fact the outline is of a rather different, and fallacious argument, similar to the one that Dickson has criticized as an - unattrubuted - *misreading* of the EPR argument [19].)

[17]Equations of quantum mechanics appeared only once in his reply to the EPR argument, and that was in a footnote (presenting the two choices of commutators, for relative positions or total momenta, in terms of the transformation theory).

This idea also preceded the EPR argument. It was clear from Bohr's address to the Scandinavian Meeting of Natural Scientists in 1929, which first appeared in German in 1931 and in English as "The atomic theory and the fundamental principles underlying the description of nature" in 1934 [2, pp.102-19]. In this lecture he proposed that complementarity might apply also to "the more profound biological problems"; there "we must expect to find that the recognition of relationships of wider scope will require that the same conditions be taken into consideration which determine the limitations of the causal mode of description in the case of atomic phenomena". The point was made more starkly in an Addendum to his "Introductory Survey", that he added in 1931: "*the strict application of those concepts which are adapted to our description of inanimate nature might stand in a relationship of exclusion to the consideration of the laws of the phenomena of life*" [2, p.22-23, emphasis original]. He suggested that the same may apply to psychological laws (or rather the opposition of psychological and physical laws), and that the recognition of this "will enable us to comprehend...that harmony which is experienced as free will and analyzed in terms of causality". In his Faraday lecture of 1930 [3], Bohr suggested that the concepts of thermodynamics were mutually exclusive of the concepts of statistical thermodynamics, and that complementarity could be applied to that domain as well.

Complementarity, it was clear by the end of the 1920s, was for Bohr a novel explanatory framework, more general than the traditional one of causal space-time descriptions, that applied in principle to any empirical domain in which concepts could only be applied under mutually exclusive experimental conditions, providing room for the discovery and definition of entirely new laws, despite their restriction to those self-same concepts.

5 BOHR'S LATER PHILOSOPHY

Bohr gave a lengthy commentary on the Como lecture and the subsequent history of his discussions with Einstein in 1949, in his contribution to the Schilpp volume of the *Library of Living Philosophers* devoted to Einstein. It is considered by many as the most authoritative of Bohr's writings on quantum mechanics. In the argument for complementarity two assumptions were now highlighted. For the first (the "necessity of classical concepts"):

> *However far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms.* The argument is simply that by the word "experiment" we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics.[8, p.208, emphasis original]

For the second (the "no-separation principle"):

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 [8, p.209, emphasis original]

The argument for complementarity now depends on the "individuality" of quantum effects (but Bohr had used just this term in his statement of the quantum postulate in the Como lecture):

> 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. Consequently, evidence obtained under different experimental conditions cannot be comprehended within a single picture, but must be regarded as complementary in the sense that only the totality of the phenomena exhausts the possible information about the objects. [8, p.209]

Bohr shortly after continued, driving home the possibility of novelty despite the restriction to classical concepts:

> While the combination of these concepts into a single picture of a causal chain of events is the essence of classical mechanics, room for regularities beyond the grasp of such a description is just afforded by the circumstance that the study of the complementary phenomena demands mutually exclusive experimental arrangements. [8, p.210]

The first and second assumptions were both stated explicitly (and the second also italicized) in the "Introductory Survey" of 1929; the stated argument for complementarity can be found in marginally different forms in every one of the publications we have considered; the argument for the possibility of novelty was clearly stated in 1929, and again in 1930 and 1931. These principles and these arguments were the central ones in his reply to the EPR argument. Commenting on the latter, Bohr again declared "we are here dealing with problems of just the same kind as those raised by Einstein in previous discussions" [8, p.231]

The charge that Bohr's views underwent a radical change as a consequence of the EPR argument, and in particular that the holism of object-apparatus were *later* developments of Bohr's though (e.g. [16, p.32-33][23, p.185]), cannot be sustained. But that is not to say that his theory did not have other failings. It made no direct reference to the mathematical formalism of quantum mechanics, so there is plenty of ambiguity in how to set it out as a formal interpretation.[18] Classical descriptions, within definite latitudes - as given by his quasi-classical

[18] It left open, in particular, the possibility that complementarity applied equally to any pair of canonically conjugate variables (and that energy and momentum *vs* spacetime coordination is only one example among many).

formulation of the uncertainty relations - replaced quantum ones, in a procedure of doubtful generality; and it admitted, without comment or explanation, the non-local sense in which a phenomenon is defined by its context. Finally, the latter continued to be subsumed under the notion of "interaction", without comment or qualification. This was true even in his most careful statement of complementarity in 1958:

> Far from restricting our efforts to put questions to nature in the form of experiments, the notion of complementarity simply characterizes the answers we can receive by such inquiry, whenever the interaction between the measuring instruments and the objects forms an integral part of the phenomena. [9, p.4]

In fact all Bohr's attempts - half-hearted at best - to derive his conclusions from independent and precisely stated hypotheses must be judged failures. Witness, in 1949, the founding principle, "the *impossibility* of any sharp separation between the behaviour of atomic objects and the interaction with the measuring instruments"; and again in the very same publication, in summarizing the lesson of a variety of thought experiments, that "the main point here is the *distinction* between the objects under investigation and the measuring instruments which serve to define, in classical terms, the conditions under which the phenomena appear." Bohr had put the latter point even more strongly in his reply to EPR:

> The necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a principal distinction between classical and quantum-mechanical description of physical phenomena. [5, p.150]

It is not at all clear, at this point, just what the assumptions of his theory of complementarity really are: the impossibility of making a sharp separation; the necessity for making a sharp separation; and somewhere in this, the quantum postulate.

In Bohr's final paper on the subject, he first emphasized the foundation of any kind of unambiguous physical evidence in the formation of permanent marks

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Bohr did of course acknowledge that from the transformation theory and non-commutativity quite generally it followed "that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables" [5, p.696]. He also acknowledged that "in the quantum mechanical description our freedom of constructing and handling the experimental arrangement finds its proper expression in the possibility of choosing the classically defined paramters entering in any proper application of the formalism" [8, p.229]. It does not follow that energy-momentum *vs* spacetime coordination do not have a special significance in his theory. It was exclusively these that were at issue in every argument and example of complementarity that he gave in the texts that we have considered. (He made two mentions of the transformation theory in the Como lecture, in neither case linking it to complementary; one other in his reply to EPR, already mentioned; and none other in his later writings.)

"such as a spot on a photographic plate caused by the impact of an electron", by processes of "irreversible amplification". The necessity of a object-apparatus divide now follows as a consequence of his earlier stipulation of 1935:

> In all such points, the observation problem of quantum physics in no way differs from the classical physical approach. The essentially new feature in the analysis of quantum phenomena is, however, the introduction of a *fundamental distinction between the measuring apparatus and the objects under investigation*. This is a direct consequence of the necessity of accounting for the functions of the measuring instruments in purely classical terms, excluding in principle any regard for the quantum of action. [9, p.3-4]

In the same essay we read that the fundamental reason why quantum indeterminism cannot be read as a species of classical statistical mechanics is:

> In the case of quantum phenomena, the unlimited divisibility of events implied in such an account is, in principle, excluded by the requirement to specify the experimental conditions. Indeed, the feature of wholeness typical of proper quantum phenomena finds its logical expression in the circumstance that any attempt at a well-defined subdivision would demand a change in the experimental arrangement incompatible with the definition of the phenomena under investigation.[9, p.4]

Natural or not, there is no contradiction between these principles. The latter insists there is no object without a context, the former that the context is purely classical - and has to be specified as such[19] to determine an unambiguous spacetime coordination of the phenomenon. All experiments (what Bohr also called the "proper" measurement instruments [8, p.221]) must ultimately be described in terms of their arrangement in space and time. As Bohr added:

> [T]he ascertaining of the presence of an atomic particle in a limited space-time domain demands an experimental arrangement involving a transfer of momentum and energy to bodies such as fixed scales and synchronized clocks, which cannot be included in the description of their functioning, if these bodies are to fulfil the role of defining the reference frame. [9, p.5]

Bohr does I think have a consistent message, although there were pitfalls in decoding it. He was never able to set it out deductively; he never succeeded in defining the quantum postulate or his other attempted principles on a clear phenomenological basis, on a par with the light postulate of special relativity (the paradigm he clearly sought to emulate). But to my mind much the most important of his failings is that he did not clearly acknowledge that at most

[19] Or as rigidly connected to bodies of arbitrarily large mass.

complementarity was a *conjecture*; that there are new *possibilities* for the definition of "the phenomena" opened up by complementarity that *it may or may not be possible* to integrate within a causal spacetime picture. There is nothing in his arguments to show that that is impossible, or that the quantum postulate must always remain an inscrutable ("irrational") foundation to the theory. Bohr had only a *theory* that there could be no such theory.

6 COMPLEMENTARITY AND THE FORMALISM OF QUANTUM MECHANICS

I am in agreement with Scheibe, in his much-admired review of Bohr's philosophy, that "there is no *single* formulation of quantum mechanics based entirely and consistently on the principles proposed by Bohr", but I disagree when he goes on to say:

> [W]e have in fact no technically elaborated formulation of Bohr's quantum mechanics and that *consequently* it is not at present possible to make a really useful assessment of his contributions."[33, p.5, emphasis original]

Bohr did not so much as *attempt* to give a formulation of quantum mechanics, of the sort that Scheibe was interested in; now was he interested in explaining the formalism. He was interested in the qualitative phenomena.

Bohr did not write down the Schrödinger equation in any of the writings we have considered; the canonical commutation relations only twice. The formalism, only once mentioned in his extensive review of 1949, was "an adequate tool for a complementary way of description", it "represents a purely symbolic scheme permitting only predictions". In 1958, that in the Schrödinger equation "we are here dealing with a purely symbolic procedure, the unambiguous physical interpretation of which in the last resort requires a reference to a complete experimental arrangement". The formalism has a "non-pictorial character". The fact that the wave-function is defined on configuration space rather than position space, and the essential role played by complex numbers in it, were cited by Bohr (from the Como lecture on) as reasons to view it as *purely* abstract (a point of view shared by de Broglie).

Present at the Warsaw conference of 1938 was John von Neumann. Like Bohr, he gave an address on the interpretation of quantum mechanics, but very different in spirit. He gave an axiomatization of quantum mechanics as a projective geometry, over one of the reals, complex numbers or quaternions, in terms of purely lattice-theoretic axioms. They were in turn interpreted as the expression of new logical laws. Indeterminacy, in the quantum mechanical sense, was related (so he argued) to the failure of distributivity. Von Neumann promised further connections with the continuous geometries that had arisen in his recent work on the classification of operator algebras.

For Bohr it was a stretch. But we learn something of his attitude to complementarity from his reply, as reported thus:

> We must also notice that the question of the logical forms which are best adapted to quantum theory is in fact a practical problem, concerned with the choice of the most convenient manner in which to express the new situation that arises in this domain. Personally, he compelled himself to keep the logical forms of daily life to which actual experiments were necessarily confined. The aim of the idea of complementarity was to allow of keeping the usual logical forms while procuring the extension necessary for including the new situation relative to the problem of observation in atomic physics. [11, p.xxx]

Bohr was prepared to acknowledge the possibility of a revised logic, and alternatives to complementarity. But as usual he was dogmatic about the impossibility of a classically visualizable interpretation of quantum phenomena:

> [I]t seems likely that the introduction of still further abstractions into the formalism will be required to account for the novel features revealed by the exploration of atomic processes of very high energy. The decisive point, however, is that in this connection there is no question of reverting to a mode of description which fulfils to a higher degree the accustomed demands regarding pictorial representation of the relationship between cause and effect. [11, p.xxx]

His case was unproven, however. The pilot-wave theory is a counterexample. Why could there be no question of reverting to determinism? It is a disaster for our understanding of Bohr and his doctrines that Bohr never made any public statement on it.

On the measurement problem Bohr was almost as evasive. Bohr never chose to address it as it arises at the formal level, in terms of the contrast between the projection postulate and the unitary equations of motion. There is only one comment he made in print that we can be sure was directed at the problem in this sense. At the same Warsaw conference, Bialobizeski, the President of the University and the chair of the conference, reviewed, somewhat imperfectly, von Neumann's formulation of the measurement problem along these formal lines. He suggested that the duality between the unitary equations and the projection postulate could not be assimilated to the choice of one or another of two complementary descriptions, and that it must remain a fundamental posit of the theory. Bialobizeski surely had a point. Here is the report of Bohr's response:

> [T]he duality he noticed in the interpretation of the formalism of quantum mechanics was, in his opinion, a question of choosing the most adequate description of the experiment. If we decide to include in the enumeration of the exterior conditions all the instruments which must be used for the study of the whole phenomenon, the only arbitrary factor remaining is, as he had explained in his paper, the free choice of these experimental conditions, and, apart from this

> freedom, the interpretation of the solution of the problem, concerning the predictions to which the phenomenon we are studying leads, is perfectly unequivocal.[11, p.xxx]

The comment is too cryptic to be really illuminating. One can read a plausible story into it: it is that the free choice of the experimental conditions determines, as a choice of the parts of the apparatus in space and time (the positions of diaphragms, mirrors, springs and what have you), the relevant basis (the classical variables defined with what latitudes), and that once this is fixed, the probabilistic interpretation is unequivocal - just as effected by the projection postulate (as a device to define probabilities from the wave-function). Wave-packet collapse, real (as in a stochastic theory) or effective (as in pilot-wave theory or in the Everett interpretation) can then be identified with the individual and holistic element of the quantum postulate as well as with its uncontrollable element - but this is all to put the matter in terms Bohr could hardly have recognized. If Bialobizeski had pressed the suggestion that the unitary equations also explicitly incorporate the choice of experiment, the deeper objection that Bohr was really committed to was that *the unitary dynamics means nothing at all in itself*. That there *is* no causal, spacetime description thus provided that has to be suspended, when a measurement is performed. The quantum formalism is *only* an abstract calculus. As we have seen, Bohr made this point over and over again.

7 COMPLEMENTARITY AS A CONJECTURE

Complementarity, as I read it, was a conjecture of unusually wide scope: that where the experimental definition of certain concepts precluded the definition of others, new regularities could be defined, that could not be captured by any unified treatment involving all of them. Similarly, one might conjecture that cosmology as an empirical discipline is impossible; or that science as a strictly unified discipline is impossible. Closer to home is Rovelli's recent conjecture [30], that whilst any system can be modelled piecemeal in quantum mechanics, no model can be given of the totality of physical systems; or conjectures by Fuchs and others [24][36] on the possibilities of encoding information and the transfer of information. Bohr has plenty of emulators today.

I see no *a priori* principle that rules out any of these strategies, but that is only to say that they are at the end *scientific* conjectures, albeit at an unusually high level of abstraction; they stand or fall by their durability and success. When it comes to complementarity, surely, the jury is by now finally in.

In its negative claims complementarity denied the possibility of a causal spacetime explanation for key experiments of quantum mechanics. In addition, Bohr insisted that the formalism can only be interpreted by specification of a (classically defined) context of measurement. But there are now plenty of examples of causal spacetime explanations for the phenomena that Bohr considered (as given in all the major realist schools today, whether pilot-wave theory, GRW theory, or the Everett interpretation); and we have in decoherence theory techniques for obtaining approximately classical descriptions from quantum ones that evade Bohr's strictures entirely.

On the positive side, Bohr did offer a framework for the analysis of quantum phenomena in terms of classical concepts. Here we may grant him limited success. The key point is that he offered an avenue - a new method in science - whereby apparently inconsistent explanations could be reconciled. But it would have been *ad hoc* to restrict it to quantum physics; if a new scientific method, it should have applications in other fields as well. On two counts progress was expected: progress with complementary theorizing in quantum physics - reasoning along the lines he laid out in the Como lecture, in his reply to EPR, and in his 1949 review - and progress with a similar style of theorizing in other disciplines, whose elementary concepts also stand in such a relation of mutual exclusion. Neither has been forthcoming.[20]

The historical record is clearly negative. But these failures could not have been foreseen in the 1920s and '30s. In retrospect, it is clear that Bohr's philosophy was essentially conservative, an extension of the methods that had served him and his contemporaries well in the creation of quantum mechanics. If a competing approach to the theory required their abandonment, without benefit in terms of new experiments, little wonder that Bohr won the day: progress by increment, by conservative extension of existing concepts and fragments of theories, may not exactly be a principle of rationality, but *pace* Popper and Kuhn, it is has served dynamics well through its long history [31].

I come back to the pilot-wave theory. At certain points I have said that it is a counter-example to Bohr's claims. How then could he have ignored it? Bohr's negligence in this respect may seem remarkable - so much so that it puts in question our whole reading of his philosophy. But it is much worse on Heisenberg's reading, that the "Copenhagen" interpretation, as he called it, was based on the need to introduce "the observer" in physics;[21] or on von Neumann's, that the measurement postulates only reflected the fact that any account of the objective world must ultimately terminate in *conscious* perception. Claims of

[20]Bohr, in collaboration with Rosenfeld in 1933 did gave one other example of the genre (an operational analysis of the limits to definability in free quantum electromagnetic field theory [12]); and later, in 1950, an attempt at a comparable study of the interacting theory [13]. But the panalophy of levers, trapdoors and springs so introduced seemed little sort of baroque.

Bohr was surely disappointed by the failure of complementarity in other fields. Whilst he still held out the hope of applications of complementarity to biology and the social sciences in 1958, he was markedly less optimistic in his final comments on the subject in 1962, a few months before his death. There he acknowledged that the use of teleological explanations in biology did not in fact imply any restriction on the application of physics to that field, adding, in a departure from his text, that "in the last resort, it is a matter of how one makes headway in biology. I think that the feeling of wonder which the physicists had thirty years ago has taken a new turn. Life will always be a wonder, but what changes is the balance between the feeling of wonder and the courage to try to understand" [10, p.26] (The implication, that with complementarity one did not have the courage to understand, was surely unintended.)

[21]Heisenberg's reading of orthodoxy made a happy target, in different ways, for Popper, Feyerabend, and Hanson, who thereby helped to popularize it (I am indebted to Don Howard for this and other observations on this history.)

this scope are *straightforwardly* refuted by the pilot-wave theory.

Consider again the conservatism of Bohr's assumptions. He insisted on no philosophical principle, unless it was a broad and shallow operationalism. He was dogmatic only in regard to the indispensability of classical concepts. He offered cogent reasons to believe that the classical ideal of explanation, as a causal spacetime description, may not be available in the quantum domain. In its place he provided simple, context-relative accounts of paradigmatic quantum phenomena in terms of classical concepts, many of them simple extensions of the ideas of the old quantum theory. Against it pilot-wave theory made of these quantities (apart from position), in any dynamical context, nothing but *epiphenomena*, artifacts of the kinematical limit. Focus instead on the *real* motions in configuration space, and all the usual interactions of electrodynamics appear to be something close to *nonsense*.[22] It made nonsense of some of the new equations as well (for example, the uncertainty relations): considered as "a principle which is necessary for the interpretation of the *totality* of quantum phenomena"

> These results, which the coherence and experimental verification of the new mechanics have placed beyond any doubt, can in no way be reconciled with the pilot-wave theory. The latter leads in particular to a well-defined value of the linear momentum and does not allow us to obtain the uncertainty relations. [18, p.177]

The remark was made by none other than de Broglie, a quarter of a century *after* the 5th Solvay conference; he allowed it to stand even *after* reading Bohm's papers of 1952. It shows how hard it was for him to accept that the dynamical quantities revealed by experiment were not the real ones outside of the measurement context, and *vice versa*.

Total energy, kinetic energy, momentum and spin as revealed by measurement are not in general to be found in the particle trajectories [14]. The pilot-wave theory is contextual with respect to all of them, in the technical sense, as required by the Kochen-Specker-Bell theorem, but (with the possible exception of spin) they are contextualized in Bohr's sense too - which of such variables has what meaning (what "latitude") is a matter of the experimental context. And what the pilot-wave theory does *not* deliver is a causal, spacetime account *of those very variables, across all contexts* - just because they do not attach to the particle trajectories. In this sense, it may even be said, the pilot-wave theory is no counter-example to the principle of complementarity.

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[22] For example, the electron may be at rest within the atom, despite the Coulomb interaction; a photon reflected from a mirror may be perfectly at rest. (These examples were pointed out by Brillouin at the 5th Solvay conference [34, p.120, 137-40, 266].)

particular light I have tried to shed on it), and to Guido Bacciagaluppi and, especially, to Antony Valentini, for any number of historical corrections (they are not at fault for any remaining ones!). Above all my thanks to the editors for the opportunity to express my thanks and appreciation for the life and work of Jim Cushing; for his friendship and support in difficult times, and for his fearless questioning in the easy ones.

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