The Copenhagen Interpretation*

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An attempt is made to give a coherent account of the logical essence of the Copenhagen interpretation of quantum theory. The central point is that quantum theory is fundamentally pragmatic, but nonetheless complete. The principal difficulty in understanding quantum theory lies in the fact that its completeness is incompatible with external existence of the space–time continuum of classical physics.

I. INTRODUCTION

Scientists of the late twenties, led by Bohr and Heisenberg, proposed a conception of nature radically different from that of their predecessors. The new conception, which grew out of efforts to comprehend the apparently irrational behavior of nature in the realm of quantum effects, was not simply a new catalog of the elementary space–time realities and their modes of operation. It was essentially a rejection of the presumption that nature could be understood in terms of elementary space–time realities. According to the new view, the complete description of nature at the atomic level was given by probability functions that referred not to underlying microscopic space–time realities but rather to the macroscopic objects of sense experience. The theoretical structure did not extend down and anchor itself on fundamental microscopic space–time realities. Instead it turned back and anchored itself in the concrete sense realities that form the basis of social life.

This radical concept, called the Copenhagen interpretation, was bitterly challenged at first but became during the '30's the orthodox interpretation of quantum theory, nominally accepted by almost all textbooks and practical workers in the field.

Recently, perhaps partly in response to the severe technical difficulties now besetting quantum theory at the fundamental level, there has been mounting criticism of the Copenhagen interpretation. The charges range from the claim that it is a great illogical muddle to the claim that it is in any case unnecessary, and hence, in view of its radical nature, to be rejected. Reference 1 contains some stoutly worded attacks on the Copenhagen interpretation. Reference 2 is a more moderately worded review article that firmly rejects the Copenhagen interpretation. Reference 3 is a list of recent articles in the physical literature that espouse a variety of views on the question.

The striking thing about these articles is the diversity they reveal in prevailing conceptions of the Copenhagen interpretation itself. For example, the picture of the Copenhagen interpretation painted in Ref. 1 is quite different from the pictures painted in Refs. 2 and 3 by practicing physicists. And these latter pictures themselves are far from uniform.

The cause of these divergences is not hard to find. Textbook accounts of the Copenhagen interpretation generally gloss over the subtle points. For clarification the readers are directed to the writings of Bohr[4] and Heisenberg.[5] Yet clarification is difficult to find there. The writings of Bohr are extraordinarily elusive. They rarely seem to say what you want to know. They weave a web of words around the Copenhagen interpretation but do not say exactly what it is. Heisenberg's writings are more direct. But his way of speaking suggests a subjective interpretation that appears quite contrary to the apparent intentions of Bohr. The situation is perhaps well summarized by von Weizsäcker, who, after expressing the opinion that the Copenhagen interpretation is correct and indispensable, says he must "add that the interpretation, in my view, has never been fully clarified. It needs an interpretation, and that will be its only defense."[6]

von Weizsäcker is surely correct. The writings of Bohr and Heisenberg have, as a matter of historical fact, not produced a clear and unambiguous picture of the basic logical structure of their position. They have left impressions that vary significantly from reader to reader. For this reason a clarification of the Copenhagen interpretation is certainly needed. My aim here is to provide one. More precisely, my aim is to give a clear account of the logical essence of the Copenhagen interpretation. This logical essence should be distinguished from the inhomogeneous body of opinions and views that now constitute the Copenhagen interpretation itself. The logical essence constitutes, I believe, a completely rational and coherent position.

The plan of the work is as follows. First, quantum theory is described from the point of view of actual practice. Then, to provide contrast, several non-Copenhagen interpretations are considered. Next, to provide background, some philosophical ideas of William James are introduced. The pragmatic character of the Copenhagen interpretation is then discussed, and the incompatibility of the completeness of quantum theory with the external existence of the space–time continuum of classical physics is noted. Finally, the question of the completeness of quantum theory is examined.

II. A PRACTICAL ACCOUNT OF QUANTUM THEORY

Quantum theory is a procedure by which scientists predict probabilities that measurements of specified kinds will yield results of specified kinds in situations of specified kinds. It is applied in circumstances that are described by saying that a certain physical system is first prepared in a specified manner and is later examined in a specified manner. And this examination, called a measurement, is moreover such that it can yield, or not yield, various possible specified results.

The procedure is this: The specifications A on the manner of preparation of the physical system are first transcribed into a wave function $\Psi_A(x)$. The variables x are a set of variables that are characteristic of the physical system being prepared. They are called the degrees of freedom of the prepared system. The description of the specifications A is couched in a language that is meaningful to an engineer or laboratory technician. The way in which these operational specifications A are translated into a corresponding wave function $\Psi_A(x)$ is discussed later.

The specifications B on the subsequent measurement and its possible result are similarly couched in a language that allows a suitably trained technician to set up a measurement of the specified kind and to determine whether the result that occurs is a result of the specified kind. These specifications B on the measurement and its result are transcribed into a wave function $\Psi_B(y)$, where y is a set of variables that are called the degrees of freedom of the measured system.

Next a transformation function $U(x;\ y)$ is constructed in accordance with certain theoretical rules. This function depends on the type of system that was prepared and on the type of system that was measured, but not on the particular wave functions $\Psi_A(x)$ and $\Psi_B(y)$. The "transition amplitude"

$$\langle A \mid B \rangle \equiv \int \Psi_A(x) U(x;\ y) \Psi_B^*(y) dx dy$$

is computed. The predicted probability that a measurement performed in the manner specified by B will yield a result specified by B, if the preparation is performed in the manner specified by A, is given by

$$P(A, B) = | \langle A \mid B \rangle |^2.$$

The experimental physicist will, I hope, recognize in this account a description of how he uses quantum theory. First he transforms his information about the preparation of the system into an initial wave function. Then he applies to it some linear transformation, calculated perhaps from the Schrödinger equation, or perhaps from the S matrix, which converts the initial wave function into a final wave function. This final wave function, which is built on the degrees of freedom of the measured system, is then folded into the wave function corresponding to a possible result. This gives the transition amplitude, which is multiplied by its complex conjugate to give the predicted transition probability.

In a more sophisticated calculation one might use density matrices $\rho_A(x';x'')$ and $\rho_B(y';y'')$ instead of $\Psi_A(x)$ and $\Psi_B(y)$ to represent the prepared system and the possible result. This would allow for preparations and measurements that correspond to statistical mixtures. But this generalization could be obtained also by simply performing classical averages over various $\Psi_A(x)$ and $\Psi_B(y)$.

The above account describes how quantum theory is used in practice. The essential points are that attention is focused on some system that is first prepared in a specified manner and later examined in a specified manner. Quantum theory is a procedure for calculating the predicted probability that the specified type of examination will yield some specified result. This predicted probability is the predicted limit of the relative frequency of occurrence of the specified result, as the number of systems prepared and examined in accordance with the specifications goes to infinity.

The wave functions used in these calculations are functions of a set of variables characteristic of the prepared and measured systems. These systems are often microscopic and not directly observable. No wave functions of the preparing and measuring devices enter into the calculation. These devices are described operationally. They are described in terms of things that can be recognized and/or acted upon by technicians. These descriptions refer to the macroscopic properties of the preparing and measuring devices.

The crucial question is how does one determine the transformations $A \to \Psi_A$ and $B \to \Psi_B$. These transformations transcribe procedural descriptions of the manner in which technicians prepare macroscopic objects, and recognize macroscopic responses, into mathematical functions built on the degrees of freedom of the (microscopic) prepared and measured systems. The problem of constructing this mapping is the famous "problem of measurements" in quantum theory.

The problem of measurements was studied by von Neumann.[7] He begins with the idea that one should describe the combined system composed of the original systems plus the original measuring devices in terms of a quantum mechanical wave function, and use quantum theory itself to calculate the needed mappings. This program has never been carried out in any practical case. One difficulty is that actual macroscopic devices are so complicated that qualitative calculations lie beyond present capabilities. The second problem is that such calculations would, in any case, provide only connections between the wave functions ϕ of the preparing and measuring devices and the wave functions Ψ of the original system. There would remain the problem of finding the mappings $A \to \phi_A$ and $B \to \phi_B$.

von Neumann's approach is not the one that is adopted in actual practice; no one has yet made a qualitatively accurate theoretical description of a measuring device. Thus what experimentalists do, in practice, is to *calibrate* their devices.

Notice, in this connection, that if one takes N_A different choices of A and N_B different choices of B, then one has only $N_A + N_B$ unknown functions Ψ_A and Ψ_B, but $N_A \times N_B$ experimentally determinable quantities $|\langle A \mid B \rangle|^2$. Using this leverage, together with plausible assumptions about smoothness, it is possible to build up a catalog of correspondences between what experimental physicists do and see, and the wave functions of the prepared and measured systems. It is this body of accumulated empirical knowledge that bridges the gap between the operational specifications A and B and their mathematical images Ψ_A and Ψ_B.

The above description of how quantum theory is used in practice will be used in the account of the Copenhagen interpretation. Before describing that interpretation itself I shall, to provide contrast, describe several other approaches.

III. SEVERAL OTHER APPROACHES

A. The Absolute-Ψ Approach

von Neumann's lucid analysis of the process of measurement is the origin of much of the current worry about the interpretation of quantum theory. The basic worrisome point can be illustrated by a simple example.

Suppose a particle has just passed through one of two slits. And suppose a 100% efficient counter is placed behind each slit, so that by seeing which counter fires a human observer can determine through which slit the particle passed.

Suppose the particle is represented initially by a wave function that assigns equal probabilities to the parts associated with the two slits. And consider a quantum theoretical analysis of the process of measurement in which both the particle and the two counters are represented by wave functions.

It follows directly and immediately from the superposition principle (i.e., linearity) that the wave function of the complete system after the measurement necessarily will consist of a superposition of two terms. The first term will represent the situation in which (1) the particle has passed through the first counter, (2) the first counter has fired, and (3) the second counter has not fired. The second term will represent the situation in which (1) the particle has passed through the second counter, (2) the second counter has fired, and (3) the first counter has not fired. These two terms evolve from the two terms in the wave function of the initial particle. The presence of both terms is a direct and unavoidable consequence of the superposition principle, which ensures that the sum of any two solutions of the equation of motion is another solution.

Notice now that the counters are macroscopic objects and that the wave function necessarily contains a sum of two terms one of which corresponds to the first counter's having fired but not the second, and the other of which corresponds to the second counter's having fired but not the first. Thus the wave function necessarily corresponds to a sum of two logically incompatible *macroscopic* possibilities.

To dramatize this situation, suppose the human observer now looks at the counters and runs upstairs or downstairs depending on which counter he sees firing. Then the wave function of the entire system of particle plus counters plus human observer will consist, eventually, of a sum of two terms. One term will represent the human observer running upstairs, and the other term will represent this same human observer running downstairs. Both terms must necessarily be present in the wave function, simply by virtue of the superposition principle.

This fact that the wave function necessarily develops into a sum of parts that correspond to incompatible *macroscopic* possibilities must be squared with the empirical facts. The human observer does not run both upstairs and downstairs. He does one or the other, not both. Therefore the wave function must collapse to a form that is consistent with what actually does happen. But such a collapse is definitely incompatible with the superposition principle.

This violation of the superposition principle bothers some thinkers. Wigner[8] calls the existence of the two modes of change of the wave function—i.e., the smooth causal evolution and the fitful statistical jumps associated with measurements—a strange dualism, and says that the probabilistic behavior is almost diametrically opposite to what one would expect from ordinary experience. He and Ludwig[9] speculate that quantum theory may have to be modified by the addition of a nonlinear effect in the macroscopic realm in order to arrive at a consistent theory of measurements. Wigner[10] even speculates that the nonlinearity may be associated with the action of mind on matter.

An even more radical proposal was made by Everett[11] and supported by Wheeler[12] and Bryce DeWitt.[13] According to this proposal the human observer actually runs both upstairs and downstairs at the same time. When the human observer sees the counter fire he breaks into two separate editions of himself, one of which runs upstairs while the other runs down. However, the parts of the wave function corresponding to these two different possibilities move into different regions of the multiparticle configuration space and consequently do not interfere. Therefore the two editions will never be aware of each other's existence. Thus appearances are saved without violating the superposition principle.

This proposal is, I think, unreasonable. A wave function times its complex conjugate, has the mathematical properties of a probability function. Probability functions for composite systems are naturally defined on the product of the spaces of the individual component systems; it is this property that allows different statistical weights to be assigned to the various logically alternative possibilities. A decomposition of a wave function into parts corresponding to different logical alternatives is thus completely natural. In the example described—with the initial specification as described there—there is a finite probability that the observer will be running upstairs, and a finite probability that he will be running downstairs. Thus the wave function necessarily must have both parts. If it collapsed to one part or the other it would no longer correctly describe the probabilities corresponding to the original specifications.

Of course, if the original specifications are replaced by new ones that include now the specification that the observer is running upstairs, not downstairs, then the original wave function will naturally be replaced by a new one, just as it would be in classical statistical theory.

In short, the mathematical properties of the wave functions are completely in accord with the idea that they describe the evolution of the *probabilities* of the actual things, not the actual things themselves. The idea that they describe also the evolution of the actual things themselves leads to metaphysical monstrosities. These might perhaps be accepted if they were the necessary consequences of irrefutable logic. But this is hardly the case here. The basis of Everett's whole proposal is the premise that the superposition principle cannot suddenly fail. This premise is sound. But the natural and reasonable conclusion to draw from it is that the wave functions describe the evolution of the *probabilities* of the actual things, not the evolution of the actual things themselves. For the mathematical form and properties of the wave function, including its lawful development in accordance with the superposition principle, are completely in accord with the presumption that it is a probability function. The addition of the metaphysical assumption that the wave function represents the evolution of not only the probabilities of the actual things, but of also the actual things themselves, is unreasonable because its only virtue is to save the superposition principle, which, however, is not in jeopardy unless one introduces this metaphysical assumption.

Everett's proposal, and also those of Wigner and Ludwig, are the outgrowth of a certain tendency to ascribe to the wave function a quality of absoluteness that goes beyond what is normally and naturally attached to a probability function. This tendency can perhaps be traced to what Rosenfeld[14] calls "a radical difference in conception (going back to von Neumann)...," this radical difference being with the ideas of Bohr. von Neumann's application of quantum theory to the process of measurement itself, coupled with his parallel treatments of the two very different modes of development of the wave function—i.e., the smooth dynamical evolution, and the abrupt changes associated with measurement—tend to conjure up the image of some absolute wave function developing in time under the influence of two different dynamical mechanisms. The living, breathing scientist who changes the wave function he uses as he receives more information is replaced by a new dynamical mechanism. The resulting picture is strange indeed.

In the Copenhagen interpretation the notion of an absolute wave function representing the world itself is unequivocally rejected. Wave functions, like the corresponding probability functions in classical physics, are associated with the studies by scientists of finite systems. The devices that prepare and later examine such systems are regarded as parts of the ordinary classical physical world. Their space–time dispositions are interpreted by the scientist as information about the prepared and examined systems. Only these latter systems are represented by wave functions. The probabilities involved are the probabilities of specified responses of the measuring devices under specified conditions.

New information available to the scientist can be used in two different ways. It can be considered to be information about the response of a measuring device to the system being examined. In this case the probability of this response is the object of interest. On the other hand, the new information can also be regarded as part of the specification of a new preparation. The wave function that represents this new specification will naturally be different from the wave function that represented the original specifications. One would not expect the superposition principle to be maintained in the change of the wave function associated with a change of specifications.

This pragmatic description is to be contrasted with descriptions that attempt to peer "behind the scenes" and tell us what is "really happening." Such superimposed images can be termed metaphysical appendages insofar as they have no testable consequences. The pragmatic interpretation ignores all such metaphysical appendages.

The sharp distinction drawn in this section between probabilities and the actual things to which they refer should not be construed as an acceptance of the real-particle interpretation which is described next.

B. The Real-Particle Interpretation

The real-particle interpretation affirms that there are real particles, by which is meant tiny localized objects, or disturbances, or singularities, or other things that stay together like particles should, and do not spread out like waves. According to this interpretation, the probability functions of quantum theory describe, typically, the probability that a real particle is in such-and-such a region. This real-particle interpretation is defended by Popper in Ref. 1, and by Ballentine in Ref. 2.

Confidence in the existence of real particles was restored by Bohm's[15] illustration of how nonrelativistic Schrödinger theory can be made compatible with the existence of point particles. The price paid for this achievement is this: All the particles in the (model) universe are instantly and forcefully linked together. What happens to any particle in the universe instantly and violently affects every other particle.

In such a situation it is not clear that we should continue to use the term "particle." For the entire collection of "particles" in Bohm's universe acts as a single complex entity. Our usual idea of a particle is an abstraction from experience about macroscopic objects, and it normally carries, as part of the idea of localization, the idea that the localized entity is an independent entity, in the sense that it depends on other things in the universe only through various "dynamical" effects. These dynamical effects are characterized by a certain respect for space–time separations. In particular, they are "causal." If the connections between particles radically transcend our idea of causal dynamical relationships, then the appropriateness of the word "particle" can be questioned.

Recently, Bell[16] has shown that the statistical predictions of quantum theory are definitely incompatible with the existence of an underlying reality whose spatially separated parts are independent realities linked only by causal dynamical relationships. The spatially separated parts of any underlying reality must be linked in ways that completely transcend the realm of causal dynamical connections. The spatially separated parts of any such underlying reality are not independent realities, in the ordinary sense.

Bell's theorem does not absolutely rule out the real-particle interpretation, if one is willing to admit these hyperdynamical connections. But they fortify the opinion that a dynamical theory based on such a real entity would have no testable dynamical consequences. For the strong dependence of individual effects here on Earth upon the fine details of what is happening all over the universe apparently rules out any ordinary kind of test of such a theory.

IV. THE PRAGMATIC CONCEPTION OF TRUTH

To prepare the mind for the Copenhagen interpretation it is useful to recall some ideas of William James.[17] James argued at length for a certain conception of what it means for an idea to be true. This conception was, in brief, that an idea is true if it works.

James's proposal was at first scorned and ridiculed by most philosophers, as might be expected. For most people can plainly see a big difference between whether an idea is true and whether it works. Yet James stoutly defended his idea, claiming that he was misunderstood by his critics.

It is worthwhile to try to see things from James's point of view.

James accepts, as a matter of course, that the truth of an idea means its agreement with reality. The questions are: What is the "reality" with which a true idea agrees? And what is the relationship "agreement with reality" by virtue of which that idea becomes true?

All human ideas lie, by definition, in the realm of experience. Reality, on the other hand, is usually considered to have parts lying outside this realm. The question thus arises: How can an idea lying inside the realm of experience agree with something that lies outside? How does one conceive of a relationship between an idea, on the one hand, and something of such a fundamentally different sort? What is the structural form of that connection between an idea and a transexperiential reality that goes by the name of "agreement"? How can such a relationship be comprehended by thoughts forever confined to the realm of experience?

The contention that underlies James's whole position is, I believe, that a relationship between an idea and something else can be comprehended only if that something else is also an idea. Ideas are eternally confined to the realm of ideas. They can "know" or "agree" only with other ideas. There is no way for a finite mind to comprehend or explain an agreement between an idea and something that lies outside the realm of experience.

So if we want to know what it means for an idea to agree with a reality we must first accept that this reality lies in the realm of experience.

This viewpoint is not in accord with the usual idea of truth. Certain of our ideas are ideas about what lies outside the realm of experience. For example, I may have the idea that the world is made up of tiny objects called particles. According to the usual notion of truth this idea is true or false according to whether or not the world really is made up of such particles. The truth of the idea depends on whether it agrees with something that lies outside the realm of experience.

Now the notion of "agreement" seems to suggest some sort of similarity or congruence of the things that agree. But things that are similar or congruent are generally things of the same kind. Two triangles can be similar or congruent because they are the same kind of thing: The relationships that inhere in one can be mapped in a direct and simple way into the relationships that inhere in the other.

But ideas and external realities are presumably very different kinds of things. Our ideas are intimately associated with certain complex, macroscopic, biological entities—our brains—and the structural forms that can inhere in our ideas would naturally be expected to depend on the structural forms of our brains. External realities, on the other hand, could be structurally very different from human ideas. Hence there is no *a priori* reason to expect that the relationships that constitute or characterize the essence of external reality can be mapped in any simple or direct fashion into the world of human ideas. Yet if no such mapping exists then the whole idea of "agreement" between ideas and external realities becomes obscure.

The only evidence we have on the question of whether human ideas can be brought into exact correspondence with the essences of the external realities is the success of our ideas in bringing order to our physical experience. Yet the success of ideas in this sphere does not ensure the exact correspondence of our ideas to external reality.

On the other hand, the question of whether ideas "agree" with external essences is of no practical importance. What is important is precisely the success of the ideas—if ideas are successful in bringing order to our experience then they are useful even if they do not "agree," in some absolute sense, with the external essences. Moreover, if they are successful in bringing order into our experience then they do "agree" at least with the aspects of our experience that they successfully order. Furthermore, it is only this agreement with aspects of our experience that can ever really be comprehended by man. That which is not an idea is intrinsically incomprehensible, and so are its relationships to other things. This leads to the pragmatic viewpoint that ideas must be judged by their success and utility in the world of ideas and experience, rather than on the basis of some intrinsically incomprehensible "agreement" with nonideas.

The significance of this viewpoint for science is its negation of the idea that the aim of science is to construct a mental or mathematical image of the world itself. According to the pragmatic view, the proper goal of science is to augment and order our experience. A scientific theory should be judged on how well it serves to extend the range of our experience and reduce it to order. It need not provide a mental or mathematical image of the world itself, for the structural form of the world itself may be such that it cannot be placed in simple correspondence with the types of structures that our mental processes can form.

James was accused of subjectivism—of denying the existence of objective reality. In defending himself against this charge, which he termed slanderous, he introduced an interesting ontology consisting of three things: (1) private concepts, (2) sense objects, (3) hypersensible realities. The private concepts are subjective experiences. The sense objects are public sense realities, i.e., sense realities that are independent of the individual. The hypersensible realities are realities that exist independently of all human thinkers.[18]

Of hypersensible realities James can talk only obliquely, since he recognizes both that our knowledge of such things is forever uncertain and that we can moreover never even think of such things without replacing them by mental substitutes that lack the defining characteristics of that which they replace, namely the property of existing independently of all human thinkers.

James's sense objects are curious things. They are sense realities and hence belong to the realm of experience. Yet they are public: They are independent of the individual. They are, in short, objective experiences. The usual idea about experiences is that they are personal or subjective, not public or objective.

This idea of experienced sense objects as public or objective realities runs through James's writings. The experience "tiger" can appear in the mental histories of many different individuals. "That desk" is something that I can grasp and shake, and you also can grasp and shake. About this desk James says "But you and I are commutable here; we can exchange places; and as you go bail for my desk, so I can go bail for yours. This notion of a reality independent of either of us, taken from ordinary experience, lies at the base of the pragmatic definition of truth."[19]

These words should, I think, be linked with Bohr's words about classical concepts as the basis of communication between scientists. In both cases the focus is on the concretely experienced sense realities—such as the shaking of the desk—as the foundation of social reality. From this point of view the objective world is not built basically out of such airy abstractions as electrons and protons and "space." It is founded on the concrete sense realities of social experience, such as a block of concrete held in the hand, a sword forged by a blacksmith, a Geiger counter prepared according to specifications by laboratory technicians and placed in a specified position by experimental physicists.

This brief excursion into philosophy provides background for the Copenhagen interpretation, which is fundamentally a shift to a philosophic perspective resembling that of William James.

V. THE PRAGMATIC CHARACTER OF THE COPENHAGEN INTERPRETATION

The logical essence of the Copenhagen interpretation is summed up in the following two assertions:

(1) The quantum theoretical formalism is to be interpreted *pragmatically*.

(2) Quantum theory provides for a *complete* scientific account of atomic phenomena.

Point (1) asserts that quantum theory is *fundamentally* the procedure described in the practical account of quantum theory given in Sec. II. The central problem for the Copenhagen interpretation is to reconcile this assertion with the claim that it is complete, i.e., to reconcile assertions (1) and (2). This problem is discussed in Sec. VII.

The aim of the present section is to document point (1) by the words of Bohr. This fundamental point needs to be definitely settled, for critics often confuse the Copenhagen interpretation, which is basically pragmatic, with the diametrically opposed absolute-Ψ interpretation described in Sec. III. In what follows, particular attention will be paid to the possible conflict of the pragmatic viewpoint with (i) the element of realism in Bohr's attitude toward the macroscopic world and (ii) any commitment to a fundamental stochastic or statistical element in nature itself.

The quotations from Bohr that follow are taken from his three major works[4]: (I.) *Atomic Theory and the Description of Nature*; (II.) *Atomic Physics and Human Knowledge*; and (III.) *Essays 1958–1962 on Atomic Physics and Human Knowledge.*

The pragmatic orientation of the Copenhagen interpretation is fixed in the opening words of Bohr's first book: "The task of science is both to extend the range of our experience and reduce it to order ..." (I.1). "In physics ... our problem consists in the co-ordination of our experience of the external world ..." (I.1). "In our description of nature the purpose is not to disclose the real essence of phenomena but only to track down as far as possible relations between the multifold aspects of our experience" (I.18).

This commitment to a pragmatic view of science runs through all of Bohr's works. He later links it to the crucial problem of communication: "As the goal of science is to augment and order our experience every analysis of the conditions of human knowledge must rest on considerations of the character and scope of our means of communication" (II.88). "In this connection it is imperative to realize that in every account of physical experience one must describe both experimental conditions and observations by the same means of communication as one used in classical physics" (II.88). "The decisive point is to recognize that the description of the experimental arrangement and the recordings of observations must be given in plain language, suitably refined by the usual terminology. This is a simple logical demand, since by the word 'experiment' we can only mean a procedure regarding which we are able to communicate to others what we have done and what we have learnt" (III.3). "... we must recognize above all that, even when phenomena transcend the scope of classical physical theories, the account of the experimental arrangement and the recording of observations must be given in plain language, suitably supplemented by technical physical terminology. This is a clear logical demand, since the very word 'experiment' refers to a situation where we can tell others what we have done and what we have learned" (II.72).

Bohr's commitment to a pragmatic interpretation of the quantum-mechanical formalism is unambiguous: "...the appropriate physical interpretation of the symbolic quantum-mechanical formalism amounts only to predictions, of determinate or statistical character, pertaining to individual phenomena appearing under conditions defined by classical physical concepts" (II.64). "... the formalism does not allow pictorial representation on accustomed lines, but aims directly at establishing relations between observations obtained under well-defined conditions" (II.71). "The sole aim of (the quantum mechanical formalism) is the comprehension of observations obtained under experimental conditions described by simple physical concepts" (II.90). "Strictly speaking, the mathematical formalism of quantum mechanics and electrodynamics merely offers rules of calculation for the deduction of expectations about observations obtained under well-defined experimental conditions specified by classical physical concepts" (III.60).

Throughout Bohr's writings there is a tacit acceptance of the idea that the external world exists, and that our physical experiences are caused, in part, by the course of external events. This is quite in accord with pragmatism: James admits the existence of hypersensible realities. But there is no commitment by Bohr to the idea that the macroscopic world really is what we naively imagine it to be. The focus is on the *descriptions* of our physical experiences and the demand that they secure unambiguous communication and objectivity. Referring to the experimental arrangements and observations he says: "The description of atomic phenomena has in these respects a perfectly objective character, in the sense that no explicit reference is made to any individual observer and that therefore, with proper regard to relativistic exigencies, no ambiguity is involved in the communication of information. As regards all such points, the observation problem of quantum physics in no way differs from the classical physical approach" (III.3). Bohr's closest approach to a commitment to the idea that the macroscopic world actually is what it appears to be is, I think, the statement: "The renunciation of pictorial representation involves only the state of atomic objects, while the foundation of the description of the experimental conditions is fully retained" (II.90). The commitment here is, I believe, to the appropriateness, in quantum theory, of a classical *description* of the experimental conditions, rather than to the *fundamental* accuracy of classical ideas at the macroscopic level. This position is in complete accord with pragmatism.

In regard to the irreducible statistical element in quantum theory, Bohr was at first ambivalent. An initial acceptance of the notion of a fundamental element of randomness or indeterminism on the part of nature is suggested by the statement: "...we have been forced...to reckon with a free choice on the part of nature between various possibilities to which only probability interpretations can be applied" (I.4). However, he soon qualifies this idea (I.19) and later on says that at a Solvay conference

> an interesting discussion arose about how to speak of the appearance of phenomena for which only statistical predictions can be made. The question was whether, as to the occurrence of individual effects, we should adopt the terminology proposed by Dirac, that we were concerned with a choice on the part of "nature," or as suggested by Heisen-

> berg, we should say that we have to do with a choice on the part of the "observer" constructing the measuring instruments and reading their recording. Any such terminology would, however, appear dubious since, on the one hand, it is hardly reasonable to endow nature with volition in the ordinary sense, while on the other hand it is certainly not possible for the observer to influence the events which may appear under the conditions he has arranged. To my mind there is no other alternative than to admit in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena that we want to study (II.51).

Later he says "The circumstance that, in general, one and the same experimental arrangement may yield different recordings is sometimes picturesquely described as a 'choice of nature' between such possibilities. Needless to say, such a phrase implies no allusion to a personification of nature, but simply points to the impossibility of ascertaining on accustomed lines directives for the course of a closed indivisible phenomenon. Here, logical approach cannot go beyond the deduction of the relative probabilities for the appearance of the individual phenomena under given conditions" (II.73). "Corresponding to the fact that different individual quantum processes may take place in a given experimental arrangement these relations (between observations obtained under well-defined conditions) are of an inherently statistical character" (II.71). "The very fact that repetition of the same experiment, defined on the lines described, in general yields different recordings pertaining to the object, immediately implies that a comprehensive account of experience in this field must be expressed by statistical laws" (III.4). "The fact that in one and the same well-defined experimental arrangement we generally obtain recordings of different individual processes makes indispensible the recourse to a statistical account of quantum phenomena" (III.25). These statements indicate a turning away by Bohr from picturesque notions of a inherent random element in nature itself, and the adoption of an essentially pragmatic attitude toward the statistical character of the quantum-mechanical predictions.

It is worth noting that Bohr's notion of complementarity is altogether pragmatic: Ideas should be judged by their utility; physical ideas should be judged by their success in ordering physical experiences, not by the accuracy with which they can be believed to mirror the essence of external reality. The use of complementary ideas in complementary situations is a natural concomitant of pragmatic thinking.

VI. SPACE-TIME AND THE COMPLETENESS OF QUANTUM THEORY

In spite of doubts cast on our intuitive notions of space and time by the theory of relativity, the idea lingers on that persisting physical objects occupy space–time regions that can be divided into ever finer parts. A basic premise of classical physics is that this classical concept of the space–time continuum is the appropriate underlying concept for fundamental physical theory.

It is important to recognize that quantum theory has nothing in it that can be regarded as a description of qualities or properties of nature that are located at the point or infinitesimal regions of the space–time continuum. On one hand, the descriptions of the experimental arrangements and observations are basically operational descriptions of what technicians can see and do. They are not, strictly speaking, descriptions of the external things in themselves. Moreover, they are not descriptions of *microscopic* qualities or properties. On the other hand, the wave functions are merely abstract symbolic devices. They do not describe qualities or properties of nature that are located at points or infinitesimal regions of the space–time continuum. The abrupt change of a wave function in one region of space–time when a measurement is performed far away at the same time makes any such interpretation unreasonable. The wave functions of quantum theory are to be interpreted as symbolic devices that scientists use to make predictions about what they will observe under specified conditions. As Bohr says it: "In the treatment of atomic problems, actual calculations are most conveniently carried out with the help of a Schrödinger state function, from which the statistical laws governing observations attainable under specified conditions can be deduced by definite mathematical operations. It must be recognized, however, that we are here dealing with a purely symbolic procedure the unambiguous physical interpretation of which in the last resort requires a reference to the complete experimental arrangement" (III.5). "In fact, wave mechanics, just as the matrix theory, represents on this view a symbolic transcription of the problem of motion of classical mechanics adapted to the requirements of quantum theory and only to be interpreted by an explicit use of the quantum postulate" (I.75).

The fact that quantum theory contains nothing that is interpreted as a description of qualities located at points of an externally existing space–time continuum can be construed as evidence of its incompleteness. However, all we really know about the space–time continuum is that it is a concept that has been useful for organizing sense experience. Man's effort to comprehend the world in terms of the idea of an external reality inhering in a space–time continuum reached its culmination in classical field theory. That theory, though satisfactory in the domain of macroscopic phenomena, failed to provide a satisfactory account of the microscopic sources of the field. The bulk of Einstein's scientific life was spent in a frustrated effort to make these ideas work at the microscopic level.[20] The rejection of classical theory in favor of quantum theory represents, in essence, the rejection of the idea that external reality resides in, or inheres in, a space–time continuum. It signalizes the recognition that "space," like color, lies in the mind of the beholder.

If the classical concept of the space–time continuum were accepted, then quantum theory could not be considered complete, i.e., if it were accepted that the persisting objects of nature literally reside in a space–time continuum, with their various parts definitely located in specific regions, then a complete scientific account of atomic phenomena would, by virtue of the natural and normal meanings of these words, in this framework, be required to describe whatever it was that is located at the points or infinitesimal regions of that continuum. Quantum theory does not do this, and hence a claim of completeness would be an abuse of language.

In a pragmatic framework the claim of completeness has a different natural meaning. The natural meaning of the claim that quantum theory provides for a complete scientific account of atomic phenomena is that no theoretical construction can yield experimentally verifiable predictions about atomic phenomena that cannot be extracted from a quantum theoretical description. This is the practical or pragmatic meaning of scientific completeness in this context.

VII. WHOLENESS AND COMPLETENESS

The second essential ingredient of the Copenhagen interpretation is the claim that quantum theory provides for the complete scientific account of atomic phenomena. During the more than thirty years spanned by his three books[4] Bohr polished and refined his views on this point. His final, and I think best, summary is as follows:

> The element of wholeness, symbolized by the quantum of action and completely foreign to classical physical principles, has . . . the consequence that in the study of quantum processes any experimental inquiry implies an interaction between atomic object and the measuring tools which, although essential for the characterization of the phenomena, evades a separate account if the experiment is to serve its purpose of yielding unambiguous answers to our questions. It is indeed the recognition of this situation which makes recourse to a statistical mode of description imperative as regards to the expectations of the occurrence of individual quantum effects in one and the same experimental arrangement. (III.60).

This statement is augmented and clarified by an earlier statement:

> The essentially new feature in the analysis of quantum phenomena is . . . 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 to the quantum of action. On their side, the quantal features of the phenomena are revealed in the informa-

> tion about the atomic objects derived from the observations. While within the scope of classical physics the interaction between the object and apparatus can be neglected or, if necessary, compensated for, in quantum physics this interaction thus forms an inseparable part of the phenomena. Accordingly, the unambiguous account of proper quantum phenomena must, in principle, include a description of all relevant features of the experimental arrangement (III.3).

The basic point here is that well-defined objective specifications on the entire phenomenon are not restrictive enough to determine uniquely the course of the individual processes, yet no further breakdown is possible because of the inherent wholeness of the process symbolized by the quantum of action.

This way of tracing the need for a statistical account of atomic phenomena back to the element of wholeness symbolized by the quantum of action appears to take one outside the pragmatic framework since it refers to the measuring device, the atomic object, and their interaction. Also, it is not immediately clear how one is to reconcile the separate identification of these three things with the "impossibility of any sharp separation between the behaviour of the atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear" (II.39).

In this connection it is important to recognize that the "atomic object" and "measuring instruments" are, within the framework of quantum thinking, *idealizations* used by scientists to bring order into man's experience in the realm of atomic phenomena. This point is developed by the author in Ref. 21. Bohr's words emphasize that these separate idealizations are inseparably linked by quantum thinking in a way that is completely foreign to classical thinking. The idealization "the measuring instrument" is a conceptual entity used in the description of the experimental specifications; the idealization "the atomic object" is a conceptual entity that is represented by the wave function. These are inseparably linked in quantum theory by the fact that the specifications described in terms of the measuring instrument are mapped onto wave functions associated with the atomic object: The atomic object represented by the wave function has no meaning in quantum theory except via its link to experience formulated in terms of specifications that refer to the measuring instruments.

Bohr evidently believed that there is in atomic processes an element of *wholeness*—associated with the quantum of action, and completely foreign to classical physical principles—that curtails the utility of the classical idealizations of the measuring instruments and atomic objects as separate, interacting entities, and that the resulting inseparability of the atomic object from the whole phenomenon renders statistical description unavoidable.

This way of reconciling the pragmatic character of quantum theory with the claim of completeness seems rational and coherent. It is, of course, based on quantum thinking itself and is therefore essentially a self-consistency consideration. The validity of quantum-mechanical thinking as a whole must, of course, be judged on the basis of its success, which includes its coherence and self-consistency.

The question of the completeness of quantum theory was debated by Bohr[22] and Einstein.[23] Einstein's counter arguments come down on the following points: (1) It is not proven that the usual concept of reality is unworkable; (2) quantum theory does not make "intelligible" what is sensorily given; and (3) if there is a more complete thinkable description of nature, then the formulation of the universal laws should involve their use.

Bell's theorem[16] deals a shattering blow to Einstein's position. For it proves that the ordinary concept of reality is incompatible with the statistical predictions of quantum theory. These predictions Einstein was apparently willing to accept. Einstein's whole position rests squarely on the presumption that sense experience can be understood in terms of an idea of some external reality whose spatially separated parts are independent realities, in the sense that they depend on each other only via connections that respect space–time separation in the usual way: Instantaneous connections are excluded. But the existence of such a reality lying behind the world of observed phenomena is precisely what Bell's theorem proves to be impossible.

Einstein's second point, about whether quantum theory makes intelligible what is sensorily given, is taken up in the next section.

Einstein's third point raises two crucial questions. The first is whether a complete description of nature is thinkable. Can human ideas, which are probably limited by the structural form of human brains, and which are presumably geared to the problem of human survival, fully know or comprehend the ultimate essences? And even if they can, what is the role in nature of universal laws? Is all nature ruled by some closed set of mathematical formulas? This might be one possibility. Another, quite compatible with present knowledge, is that certain aspects of nature adhere to closed mathematical forms but that the fullness of nature transcends any such form.

VIII. QUANTUM THEORY AND OBJECTIVE REALITY

The Copenhagen interpretation is often criticized on the grounds that it is subjective, i.e., that it deals with the observer's knowledge of things, rather than those things themselves. This charge arises mainly from Heisenberg's frequent use of the words "knowledge" and "observer." Since quantum theory is fundamentally a procedure by which scientists make predictions, it is completely appropriate that it refer to the knowledge of the observer. For human observers play a vital role in setting up experiments and in noting their results.

Heisenberg's wording, interpreted in a superficial way, can be, and has been, the source of considerable confusion. It is therefore perhaps better to speak directly in terms of the concrete social realities, such as dispositions of instruments, etc., in terms of which the preparations, measurements, and results are described. This type of terminology was favored by Bohr, who used the phrase "classical concepts" to signify descriptions in terms of concrete social actualities.

On the other hand, Bohr's terminology, though blatantly objective, raises the question of how quantum theory can be consistently constructed on a foundation that includes concepts that are fundamentally incompatible with the quantum concepts.

Perhaps the most satisfactory term is "specifications." Specifications are what architects and builders, and mechanics and machinists, use to communicate to one another conditions on the concrete social realities or actualities that bind their lives together. It is hard to think of a theoretical concept that could have a more objective meaning. Specifications are described in technical jargon that is an extension of everyday language. This language may incorporate concepts from classical physics. But this fact in no way implies that these concepts are valid beyond the realm in which they are used by the technicians.

In order to objectify as far as possible our descriptions of the specifications on preparations and measurements we can express them in terms of the "objective" quantities of classical physics. The meaning of these "objective" quantities for us is tied to the fact that we conceive of them as the qualities of an external world that exists independently of our perceptions of it. The formulation of the specifications in terms of these classical quantities allows the human observer to be eliminated, superficially at least, from the quantum theoretical description of nature: The observer need not be explicitly introduced into the description of quantum theory because the connection between his knowledge and these classical quantities is then shifted to other domains of science, such as classical physics, biology, psychology, etc.

But this elimination of the observer is simply a semantic sleight of hand. Since the conceptual structure of classical physics is recognized as fundamentally an invention of the mind that is useful for organizing and codifying experience, the knowledge of the observer emerges, in the end, as the fundamental reality upon which the whole structure rests. The terms "knowledge of the observer," or "classical description," or "specifications" are just different ways of summing up in a single term this entire arrangement of ideas, which follows from the recognition of the limited domain of validity of classical concepts.

Bohr cites certain ideas from biology and psychology as other examples of concepts that work well in certain limited domains. And he notes that there have been repeated attempts to unify all human knowledge of the basis of one or another of these conceptual frameworks.[24] Such attempts are the natural outgrowth of the absolutist viewpoint, which holds that the ideas of man can grasp or know the absolute essences. The pragmatist, regarding human concepts as simply tools for the comprehension of experience, and averring that human ideas, being prisoners in the realm of human experience, can "know" nothing but other human ideas, would not be optimistic about the prospects of complete success in such ventures. For him progress in human understanding would more likely consist of the growth of a web of interwoven complementary understandings of various aspects of the fullness of nature.

Such a view, though withholding the promise for eventual complete illumination regarding the ultimate essence of nature, does offer the prospect that human inquiry can continue indefinitely to yield important new truths. And these can be final in the sense that they grasp or illuminate some aspect of nature as it is revealed to human experience. And the hope can persist that man will perceive ever more clearly, through his growing patchwork of complementary views, the general form of a pervading presence. But this pervading presence cannot be expected or required to be a resident of the three-dimensional space of naive intuition, or to be described fundamentally in terms of quantities associated with points of a four-dimensional space–time continuum.

APPENDIX A: PHILOSOPHICAL ADDENDA

Several questions of a philosophic nature have been raised by a critic. This appendix contains my replies.

Question 1: How does one reconcile the commitment of James and Bohr to the public character of sense objects with the radical empiricist doctrine that ideas can agree only with other ideas? Russell's *Analysis of Matter* indicates the difficulty in performing this reconciliation.

Reply: Russell's arguments do not confute the ideas of James and Bohr as I have described them. Both of the latter authors would, I think, readily admit that human experiences are probably not the whole of reality but are probably merely a part of the whole that is related to the rest via some sort of causal-type connection. The critical question, however, is not whether there is in fact a world "out there," but rather what the connection is between the world "out there" and our ideas about it.

Russell argues, essentially, that we can make plausible inferences, based on the structure of our experiences, and build up a reasonable idea of the outside world. James would insist that this whole structure is nothing but a structure of abstract ideas built upon our common experiences, and that the transexperiential world that may somehow "cause" our common experiences never enters into this structure at all.

James evidently believes that his idea of a table is similar to yours and mine. In general, different people's ideas about sense objects are not identical, but they are similar enough to form the basis of effective social communication. There exists, in this sense, a realm of public or shared experiences that form the basis of interpersonal communication. This realm constitutes the primary data of science. The aim of science is to construct a framework of ideas that will link these common, or public, or shared, experiences together in ways that reflect various aspects of the empirical connections that exist between them. Thus the whole structure of science is, quite obviously, a structure that is wholly confined to the world of ideas.

Russell would presumably grant this. But he would argue that we can, nonetheless, make plausible inferences about the world based on the structure of experience. Yet his commitment to rationality requires him, I think, to admit that our ideas *might* not be able to fully comprehend the realities that are the causes of our experience. And if the evidence of science indicates that this possibility is the one realized by nature, then I think his rational approach, based on plausible inferences drawn from available evidence, would require him to admit that this possibility has a good "probability" of being correct.

Although the arguments of Russell do not confute the position of James, as I have described it, there is definitely a basic difference in orientation. Russell embarks on a quest for *certainty* about the external world, but settles for an account to which he assigns high "probability." James views the quest for certainty about the external world as totally misdirected. Certainty in such matters is clearly unattainable. The truly rational course of action is to admit at the outset that our aim is to construct a framework of ideas that is useful for the organization of our experience—and for the conduct of our lives—and to put aside the whole vague question about the connection of ideas to nonideas, and the equally vague question about the "probability" that a certain scheme of ideas gives us a true or valid picture of the world itself.

In any case, the claim that we *can* make valid inferences about the world itself acquires credibility only to the extent that a truly adequate picture of the world itself can be constructed. No such picture exists at present. And the difficulties in constructing a scientific view of the world itself are precisely those admitted by Russell himself, namely the incorporation of quantum phenomena and infinitesimal space–time intervals. It is precisely these difficulties that force us to fall back to the position of James.

In short, the position of Bohr and James, as I have described it, is not a denial of the causal theory of perception. It is simply a recommendation that we view science not as a quest for a metaphysical understanding of that which lies outside the world of ideas, but rather as an invention of the human mind that man constructs for the purpose of incorporating into the world of human ideas abstract structural forms that capture certain aspects of the empirical structure of man's experience. In this undertaking an important class of data are those experiences that are common to different human observers, such as our common or shared experience of the table about which we all sit. The level of experience at which these common experiences are most similar is the level at which a round table is experienced as a round table, not as an oval two-dimensional visual pattern that depends upon where one sits, or a set of tactile sensations that depend on where one's hand rests. In science we need "objective" descriptions of the experienced world. We need descriptions that do not depend on who it is that has the experience. Operational specifications fill this need. They are descriptions of possible human experiences that do not refer specifically to any particular individual. They allow us to create a science that is thoroughly objective, yet securely rooted in the realm of ideas and experience.

Question 2: In the author's article[21] on the S-matrix interpretation of quantum theory it was admitted that the pragmatic interpretation of quantum theory leaves unanswered deep metaphysical questions about the nature of the world itself. And it was noted that the apparent absence of unanalyzable entities in quantum theory suggests a "web" structure of nature that somewhat resembles the structure proposed by Whitehead. Does the absence of similar remarks in the present work signify a retraction of the earlier views?

Reply: The aim of the present work is to describe the Copenhagen interpretation. More precisely, the aim is to describe this author's understanding of the essential common ground of Bohr and Heisenberg on the question of the interpretation of quantum theory. The author's own views are an elaboration upon his understanding of the Copenhagen interpretation, and are given in the S-matrix article.

APPENDIX B: CORRESPONDENCE WITH HEISENBERG AND ROSENFELD

The views that have been put forth as representations of the Copenhagen interpretation differ widely. Thus the question arises whether my description succeeds in capturing the essence of the Copenhagen interpretation as understood by Bohr and Heisenberg. To shed light on this question I inquired of Heisenberg whether the description given in a first version of this paper seemed to him basically in accord with the views of himself and Bohr, or whether it seemed different in any important way.

Heisenberg replied:

> Many thanks for your letter and for your paper on the Copenhagen interpretation. I think that your text is a basically adequate description of the Copenhagen interpretation, and you probably know that Niels Bohr was very interested in the ideas of William James. I would, however, like to mention one point where you seem to describe the Copenhagen interpretation too rigorously. On p. 35 you ask the question "Can any theoretical construction give us testable pre-

dictions about physical phenomena that cannot be extracted from a quantum theoretical description?" and you say that according to the Copenhagen interpretation no such construction is possible. I doubt whether this is correct with respect to, for example, biological questions. Logically it may be that the difference between the two statements: "The cell is alive" or "The cell is dead" cannot be replaced by a quantum theoretical statement about the state (certainly a mixture of many states) of the system. The Copenhagen interpretation is independent of the decision of this point. It only states that an addition of parameters in the sense of classical physics would be useless.

> Besides that it may be a point in the Copenhagen interpretation that its language has a certain degree of vagueness, and I doubt whether it can become clearer by trying to avoid this vagueness.

The paper was revised so as to make it absolutely clear that the claim of completeness of quantum theory refers specifically to atomic phenomena. Some superfluous material was eliminated, and the present Secs. V and VII, with their extensive quotations from Bohr, were added. Heisenberg's comments on the revised version were as follows:

> Many thanks for sending me the new version of your paper on the Copenhagen interpretation. It is certainly an improvement that you quote Bohr extensively, and your whole paper has become more compact and more understandable after these changes. There is one problem which I would like to mention, not in order to criticize the wording of your paper, but for inducing you to more investigation of this special point, which is however a very deep and old philosophical problem. When you speak about the ideas (especially in Chap. IV), you always speak about the human ideas, and the question arises, do these ideas "exist" outside of the human mind or only in the human mind? In other words: Have these ideas existed at the time when no human mind existed in the world?
>
> I am enclosing the English translation of a passage in one of my lectures in which I have tried to describe the philosophy of Plato with regard to this point. The English translation was done by an American philosopher who, as I think, uses the philosophical nomenclature correctly. Perhaps we could connect this Platonic idea with pragmatism by saying: It is "convenient" to consider the ideas as existing even outside of the human mind because otherwise it would be difficult to speak about the world before human minds have existed. But you see at these points we always get easily at the limitation of language, of concepts like "existing," "being," "idea," etc. I feel that you have still too much confidence in the language, but that you will probably find out yourself.

I replied:

> Regarding the question of nonhuman ideas it seems to me unlikely that human ideas could emerge from a universe devoid of idealike qualities. Thus I am inclined to the view that consciousness in some form must be a fundamental quality of the universe . . . . [However] It is difficult to extract from Bohr's writings any commitment on Platonic ideals. Indeed, Bohr seems to take pains to avoid all ontological commitment: He focuses rather on the question of how we as scientists are to cope with the limited validity of our classical intuitions.
>
> In view of Bohr's reluctance to speculate (in print at least) on the nature of the ultimate essences it has seemed to me that the consideration of these matters should not be considered a proper part of the Copenhagen interpretation. If the Copenhagen interpretation is considered to be an over-all world view that coincides with the complete world views of both you and Bohr, then there is danger that the Copenhagen interpretation may not exist; for it is not clear (from your respective writings at least) whether you and Bohr are in complete agreement on all ontological and metaphysical questions. Moreover, in your work *Physics and Philosophy* you discuss many of these

deeper philosophical questions, yet have a separate chapter entitled "The Copenhagen Interpretation." This suggests that "The Copenhagen Interpretation" should be interpreted in a restricted way. I have interpreted it to be not the complete over-all joint world view of Bohr and yourself, but rather the essential *common ground* of you and Bohr on the specific question of how quantum theory should be interpreted.

My practical or pragmatic account of quantum theory was based on the account given in your chapter "The Copenhagen Interpretation." This concrete account jibes completely with the abstract pronouncements of Bohr, as the quotations of Bohr in my Sec. V bear witness. Thus I think it correct and proper to regard the pragmatic interpretation of the formalism as an integral part of the Copenhagen interpretation. Similarly, I drew from our conversations at Munich an understanding of your commitment to the position that quantum theory provides for a complete description of atomic phenomena, and this position seems completely in accord with that of Bohr. Thus I think it correct and proper to regard also this position as an essential part of the Copenhagen interpretation. But in view of Bohr's silence on Platonic ideals I would hesitate to include considerations on that question in my account of the "logical essence of the Copenhagen interpretation." This is not meant to suggest that the Copenhagen interpretation bans further search for a comprehensive world view. It indicates only that the Copenhagen interpretation is, in my view, not itself a complete over-all world view: It is merely part of an over-all world view; the part that establishes the proper perspective on quantum theory. I emphasized in the closing passages of my paper that man's search for a comprehensive world view is not terminated by the Copenhagen interpretation. Rather it is significantly advanced.

Heisenberg replied:

Many thanks for your letter. May I just briefly answer the relevant questions. I agree completely with your view that the Copenhagen interpretation is not itself a complete over-all world view. It was never intended to be such a view. I also agree with you that Bohr and I have probably not looked upon the Platonic ideals in exactly the same way, and therefore there is no reason why you should go more into the problems of the Platonic ideals in your paper. Still there is one reservation which I have to make in connection with your paper and which I mentioned in my last letter. I think that you have too much confidence in the possibilities of language. I think that the attitude which is behind the Copenhagen interpretation is not compatible with the philosophy of Wittgenstein in the *Tractatus*. It may be compatible with the philosophy contained in the later papers of Wittgenstein. As you probably know, Bertrand Russell liked the *Tractatus* of Wittgenstein, but disapproved of the later papers and therefore I could never come to an agreement with Russell on these philosophical questions.

I replied:

Thank you for your very informative letter. I had not previously fully appreciated the point you were making, which as I now understand it, is this: You regard recognition of imperfectability of language to be an important element of the attitude that lies behind the Copenhagen interpretation. This point was not brought into my account of the Copenhagen interpretation, and is indeed somewhat at odds with its avowed aim of clarity. . . . [But] scientists must strive for clarity and shared understandings, since without striving even the attainable will not be achieved. . . .

Your words on the matters raised in our correspondence would certainly be extremely valuable to readers of my article. And any paraphrasing I might make would diminish this value. Thus, with your approval, I would like to include the full content of your letters (apart from personal openings and closings) in an appendix to my paper, along with certain connecting excerpts from my own letters. I have enclosed a copy of the proposed appendix, apart from your reply to the present letter.

Heisenberg replied: "Many thanks for your letter. I agree with your intention to publish my letters in the appendix to your paper."

I required also of Rosenfeld, as the close companion and co-worker of Bohr, and prime defender of his views, for an opinion of the extent to which my description succeeded in capturing the essence of the Copenhagen interpretation as it was understood by Bohr.

Rosenfeld expressed full agreement with my account, and gave hearty approval. He went on to comment on the relationship of Bohr to James. I include his remarks because of their historical interest:

> It may interest you to know that I several times endeavoured to persuade Bohr to make explicit mention of the affinity between his attitude and that of James, but he firmly refused to do so; not because he disagreed, but because he intensely disliked the idea of having a label stuck onto him. Indeed you may have noticed that some philosophers are already busy tracing imaginary influences of various philosophers upon Bohr. With regard to William James, I am quite sure that Bohr only heard of him from his friend, the psychologist Rubin, and from myself in the '30's. He then expressed enthusiastic approval of James' attitude, which he certainly felt akin to his own; but it is a fact—a very significant one, I think,—that James and Bohr developed a pragmatic epistemology independently of each other.
>
> It might be advisable to add somewhere in your paper a remark to that effect in order to avoid further misunderstanding. As a matter of fact, I have never myself in the papers I wrote on complementarity brought the pragmatic aspect of Bohr's thinking in explicit relation with James, precisely in order to avoid such misunderstanding.

He went on to say:

> I notice from your further letters with new title pages that you hesitate about the best title for your essay. I have no very strong view about this, but I would incline to prefer your March 31 title ["Quantum Theory, Pragmatism, and the Nature of Space–time"], the reason being that it does not contain the phrase "Copenhagen interpretation," which we in Copenhagen do not like at all. Indeed, this expression was invented, and is used by people wishing to suggest that there may be other interpretations of the Schrödinger equation, namely their own muddled ones. Moreover, as you yourself point out, the same people apply this designation to the wildest misrepresentations of the situation. Perhaps a way out of this semantic difficulty would be for you to say, after having pointed out what the difficulty is, that you make use of the phrase "Copenhagen interpretation" in the uniquely defined sense in which it is understood by all physicists who make a correct use of quantum mechanics. Surely, this is a pragmatic definition.

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[22] N. Bohr, Phys. Rev. **48**, 696 (1935) and in Ref. 20, p. 201.

[23] A. Einstein, in Ref. 20, p. 665.

[24] For an interesting and very readable account of the four principal conceptual structures that have been advanced as the basis of overall world views see S. C. Pepper, *World Hypothesis* (U. of California Press, Berkeley, Calif., 1966).

On the Doppler Effect

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Expressions for the Doppler effect are derived without employing any assumption about the phase of a plane wave. It is shown that the invariance of this phase under both the Galilean and the Lorentz transformations is a trivial consequence of the mathematics and is independent of its association with any physical process.

A derivation of the nonrelativistic Doppler effect as given by Møller[1] proceeds as follows:

It is asserted that the phase $kx-\omega t$ of a plane wave $\exp[i(kx-\omega t)]$ is invariant for two observers moving uniformly relative to each other.[2] This assertion is justified by associating the phase with the number of waves passing a point with coordinates (x, t) in the reference frame of the first observer in a time t, with the first wave crest to reach there being one which passes the origin at $t=0$, and then stating that this number will, of course, be equal to the number of waves passing over the corresponding point with coordinates (x', t') in the reference frame of the second observer in the time $t'=t$, the point (x', t') being such that it coincides with (x, t) at $t=t'$. The invariance of the phase is then used to derive the expression for the nonrelativistic Doppler effect. The same derivation is repeated for the relativistic case[3] with the exception that the Lorentz transformation is used in place of the Galilean transformation to connect the two frames.

Three objections can be raised to this derivation. First, the validity of the statement that the number of waves passing the point (x, t) in the unprimed system in a time t is equal to the number of waves passing over the corresponding point (x', t') in the primed system, up to the time $t'=t$, is not at all obvious. Second, while the association of phase with the physical process of counting may be useful, the invariance of the phase is completely independent of this physical association. Third, Møller's treatment