Chapter 2
Folder C130. Correspondence with Loinger, Rosenfeld, Schumacher and Bub. October–December 1966

Oct 3, 1966

Dear Jeff,

I was very glad to hear from you, and to learn that all is well. As for me, I am just getting back to work, with a number of new ideas.

I am having 25 reprints of your article sent to you.[1]

I have already written to Biederman, and he is very willing to see you, if you will visit him in Red Wing.[2]

Everyone here sends you and Mead[3] our best regards, also to your wife. Please let me hear what you are doing, and how you get on with Biederman.

Yours sincerely

David Bohm

This is my reply to a letter I received from Professor Loinger.[4] He also sent one to you here and this has been forwarded.

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[1] Presumably (Bohm and Bub 1966b)—CT.

[2] In a letter from Biederman to Bohm dated Dec 1, 1966 (C91b in the Birkbeck archives) he reports that Bub and his wife visited on Thanksgiving Day, Nov 24, 1966. Biederman and his wife enjoyed their visit. In relation to Bohm, Bub "has a great admiration for what you are searching to do in science, in spite of those who oppose your intentions"—CT.

[3] Alden Mead, was a physical chemist at the University of Minnesota. He spent a sabbatical at Birkbeck College during Bub's last year there as a graduate student and offered him a post-doc position in the Chemistry Department after graduation, which was his first job (information from Jeffrey Bub)—CT.

[4] Angelo Loinger was one of a trio of Italian physicists, Adriana Daneri, Angelo Loinger, and Giovanni Maria Prosperi referred to in these letters as DLP. Their main publications were Daneri et al. (1962) also at Wheeler and Zurek (1983), pp. 657–679 and Daneri et al. (1966). See Freire (2015), Chaps. 4 and 5 for more details. See Appendix A for Bub's correspondence with Loinger—CT.

C. Talbot (ed.), *David Bohm's Critique of Modern Physics*,
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12th October, 1966.

Professor A. Loinger,
Universita Degli Studi di Pavia,
Istituto di Fisica Teorica,
Via Taramelli 4,
Pavia,
Italy.

Dear Professor Loinger,

Thank you very much for your recent letter. I agree with you that refinements of the conventional formation by hidden parameters is an unlikely way of discovering a new theory along these lines. The only purpose of our article[5] was to clarify the question of what it would mean to have a hidden variable theory. I would be inclined to think that you are right in saying that a generalized field theory in the Einsteinian sense is the probably real alternative to the Copenhagen point of view, though it is my view also that this field should refer to some sort of discrete space time, rather than to a continuum. When I discussed the question with Einstein in 1951, he agreed that there was no intrinsic reason why the underlying reality should not be discrete. However, he said that he knew of no mathematics that could handle such a theory. I am at present working on such mathematics, and a very early version of this work is enclosed with this letter.[6] Naturally, it has gone a lot further since then.

About your own paper, I am not sure that you have actually succeeded in definitively settling the measurement question in the present theory, at least not to my satisfaction. You have made an important step in clarifying the role of the measuring apparatus. In this connection, I would like to suggest that you have carried to its logical point of completion a notion that was implicit in what I wrote in Chapter 22 of my book, Quantum Theory (Prentice-Hall, N.Y., 1951).[7] However, with regard to the status of the present physical theory, I feel that the significance of your work would be clarified, by pointing out that it leaves certain questions unanswered, which are reasonable to ask in any physical theory. Here, I want to stress not the questions that arise merely in implementing Bohr's philosophy, but rather, those that arise by careful inspection of the physical facts themselves.

Now, the essential point about a classical probability concept is that when new information is obtained, the domain of possibilities is always narrowed down. But in quantum mechanics, it may be narrowed down in certain ways and extended in others. Thus, if we have a state of spin $\frac{1}{2}$, with $\sigma_2 = 1$ then if we measure σ_2, it is impossible to obtain $\sigma_2 = -1$. But after you measure the X component of the spin, it becomes possible to have $\sigma_2 = -1$. So measurement did not merely give information about σ_2; it created new possibilities for σ_2. Thus, as everybody agrees, is something not contained in the classical concept of probability.

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[5] Bohm and Bub (1966a)—CT.

[6] Not in the Birkbeck archives, but see Bohm (1965), also http://www5.bbk.ac.uk/lib/archive/bohm/BOHMB.149.pdf—CT.

[7] Bohm (1989)—CT.

Now, you manage to deal with this question by considering the role of the observing apparatus. Generally speaking, after an interaction between a system I and its observing system II, but before anyone knows what the result of the interaction is, the wave function of I + II is

$$\Psi_{I+II} = \sum_i \varphi_i(y) c_i \psi_i(x) \tag{1}$$

where x is the coordinate of I and y of II, and where the initial wave function was

$$\Psi_{I+II} = \varphi_0(y) \sum_i c_i \psi_i(x) \tag{2}$$

Now, you show that because of the ergodicity of the apparatus system II, there is no interference between the terms of Eq. (1). Therefore, it is impossible by looking at system II to create new possibilities for I, because these latter depend on interference between the terms in Eq. (1). Therefore, the quantum mechanical rules for probability have now become more like classical rules.

Nevertheless, there is something left out of your account. This is that when a certain result is obtained in the measurement (e.g. $i = n$), then this result is reproducible. It is this behaviour which indicates the individual properties of a system, i.e., that certain properties of such a system are uniquely determined by the wave function. These include not only those operators for which the wave function is an eigenfunction, but also all properties that have probability unity. For example, one knows for certain that the electron will be found in the region in which its wave packet is appreciable, while its momentum will be found in the region where the fourier coefficient of this packet is appreciable.

This was all explained in our article, in connection with the diffraction experiment carried out with the cine camera, in which one electron entered the apparatus at a time. Each electron has its wave function, which is similar in shape to that of the others, but different in time of entering the system. Whether you say the wave function represents our knowledge or an objective reality beyond this, you must admit that when the wave function is given, then properties of this kind belong to the individual system, and are uniquely determined by the wave function. This is verified by the fact of reproducibility of a series of measurements of properties of this kind.

Now, as I take it, we both agree that the actual state of the system does not depend on our knowledge of the system. Therefore, after interaction of system I with the apparatus, II, but before anyone has looked at the apparatus, the wave function must be

$$\Psi_{I+II} = e^{i\phi} \varphi_n(y) \psi_n(x) \tag{3}$$

where ϕ is an arbitrary phase factor and n is the actual state of the system, which will be known later when someone looks at the apparatus.

Therefore, it is wrong to say that after interaction of I and II, but before anybody looks, the wave function is given by (1). If it were given by (1), then one would be forced to conclude that as soon as someone looked at the apparatus the wave function suddenly changed to III when he became conscious of the result of the measurement. For there is no doubt that if the result is to be reproducible, the wave function must be given by (3) after someone is conscious of the results of the measurement. So if we accept your claim that your treatment of the question is complete, we must either adopt the assumption that the system is influenced by our consciousness of it, or else we must say that both before and after an observer looks, the wave function is (1). But this latter assumption does not provide for the reproducibility of the measurement. Therefore, it is unacceptable. Since you also do not accept the influence of the observer's consciousness, you have not accounted for why the wave function is (3) and not (1), as soon as the interaction of I and II is complete, but even before anybody looks at the results of this interaction.

We see then that a simple inspection of the physical facts, along with an acceptance of the "realist" philosophy about the significance of measurements in physics, forces us to say that while the interaction between I and II is taking place, something more is happening than the destruction of interference between the terms of (1), because of ergodicity of the apparatus. This something more is that the wave function is not only undergoing a unitary transformation that turns it from (2) into (1). It is also undergoing a non unitary transformation that turns it from (1) into (3).

One is then immediately led to ask questions, such as: What is the equation of the process by which the wave function turns from (1) into (3)? Just when does this take place? (For until it does, the measurement is not complete.)

These are the sort of questions that our article shows how to answer, in terms of a theory that is admittedly not likely to prove to be on the correct lines. But we only wanted to make it clear what the nature of these questions is. Any theory of hidden variables would have to face similar questions, and I feel that your article would have been more clear and useful, if you could have brought this point out.

It may be that I have misunderstood your article. If so, please let me know in what way I am wrong.

With best regards, Yours sincerely,

D. Bohm.

Nov 15, 1966

Dear Jeff,

Thanks very much for your letter. I think your position is correct, as you will see from the enclosed copy of a letter to Loinger. By all means, write a detailed letter to him on the subject.[8]

[8] See Appendix A—CT.

I am glad to hear that you might get job in the philosophy dep't in Minnesota in Feigl's[9] dep't. I hope it comes through.

I shall be interested in hearing how your talk with Biederman goes.

It seems to me that the epistemological question raised by Bohr (see my letter to Loinger) is a crucial one. But his answer is on the wrong track. The key question is that of regarding physics as an extension of perception (as in the appendix to the Relativity book).[10] (But of course, it has little or nothing to do with Bertrand Russell). The question is: "What is really the immediate fact for physics? What does the physicist really perceive? Isn't classical physics largely a "descriptive fiction?" It is an exceptionally slippery question to discover just what the directly and immediately observed fact really is. Many philosophers have postulated fictions such as sense impressions. But these are as abstract as are the "atoms" that they are intended to replace. Somehow, man really perceives order, pattern, structure, potentiality along with actuality, totality and its partial aspects, etc., etc. Our mathematical terms are very far from these perceptions indeed. That is why physics seems to have degenerated into a purely formal one [of] practical manipulations of algorithms. Perhaps you can raise this question with Biederman, and ask him how art can help the physicist to be aware of how and what he actually perceives.

Best regards

D Bohm

Nov 24, 1966

Dear Jeff

I received your second letter. Meanwhile, I hope you received my letter to you, along with a copy of my answer to Loinger.

In my view, it is not possible to prove that Bohr's epistemology is compatible with "objective" probabilities in quantum theory, if only because the whole theory has been constructed in such a fashion that the interpretation of the "algorithms" depends essentially on what an observer is supposed to be able to communicate. As long as the equations are linear, you can't get out of the conclusion that the wave function spreads over a range of "quantum states", until another apparatus (III) observes (I+II). Any "objective" description would have to discuss the probability that the system I+II is in a certain state, S, (which we could call $P(S)$). But I+II is not in a state corresponding to eigenvalues of what is measured, unless the wave function is $\psi_i(I)\varphi_i(II)$. So there is no way to talk about the "objective" probabilities. We can only talk about probabilities of what III will find, when a further observation is made. I think that this is the essence of the argument against Loinger. No doubt,

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[9] Herbert Feigl, Austrian philosopher and member of the Vienna circle, was for many years Professor of Philosophy at Minnesota, establishing the Minnesota Center for Philosophy of Science in 1957—CT.

[10] Bohm (1996)—CT.

Rosenfeld[11] has confused both what DLP say and what Bohr says. It would be much better if he had said nothing at all.

Could you write to Loinger a letter[12] doing the following?:

(a) Explain where DLP go wrong.

(b) Explain where Rosenfeld confuses DLP.

(c) Explain where Rosenfeld confuses Bohr.

How did you get on with Biederman.

Best regards

D Bohm

10th November, 1966.

Professor A. Loinger,
Universita Degli Studi di Pavila,
Istituto de Fisica Teorica,
Via Taramel1i 4, Pavia,
Italy.

Dear Professor Loinger,

To some extent, we have been arguing at cross purposes and not meeting. I have always meant to say that you have made a contribution to the more nearly complete expression of Bohr's point of view. What I have been trying to emphasize is that I am not satisfied with this point of view, and that it has inherent inadequacies, which are not really changed by your work.

When I wrote my book, Quantum Theory, my main objective was to try to understand what the subject is all about, and in particular, what Bohr is actually saying. After writing it and thinking the whole question over, I began to feel that, after all, I did not understand. This was perhaps the main reason why I started then to inquire into hidden variables.

Since that time, I have had several opportunities to discuss with Niels Bohr and with his assistant, Aage Petersen.[13] Although we never brought up my own book in a direct way, I did gather (largely from Petersen) that Bohr did not really like my approach in this book, and felt that I had not understood what the point of complementarity actually is. I also gathered (though it was never stated explicitly) that Bohr and Rosenfeld did not really see eye to eye on these questions. Indeed, I

[11] Léon Rosenfeld, long associated with the Niels Bohr Institute in Copenhagen, was then professor at Nordita (Nordic Institute for Theoretical Physics). For more on Rosenfeld including his earlier conflicts with Bohm see Freire (2015), Kožnjak (2017). For a letter from Bub to Bohm on Rosenfeld's paper, see Appendix B—CT.

[12] See Appendix A—CT.

[13] Petersen was Bohr's assistant from 1952 to 1962. His 1966 doctoral thesis was published as Petersen (1968)—CT.

had long suspected that Rosenfeld's belief that Bohr is a natural born dialectician was, to a considerable extent, merely Rosenfeld's own interpretation. Later, when I received a manuscript from Petersen (about a year ago) on Bohr's philosophy, this suspicion was confirmed.

Now as I see it, Bohr has been giving top priority to the role of language. As Petersen says, Bohr was fond of emphasizing that "Man lives suspended in language". Therefore, to Bohr, the principle of complementarity is primarily a linguistic question. As Kant raised the question of what conditions (such as space, time, causality, etc.) are the preconditions of all experience, so Bohr asked what is the precondition of the precise communicabi1ity of descriptive information about nature. He came to the conclusion that the concepts of classical physics (position and momentum) are what determines this precondition. This conclusion is not primarily physical, but rather, epistemological. What it amounts to is that Bohr believes that the language of classical physics is in some way a part of the "human condition" (or perhaps one could say that it is an intrinsic aspect of human nature, and of man's interactions with his environment). Another closely related aspect of the "human condition" is the language of subject and object. That is, we always say that there is an observer who is looking at some object, that is being observed. As long as the interaction of the observer with what is observed is not subject to restrictions, then there is no limit to the precise communicability of information about momentum and position. But because of the quantum, subject and object can no longer be separated, and therefore precise information about the object is no longer possible.

Bohr then emphasizes that precise classical descriptions are to be replaced by the mathematical algorithm of quantum mechanics, which contains just the required limitation on precision of information about the object.

It can be seen that the essential assumption of Bohr is in the field of epistemology, and more particularly, in the field of linguistics. Bohr's belief that the language of separation of subject and object, along with the description of the object by classical concepts such as position and momentum, is inherent in the "human condition", means, in effect, that he puts certain aspects of the human mind (i.e., language) in the first place, in man's relationships with nature. This approach is not really compatible with the dialectical materialist point of view adopted by Rosenfeld.

I think that Rosenfeld does a disservice both to Bohr and to his own dialectical materialist views, by failing to recognize that Bohr is not a materialist (though he may perhaps be, in certain ways, a dialectician). Indeed, I learned in conversations with Petersen that in his early life, Bohr as strongly influenced by Kierkegaard. One can in fact see the influence of existentialism in the principle of complementarity. Man is somehow an individual and yet he must be in indivisible union with God. This unresolvable contradiction must cause man to be in a state of torment. For every time he tries to determine some aspect of his individuality, this comes into contradiction with another equally significant aspect. Somehow, man exists as a totality, but in his perceptions and actions, he can always only define one fragment of himself at the expense of another. Similarly, in physics, Bohr takes subject and object as being indissolubly united by the quantum that "connects" them. Yet, this unity can never be defined. When one aspect (position) is defined, this comes into contradiction with

another aspect (momentum). Our language, according to Bohr, forces us to assert the separation of subject and object, while we know that they are united (as it also by implication forces us to assert the separation of God and man, though all religious people have said that they know the two are inseparably united).

Now, while I think it is useful to raise all these questions, I do not believe that Bohr's answers are pertinent. There is no evidence that the classical language of position and momentum is part of the "human condition". Rather, it evidently evolved historically for specific reasons. Indeed, in a typical cloud chamber, we never observe positions and momenta. We observe the order of a series of droplets (approximated as on a curved arc) and then we translate these into positions and momenta. So I would like to emphasize that man's direct perceptions are not limited by the need to be expressed in classical concepts of position and momentum.

To be sure, mathematics today is not able to say much about order, pattern, structure, etc. But this deficiency can ultimately be remedied. I am publishing an article on the subject, which I will send to you when reprints are available. In essence, I have reason to believe that both mathematics and physics can take the concepts of order, pattern and structure as basic. When this is done, quantum mechanics will be seen to fit naturally into this new language. The dichotomy of subject and object is thus removed. For order, pattern, and structure refer both to the order in the mind that perceives them and to the order in what is perceived. Indeed, the whole concept of an object is then merely an abstraction from an over-all structure. To each abstraction of an object, there must correspond an abstraction of a subject, which determines the "perspective" from which the object is "observed". But the basic reality is the total structure, in which subject and object are both contained as abstractible aspects.

So much for the Bohr point of view. Now, I understand that you are trying to base quantum theory on objective laws applied both to the measuring apparatus and to the observed system. Of course, you realise that this approach is diametrically opposed to Bohr's philosophy. I am sure that Bohr would have regarded it as being on a wrong track altogether, just as he probably thought of the approach in my Quantum Theory book. For this reason, I think that Rosenfeld is confusing the issue, by falling to distinguish clearly between his own point of view and Bohr's basically epistemological approach. In Bohr's point of view, it is absolutely essential that the ultimate observing apparatus be treated purely in terms of classical concepts, and that no attempt be made to represent an "objective" system in terms of the quantum mechanical algorithms.

The way Bohr would interpret your work is as follows. It is permissible to treat "object" (system I) and "observing apparatus" (system II) as a combined system, I+II, which is handled in terms of the quantum mechanical algorithms. But then, this presupposes an observing apparatus, III, which is "observing" system II. Apparatus III must be treated in terms of classical concepts only. The usual probability laws of quantum mechanics apply then to I+II. This means, of course, that the "reduction postulate" applies to the wave function, I+II, when the system, I+II, is "observed" by III.

As I indicated at the beginning of this letter, your treatment could be interpreted by Bohr as an extension and completion of his own notions. In effect, your theory pro-

vides a detailed explication of how it comes about that the "cut" between "observer" and "observed" can be moved around freely in the large scale domain. In your case, this "cut" is placed between apparatus II and apparatus III, which latter is assumed to belong to the purely classical domain, concerning the state of which human language allows the precise communication of detailed information. But of course, it is, according to Bohr, entirely inadmissible to discuss the quantum mechanical side (I+II) as if no human being were present. For the whole meaning of the quantum mechanical algorithm is that it refers to what a human being can communicate in precise terms about system III. To treat system I+II as if it existed objectively without III (which is taken to be, in essence, an extension of the observer's sense organs) is to deny the deep meaning and basic spirit of the principle of complementarity.

You ask my opinion of Tausk's paper[14] In my view, it is essentially right, though I think that Tausk does not pay enough attention to the significance of the ergodic properties of large scale systems in making possible a consistent theory, along Bohr's lines, in which the "cut" can be moved freely in the large scale domain. Dr. Bub has written to me that he also has analyzed Tausk's paper and found it to be essentially correct. I understand that he will be sending you some detailed comments on this subject.[15] Meanwhile, I would only emphasize that it is not enough to get the right probabilities for the properties of system I. If your treatment is to be "objective", it must also show that after I and II have interacted, but before system III has "observed" system II, the wave function is definitely on one of the "channels" of II, corresponding to non interfering results of possible measurements. As I see it, Tausk's treatment shows clearly that you have only provided for the "weak" reduction postulate – i.e., that after I+II have interacted, the "probabilities" are the same as if the system II were "objectively" in a certain channel. But where do these "probabilities" come from and to what do they refer? Basically, they are brought in only by tacitly assuming system III, which will "observe" I+II, and which can obtain certain well defined results with corresponding probabilities. But once you have presupposed system III, you have also brought in the reduction postulate. It is this postulate which provides for the fact that the wave function II is in one of the channels. As Tausk points out, if you don't assume the reduction postulate, then after I+II have interacted, the wave function of I+II still spreads out over all the channels. As long as this is the case, there is no way to provide for the reproducibility of a measurement of II. For after you obtain a certain result for II in any one measurement, the wave function still spreads out over all the channels, so that in the next measurement, you will in general get some other result. It is only because you have tacitly assumed the operation of system III, which is in essence an extension of the observer, that we can say that after the observation of II by III, the reduction postulate implies that II is in a part of its Hilbert space corresponding to a single one of its channels, and not in a part corresponding to its being in many channels at the same time.

[14] Tausk's paper is not available. See Freire (2015), Chap. 5 and Pessoa et al. (2008). See also Appendix A—CT.

[15] Appendix A—CT.

In your letter, you refer to "one parameter distributions". These are, as far as I can see, equivalent to von Neumann's notion of an indecomposable distribution - i.e., one that cannot be divided into sub ensembles. Now, I do not think that such a distribution is a meaningful mathematical concept. The very notion of a distribution, as defined in all mathematical theories, is inseparable from the idea that the various members of a distribution can be distinguished from each other, so that each member is an individual. This has no essential relationship to the question of determinism. Indeed, even in Brownian motion theory, which is not deterministic, there is a distribution over possible trajectories. Each trajectory is distinguishable from the others, and this distinction is made possible by a suitable set of parameters which specify the orbit completely, (e.g., Wiener's use of differential space). When you talk of a "distribution" over elements that are not distinguishable in any way at all, it seems to me that this is only a set of words that has been put together, and not a meaningful concept.

Indeed, in reality, there is a "hidden" distinction in the ordinary quantum theoretical probabilities. To each "first" wave function, Ψ, and to each observable, $\mathcal{O}$, there corresponds a distribution of "second" wave functions, ψ_i, with probability $|a_i|^2$ (where $\Psi = \sum_i a_i \psi_i$). These "second" wave functions are those which result from the reduction postulate, which asserts that the wave function "collapses" to ψ_i (in your theory, when system III observes system II). And this "collapse" is not compatible with an "objective" over-all treatment, (since ultimately system III must be observed by IV, and so on until we reach a conscious observer). Thus, we are back in Bohr's philosophy, which asserts that the content of physics is limited by linguistic considerations, built into the "human condition" and referring to what can be communicated precisely by an observer.

As Dr. Bub and I point out in our paper, there is a further distinction between individual members of an ensemble, not usually taken into account in the concept of a "one parameter" distribution. This is that (as we showed with the aid of a hypothetical cine camera experiment), each individual electron comes into the system at a different time. So, in a way, the "indecomposable" distributions have already been "decomposed" even without the help of hidden variables. In this point alone, the current quantum theory is confused, as to how it deals with the relationship between individual and ensemble. In my view, this confusion along with the other unsatisfactory features that I have described in this letter, can be dealt with in terms of the concept of hidden variables. There may well be other and better ways to do so, and I am trying to look into some of them. But something must be done about the problem, which cannot be solved in the framework of the current theory, not even with the aid of the theory that you and your colleagues have developed.

With best regards,

Yours sincerely,

D. Bohm.

Dec 2, 1966

Dear Jeff

Enclosed is a copy of a letter to Loinger. I don't think we will get much further with him.

How are you doing!

Sincerely yours

D Bohm

2nd December, 1966.

Professor A. Loinger,
Universita Degli Studi di Pavia,
Istituto de Fisica Teorica,
Via Taramelli 4,
Pavia,
Italy.

Dear Professor Loinger,

Thank you very much for your letter of November 23rd.

(1) With regard to Bohr's philosophy, I should be very doubtful that he would have agreed to the notion that the quantum algorithm directly represents an "objective" property of nature, in the way you wish to propose. Rather, he would have said that objective and precisely describable (therefore communicable) properties must refer only to classical variables, such as position and momentum. The quantum algorithm is then a kind of "metalanguage" which makes statements about the "objective" properties appearing in the classical "language". But to attempt to describe objective properties of nature directly through the quantum algorithm is surely contrary to Bohr's philosophy.

(2) I do not think that Bohr's concept was completely coincident with that of Von Neumann. Von Neumann was an outstanding mathematician, but his physical concepts were often best describable as "primitive" in their naive character. As far as I can tell his concept was that of "psycho-physical parallelism". That is, there is something appearing in the mind, which is parallel to what also exists physically. Bohr was much more subtle than this. He said that all precisely communicable "objective" properties must be described in classical language. At this level, the concept of objective reality is therefore not basically different from that used classically. The quantum mechanical algorithm is nothing more than a "metalanguage" making statements about what happens to "classical" quantities, such as position and momentum. These "classical" quantities generally belong to the directly observable aspects of the measuring apparatus.

If you treat the system II in terms of the quantum mechanical algorithm, then Bohr would say that this algorithm refers to predictions of the behaviour of system III, which is used to "observe" II. I don't see how you can possibly get out of this, without giving up the very essence of Bohr's philosophy. (This point has been explained very clearly in a recent preprint by Aage Petersen, also by a man named Schumacher in Cornell University, who has been in correspondence with Rosenfeld.)

I am sure that Rosenfeld has, as you say, an extraordinary esprit de finesse. Nevertheless, I do not think that he has really understood the essence of Bohr's point of view in spite of having lived for a long time with Bohr. Bohr's ideas are very hard to grasp. Indeed, they are so unclear that I think it would be good if those of us who are interested in them (including you and Rosenfeld and myself, along with others) could some day get together for a full and informal discussion.

In von Neumann's point of view, it is never clear just what is meant by the term "making a measurement". Sometimes it seems that he refers to the registration of the event on the mind of an observer and at other times, he refers to its registration in what he calls "classical observables". Neither of these notions coincides with Bohr's position. Indeed, as Bohr has explicitly said to me, "Nothing is ever really measured at all in quantum theory". Rather one observes the state of the apparatus and describes this state in classical terms, including the whole set-up of the "experimental conditions". One then applies the algorithm of quantum mechanics, to make statements about what can or will be observable later in the apparatus. One can see that the experimental conditions needed to measure conjugate variables precisely are not compatible with each other. This incompatibility is expressed perfectly by the failure of the corresponding operators in the algorithm to commute. As a result, the statements that one can deduce from the algorithms about these sets of experimental conditions have exactly the right degree of ambiguity to match the ambiguity of the classically describable specification of the experimental conditions themselves. This is the meaning of the uncertainty relationships. Physical experiments have nothing really to do with measurements of "objective" conditions at the quantum mechanical level. Rather, they have to do with statements about the behaviour of classically describable aspects of the world (usually of a piece of laboratory apparatus). Therefore, your whole programme is directly contrary to what Bohr wants to do.

(3) You say that practically, a superposition of macroscopically distinguishable states is equivalent to a mixture. It is just this word "practically" that I cannot accept. If we were satisfied to regard quantum mechanics as nothing more than an elaborate formula for an engineer's handbook, then your reasoning would be adequate. But surely, Bohr and von Neumann were not satisfied to look at it in this way, and I doubt that you are either. Rather, there is the implication that quantum mechanics is a logically coherent structure of physical and mathematical ideas. Whenever, in a purely logical argument, you identify something with something else that merely approximates some of its qualities, you have a contradiction. It doesn't matter how "small" the error is, it is still a contradiction. For logically, there is no such thing as a "small contradiction". Either the theory is logically coherent, or it is self-contradictory.

No matter how "small" a contradiction is, there is a theorem in mathematics that from it, one can derive any statement whatsoever (this includes both true and false

statements). Of course, if you are careful to limit the domain of application of a contradictory theory, you can manage to get correct results in a limited field. For example, consider the contradictions, $X = 2X$, $Y = 2Y$. From these, you can derive correct results for any function of the ratio, X/Y, but not for functions such as $X + Y$. Similarly, your own contradictory theory will give correct results in a certain domain. If all that you want your theory to do is to state, in a different language what others have already stated more directly, then you can avoid coming into contact with the consequences of its contradictions. But if your wish is to apply your theory to the treatment of something that depends on the total logical structure of the theory, the latter cannot be relied upon.

What you have shown is that for "practical purposes", one can replace the pure ensemble of I+II by a mixed ensemble, which gives the same averages for system I as are obtained if one uses the usual quantum mechanical probability rules, which assume that II is "observed" by a classically described system, III. In other words, certain aspects of I+II can be treated, to an adequate practical degree of approximation by replacing the pure state of I+II by a suitable mixture. But logically, there is a gap in the argument. This gap is a contradiction. For it comes about because you identify something (a pure state) with something else that it is not (a mixed state). The "smallness" of the error in predicting the properties of I does not change the fact that this is a contradiction.

To avoid a contradiction, you must restrict yourself to stating the facts. These are that the probabilities for "finding" system I in a certain state are the same as if the ensemble for I+II were a mixture. But what is it that would "find" system I in such a state? Surely, it cannot be other than another system, III, IV, V, etc., which will function as a higher order observing apparatus. Without such a higher order observing apparatus, there is nothing that could "find" either I or I+II in a certain state. Therefore, the basic formulae from which you start would have no meaning. Moreover, it is these higher systems that guarantee the "reduction" of the wave packet and the reproducibility of the results, for system I. Without them, you never have anything else but a pure state for I+II, no matter what you do (unless, of course, you admit the logical contradiction, pure state = mixed state).

(4) Finally, let me say that von Neumann's systematic foundation of q.m. strikes me as being very excellent mathematics, but extremely naive and weak as an example of physical reasoning. As has been demonstrated clearly, his proof that there are no hidden variables is due to his not understanding the physical content of his mathematical assumptions. I also think he does not understand the physical meaning of probability distributions. He has a coherent set of mathematical equations. But his statements that these refer to "indecomposable distributions" are, in my view, only a set of words that has been put together, and not a meaningful concept.

With best regards,

Yours sincerely,

D. Bohm.

Dec 8, 1966

Dear Jeff,

Just received your letter of Dec 4. Meanwhile, I have already sent you another reply to Loinger.

I rather doubt we are going to get a lot further with DLP. I would only add that their point (3) is unclear (See Loinger's letter to you).[16] If the probability of "finding" is a probability in the sense of q.m., it means that it is the probability what apparatus III would find, if it looked at I + II. So unless you begin with apparatus III, the rest of what DLP [say] makes no sense. Apparatus III will always "reduce" the wave packet of I+II. The only contribution of DLP is to show that it doesn't matter whether you regard apparatus II as being on the classical or q.m. side of the "cut". This is also what I did in my quantum theory book, Chap 22. But Loinger does not accept this, nor do I think that he will ever do so.

Also, about Loinger's point (4), von Neumann's work is not clear, as is shown by his false proof of the impossibility of hidden variables. It is only a structure of mathematics. Its physical notions are often confused.

Their conclusion that practically a superposition of macroscopically distinguishable states is equivalent to a mixture is hardly a surprisingly new discovery. Many people have shown this, in different ways. DLP do it in yet another way.

Their point (7) is totally wrong. Without apparatus III, there is first of all no meaning to "finding" I+II in a certain state. And secondly, without apparatus III, I+II is still always in a pure state. It is, as I pointed out, a logical contradiction to equate it with a mixed state, however similar its "practical" consequences may be.

I shall be interested in hearing your further questions about Biederman and related topics. I do not like the word "caricature" either. The question is whether theories are a direct reflection of things as they are. In a way, they have to be just this. But in another way, they cannot be this. The interesting question is: "What, if anything, do theories reflect?" This leads to the question "What is knowledge, and its relationship to what is known?" I shall be sending you something on this soon.

Sincerely

David Bohm

Dec 10, 1966

Dear Jeff

I enclose several copies of my recent correspondence with Dr Schumacher, which will give you some idea of my notions on questions of epistemology. I will write you in more detail later.

[16] In Appendix A—CT.

Best regards

D Bohm

P.S. I would say that a theory is, in certain ways, like a work of art, created by scientists. Instead of reflecting particular objects or scenes, it reflects the general order, pattern, and structure of nature. New theories are like new works of art. Different painters on the same scene can do very different works. In a way, all may be true reflections. They are all related to each other in certain ways. Each is a better reflection in some ways, worse in others.

24th October, 1966.

Dr. D. L. Schumacher,
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Clark Hall,
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N.Y. 14850,
U.S.A.

Dear Dr. Schumacher,

Thank you very much for your two papers,[17] which I have read with great interest. I expect to get in touch with Peter Szekeres[18] in the near future, in order to discuss the questions further with him.

Let me say first of all, that insofar as I understand your position on complementarity, I do not fully agree with it. In particular, I would doubt the validity of identifying the term "quantum" with Bohr's particular epistemology. I would rather let it refer to the general body of fact and mathematical theory, which is underlying modern atomic physics. For in my view, the epistemological question is still very confused, and this confusion is seriously impeding the development of the other sides of physical theory. In this regard, see the articles which I enclose.

I do agree with you, however, that to understand physics as an extension of perception is the root of the matter. I think, in this regard, that an absolute separation of subject and object, is false. I am sorry to hear (in your footnote 11)[19] that you gath-

[17] Presumably one of these is Schumacher (1967). The other may be a preprint as there seems to be no other publication by Schumacher from that period—CT.

[18] Peter Szekeres, a respected mathematical physicist, now at the University of Adelaide was then presumably a colleague of Schumacher at Cornell. Szekeres did his doctorate at Kings College, London in the early 1960s so would be well known to Bohm—CT.

[19] Schumacher's note: "There is a useful review of recent pertinent work on perception in the Appendix, "Physics and Perception," in *The Special Theory of Relativity* by D. Bohm (New York: Benjamin, 1965). (Earlier edition of Bohm 1996—CT) It may be helpful to examine some of the general remarks and conclusions made there. The notion of a separation of subject and object is discussed in other terms, and while it is rightly emphasized in the context that this notion is itself

ered the opposite from my book, Relativity.[20] When I used the term "inner show", I wanted to distinguish the content of consciousness from a broader reality, which is reflected in this consciousness. My view is that consciousness is, in some way, embedded in a broader reality, of a nature that is basically unknown and indeed, even unknowable, if one considers its totality. There is no separation between this broader reality and consciousness. But the contents of this consciousness are evidently reflections of a reality that transcends the contents of consciousness, while containing the latter as part of itself.

The "inner show" is "inner" only insofar as the events on which it is based are taking place "inside the body" and mostly in the brain. But brain, body and the whole environment are one total process. The "inner show" can reflect not only the external environment, but also, its own contents. The test of the correctness of this reflection comes when we use it as a guide to function and action. This does not commit us to the view that reality is either external to man or internal to him. Rather, it means that whatever the world is we can function in a way that is directed by the contents of consciousness, and as a rule, our functioning produces more or less the expected trend of results.

This is the same as to say that perception is learning in action and action in learning. That is, we act (function) we learn how the results of function differ from that implied by the contents of consciousness, and these contents then change themselves, so as to remove the discrepancy. In other words, we do something, and see what happens, learning from our "mistakes". With regard to your ideas on the role of epistemology in science, I find them very interesting. Certainly, we must take into account the fact that knowledge is the result of a process of learning, in which man is inseparably interacting with the milieu in which he lives. However, I am not at all sure that Bohr's particular suggestions as to how to do this are right. Especially, I am dubious of his emphasis on the supreme role of language. Thus, Bohr has often said "Man lives suspended in language".

Of course, language does undeniably have a certain kind of importance. It is the major means of communication between men. But I don't think that limitations in what we have thus far been able to communicate by language are ultimate limitations on what we can perceive and learn. No doubt, we are in the habit of allowing our perceptions to be limited by what fits into our current linguistic structures. But if I may be permitted to say so, I would like to suggest that this is nothing more than a bad habit. Such a limitation is by no means intrinsic in man's perceptual relationships with

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learned together with the terms of a physical description which is not of an absolute character, the general conclusions given there tend to attribute absolute significance to the separation of subject and object. In particular, the reliance on the notions of a so-called "inner show," and of the entity to which it refers, essentially prejudges the separation of subject and object to be an absolute. (Cf. especially *ibid*., p. 204, par. 1, and pp. 216–217.) In this connection, it is instructive to appreciate that Bohr, in writing on these questions, rarely used terms of this kind; if they were used, they were clearly not intended to bear the full weight of the arguments. Even though Bohr apparently intended to give absolute significance to the separation of subject and object, he did not do so by means of particular terms which might be mistaken to establish the absoluteness to this notion."—CT.

[20] Bohm (1996)—CT.

his total milieu. On the contrary, as man can learn the structure of his environment by sensitive and careful perception, he can also learn in a similar way how he is being limited by language, and thus develop new ways of expressing the new content that is disclosed in perception.

As I see it, Bohr is emphasizing certain epistemological conditions of communicability of precisely defined information, which, as it were, come before all questions of the actual content of this information. In this sense, he is using an argument similar in some ways to that of Kant, who emphasized the conditions of space, time, causality, etc., that are the necessary forms of experience, as distinct from the particular content of this experience. I regard it as a valid line of study to raise these questions. But before one answers them, one must also ask a similar question: "What are the conditions of communicability of knowledge about epistemology itself?" Bohr seems to accept tacitly the notion that whereas we must learn the actual content of our knowledge of nature, we are given some kind of direct and completely reliable intuitions about epistemology, that are free of epistemological confusion. Thus when he says certain things about the relationship of subject and object, the "cut" between them, etc., etc., he seems to accept all this as a self evident truth, which will be eternally valid, and is not open to serious questions. To me, however, it seems that we are infinitely more confused about just these epistemological questions than we are about the content of our scientific knowledge. As you yourself have remarked, the unity of subject and object is inseparable. What can it mean then to think a "cut" between them? This "cut" is an absolute contradiction of their unity. Of course, Bohr argues that language forces us into this cut. But I do not accept this limitation as inherent or necessary. Probably, it is only a result of certain habits of thought, expressed in terms of certain linguistic structures that have been common over the past few thousand years.

Whatever the truth may be about this point, I feel that we have hardly scratched the surface of the question, and that Bohr has therefore prematurely claimed to settle the issue definitively with the principle of complementarity.

In addition, there is the further unclear question of what it can mean to have knowledge about epistemology. Epistemology is by definition supposed to deal with the general structure of knowledge and the means by which we attain it and express it. But when we come to learn about epistemology, what we learn in this way is part of the total content of knowledge. If there are inherent structural limitations in the applicability or definability of the content of knowledge (e.g., inherent in man's mode of communication through language), then such limitations very probably exist on the content of knowledge about epistemology. Therefore, one cannot be sure that limitations deduced from the content of knowledge about epistemology are genuine.

On the other hand, one may suppose that precisely with regard to epistemology itself, man may have completely reliable knowledge which is not limited by epistemological considerations (such as the need to express it in language). But then, this admits the principle that some kinds of knowledge are not subject to epistemological or linguistic limitations of any kind. If this is true of some kinds of knowledge, where is the "cut" to be drawn between this kind of knowledge and the other kind that is thus limited?

Does the "conscious subject" make a "cut" between "himself" and his knowledge about epistemology? Is he sure that what he seems to know about epistemology is not limited by the division of subject and object, in much the same way that Bohr says about knowledge of the electron? Or does a man transcend this division of subject and object only when he is communicating what he has learned about epistemology?

The consideration of these questions suggests to me that ultimately, all knowledge is one total field, and that divisions between the content of knowledge, and its overall structure as studied in epistemology, have only relative and limited domains of valid application. If the content of knowledge is assumed to be subject to limitations arising from limitations on what can be communicated in a clearly definable way that is free of epistemological confusion, then there will be a contradiction, if we also assume that these limitations are themselves similarly knowable and communicable. If they are not thus knowable or communicable, then for all we know, they may not even exist or they may be illusory and delusory, so that it is fruitless even to think about them. On the other hand, there is no contradiction in supposing that knowledge is the outcome of a process of learning, and that whatever we learn in this process, we can also learn to communicate. To place a-priori limitations on what can be learned is, in effect, to try to limit the process of learning in arbitrary way, and thus to impede perception. But epistemology has a valid and useful role to play, if it concerns itself with learning about how we do in fact learn, rather than with trying to set linguistically determined limitations on what we can learn.

I should like here to indicate some of my own ideas on epistemology. These are developed more fully in an article to be published soon in the Progress of Theoretical Physics.[21] When I have reprints, I shall send one to you.

I think the key point is in relation to concepts, such as order (and disorder), pattern, structure, individuality, totality, etc. These are evidently, as you point out in your article, really aspects of epistemology. But it is my view that they are also aspects of the content of our physical knowledge. However, because of the present inadequate development of mathematics, one has no language for the precise expression of the meaning of terms like order and disorder at the level of physical theory. So one is content to regard them as "undefinable" epistemological categories. However, what I am working on is to show that such terms can have a precise definition at the level of physical theory. (In the reprints, some notion of how this is to be done is given.)

Now, Bohr's tacit assumption is that the content of physical knowledge has to be expressed in terms of concepts like position and momentum. He seems to feel that this language is inherent, necessary and absolutely inescapable for anything that is to be precisely communicable in physics. Thus when we come to questions like individuality of a quantum and the disorder in statistical mechanical processes, he concludes that these have to be considered only at the next level of knowledge, i.e. as epistemologically determined categories determined by the nature of our language.

[21] I.e. Bohm (1965), also http://www5.bbk.ac.uk/lib/archive/bohm/BOHMB.149.pdf—CT.

On the other hand, I would say that concepts of order (disorder), pattern, structure, individuality, totality, etc., are universal. They apply in man's immediate perceptual experience and they apply in physics. The notion that in physics we measure only momentum and position is false. This notion is merely due to wrong generalization of the inadequate language of classical physics. In fact, in typical measurements (e.g., in a cloud chamber) we measure the order, pattern, and structure in a series of photographs of droplets. We translate these into momenta of particles, but I suggest that this procedure is inappropriate at the micro-level. Rather, we need a new theory in which order, pattern, structure, etc., are the basic terms of thinking (the basic language). So all the way through, from the micro-level to the macro-level to our own thoughts and perceptions, we are concerned with the same kind of thing – i.e., order, pattern, structure, etc. (The "disorder" in thermodynamic processes is itself merely a change of order, from one kind to another.)

Since the one set of concepts runs through the whole field of experience, the division of subject and object is transcended. The order in our physical theories is a particular reflection of the order in our perceptions and the order in the actual structure of the world, including man and his interactions with it. Our ability to know the order of anything (including ourselves) is based on the fact that thought can accommodate itself to a limitless set of possible orders. What is to be known is order, pattern, structure, etc., and this knowledge is itself a similar but different kind of order, pattern, and structure. So one denies the Cartesian dichotomy between "extended substance" and "thinking substance". Extension in space is ordered extension, and thought reflects this order. It is only the order that can be known. Whatever the "substance" of space (or anything else) may be, it cannot be known.

The essential point is to get away from the assumption that current "physical language" is all that the human brain is capable of. Actually, it is the result of a certain historical development. And the new discoveries in physics have, my view, demonstrated the need for a new language in which order can be defined relatively precisely. Perhaps we could discuss this point in more detail later.

It is also to be noted that all perception is an ordered interaction with the world. It is because these interactions are carried out in an appropriate order, related to what is already known, that we can learn something new, by discovering what is out of this order (and what therefore calls for a new order on thought and knowledge.) So the concept of order applies to man, his body, his mind, to the world as a whole, and to man's perceptual interaction, in which he learns the order of things by assimilating them into the order of his actions, as he observes with his senses or probes with his instruments.

Yours sincerely,

D.Bohm

23rd November, 1966.

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Dear Dr. Schumacher,

I would like to supplement yesterday's letter. From my discussion with Dr. Szekeres, I can see that our points of view have certain peculiar similarities and dissimilarities, in so complex a way that we could hope to understand each other only in a direct discussion. Failing this, it may help if I write an article on my own views. For I believe that my ideas are very different from the usual ones. It is not my aim, for example, to eliminate the concept of randomness from quantum mechanics, by introducing hidden variables. What I hope for is to present this concept in a more natural way. In my opinion, in quantum theory, randomness (like space, time, causality, etc.) is brought in a very confused manner. I have what I feel to be a quite new view on the meaning of natural law, in terms of various kinds of order. One extreme is the order in which two points on a trajectory determine the order of development of a whole orbit. This is complete determinism. The opposite extreme is that of a series of events whose actual order cannot be determined in terms of less than the total series in question. This is complete randomness. At present, it is tacitly assumed that these are the only two ways of thinking open to the human mind in physical problems. In my view, we can conceive of an order that lies between randomness and determinism. This is the order that quantum theory requires. But to explain this in detail would need a long article.

You say that Bohr is unclear but consistent. I question whether this is really true. In my view, when something is unclear, it means that either I have failed to understand it or that it is inherently confused. After a long study, I have come to believe that what Bohr says is inherently confused. However, he has developed a very clever way of hiding from himself and others the fact that his ideas are confused. That is to say, his words have consistent "ring". But when you look into what they mean, you discover that his ideas are contradictory. This contradiction is not presented with clarity. Rather, Bohr thinks in terms of a mechanism that breaks his concepts into disjoint fragments, such as wave and particle, subject and object, position and momentum, etc. One is directed to look at only one of these fragments at a time. Then, one is directed to "jump" to the opposite fragment, and to believe that "somehow the fragments meet, but in a way that one cannot precisely describe". By keeping one's concepts purposely ambiguous, one can fail to see that the fragments do not meet. This is the classical form of confused thinking. Usually, when we find ourselves caught in such confusion, we try to get out of it. But Bohr has turned confusion into a new principle, which he calls "complementarity".

Finally, I would 1ike to make a few remarks about your method of regarding the distinctions of subject and object as the basic theme of epistemology, adopted because you have not been able to make sense of the concept of reality. Firstly, let me suggest that the basic distinction is between reality and illusion, truth and falsity, fact and fantasy. As long as you are using the notion that basic concepts are undefinable. Why is one not free to take the above distinction as an undefined term? In my view, the words for basic notions like truth, fact, reality, etc. have mainly a denotative function. For example, if you and I are looking at the same tree, I can use words such as "Look at the color, the form, the structure of this tree !" I assume from the way you behave that you see more or less what I see. Similarity, truth, reality, fact, are aspects of the total functioning of the mind. By these words, I only intend to draw your attention to a vast dynamic movement, which is a fact. This movement cannot adequately be described, any more than words can really impart the color of a sunset to a man who has been blind from birth.

What I find missing from your views is an adequate attention to what is to be meant by the fact and by its distinction from what is not a fact. (If you don't like the word "reality", then I would instead emphasize the need to discuss and understand what is meant by "factuality".) I get the impression that you regard what people say as the essential fact of human existence. For example, you want to start by studying all that they have said about subject and object (or about randomness). In my view, this is not an adequate approach. Rather, it seems to me that what is necessary is to discover the real fact, the true fact, about subject and object (as well as about randomness). It is entirely possible, for example, that almost all that has been said about subject and object is entangled in hopeless confusion. Is it possible to see for oneself what is the fact about subject and object? Thus, eventually, what people say about the sunset and the tree is significant only to the extent that it indicates or denotes the deeper non-verbal fact of the sunset or the tree.

Now, I have been going rather deeply into inquiring into the fact about subject and object. My inquiries indicate to me that it is confused to regard these as fundamental categories of thinking. They do have a relative, limited significance in a certain rather superficial domain of experience. But I feel that to start, as you do, by trying to see all the rich meanings that people have attributed to these terms, will only entangle you in the general confusion that has been built up around these terms over thousands of years.

I would take consciousness (or preferably awareness) as a basic undefined term. When I talked to Szekeres, he agreed that this has to him a denotative significance. While I cannot explain the meaning of this word in terms of other words, it is a fact, both to me and to Szekeres, that this word denotes something, a dynamic process too vast to be described or thought about. I suspect that it has the same significance to you.

Awareness is neither subjective nor objective. Indeed, awareness of the subject and awareness of the object are in one and the same field. So awareness transcends the

distinction of subject and object. As Piaget[22] has shown, young infants very probably do not make this distinction. It is learned in the first few years of life. The first step is probably to learn the ordered distinction of "inside" and "outside". When the infant realizes that his body is one object among many, he begins to distinguish what is going on "inside the body" from what is going on "outside the body". But in fact, this distinction can be drawn only in the field of thought, which is based on certain orders, such as inside-outside, before-after, etc. The feelings in the solar plexus, the intestines, the chest, the head are sensed to be inside, and labelled as belonging to "I" or "me". It seems then that there is an entity inside the body, the "subject", who is "doing the observing" and is thereby looking at something outside the body called "the object". Later, the child learns to treat the body as yet another object. And still later, the child begins to treat the feelings and thoughts going on inside him as still another object, which he calls "me" or "the self". But then, it is tacitly assumed that somewhere still deeper in him is the "inner self" that is the "subject" who is supposed to be "doing the observing" of the "self" that is being "looked at".

Now in my inquiries into this question, I have observed that this is such a confused account of what is actually happening that it is best described as illusion and delusion. You seem to believe that merely to recognize the possibility of moving the distinction between subject and object freely will remove the confusion. But in my view, there is no way to make the concept of subject and object free of confusion.

If one begins with the totality of awareness (interpreted denotativity) it is an evident fact that all that one knows about anything, (whether it be subject or object) is in this field. So this is the place to begin the study of epistemology or the nature of knowledge. To begin here does not commit one to solipsism. For I do not assume that all that one knows is all that there is. But it surely cannot be denied that all that one knows is what is potentially or actually in the field of awareness.

The distinction of subject and object is never perceived directly (as one can perceive the tree). Rather, it is always inferred. Indeed, the subject is never really perceived at all. Various sensations are perceived, in the head, the solar plexus, the body, etc. These are called "I", "me", "mine", or the "subject". But in reality, anything that could be denoted in this way is not a subject. Rather, it is just another object, that happens to be inside the body rather than outside. This is a peculiar feature of the so-called distinction of subject and object. In fact, it always turns out that whatever the word "subject" a supposed to denote, this always turns out to be just another object. Ultimately, the word "subject" can never have any denotative significance at all. The distinction of subject and object is in reality always a distinction between one object and another object. Implicit in this distinction is an unperceived and unperceivable subject, who is looking at both the so-called "subject" and the so-called "object".

At this point, one may become suspicious of a word that has no denotative significance at all. Is it not possible that the word "I" corresponds largely to a pure fantasy? Of course, insofar as it refers to "this body", "this brain", etc, it has a denotative significance. But insofar as it refers to an "observing subject", it has no such signifi-

[22] Bohm summarizes some of Piaget's views in the Appendix to Bohm (1996). He gives references to Piaget (1953), Piaget and Inhelder (1956)—CT.

cance. I want to suggest that the latter meaning of the word "I" is an illusion. Perhaps it is not entirely a coincidence that almost all human misery stems from confusion about the meaning of the word "I". For while it is generally regarded as representing the most important thing in the universe, it may in fact represent nothing at all. Surely, a person who mistook a mere nothing for something that is all important must become confused about almost everything. And it may well be that this is the explanation of the confused state of what is called "human history" over the past few thousands of years.

Can one not suggest that the trouble here is basically verbal? Thus we say "It is raining"; mainly because every verb seems to demand a subject, who carries out the corresponding action. But no matter how much you look, you can never find the "it" that would be "doing the raining". Rather, a process of raining is "going on". Similarly, let me suggest that a process of "awareness" is going on, but that there is no entity called "I" who is "doing the awarenessing". In this process, awareness of what is called the "subject" is inseparably intermingled with awareness of what is called the "object". And in awareness, there is no "cut" between subject and object, whether one regards it as freely movable or not. Rather, there is one field of awareness. However, confused linguistic habits and confused social relationships that have been shaped by these habits cause one to give a fundamental transcendental significance to a rather superficial distinction, that is at best a mere convenience, and at worst, a gigantic source of general befuddlement of the brain.

Now one may perhaps say that no matter how far one goes in any such discussion as this, there is always firstly the distinction between the content of what is said (which is the object of thought) and the subject who is "doing the thinking". I want to suggest that this distinction is unreal, illusory, and not a fact. To understand what is happening here, it is necessary to draw a distinction between verbal reflective processes of the brain, which are called "thought", and some deeper perception that they are reflecting upon. For example, a verbal description of a tree is at best a reflection of what one actually perceives or could perceive, which is an infinitely richer experience than anything that words can evoke. I would suggest that thought is a reflective process, sensed as "going on" in awareness, along with a more direct kind of perception. It is as if one were looking at something directly and also seeing its reflection in one or more mirrors. Only the trouble is that the "mirrors" of thought do not always function properly. Very often, they are so disturbed that what is reflected in them does not correspond to any direct perceptions at all (whether potential or actual). It is evidently necessary that each person shall be alert and aware of the distinction between thoughts that are thus false, illusory or delusory and those that give true reflections of some directly perceivable fact. (Or do you say that this distinction has no relevance?)

Now, the word "subject" does not seem to reflect anything that could be thus perceived. Insofar as it is distinguishable from "object", it is just another object, one among many. Thus, to assert that the subject is distinct from all objects, and yet not an object, is to become entangled in confusion. It could only be distinguished from an object, to the extent that it was another such object. Even if the distinction is freely movable, this conclusion is not altered.

Let me suggest that the paradox arises because everything that words can possibly refer to must be contained potentially or actually in awareness. This is the only way in which words can mean anything at all. So if the word "subject" means something, whatever it means must be part of the contents of the contents of awareness. The moment one draws a distinction between one part of the contents of awareness and another, one has tacitly brought in the concept of an object (E.g., one distinguishes the object in the "foreground" from what is called the "general background", which is another object, on which attention is not centered.) So whatever the word "subject" refers to, it can only imply the disjunction of the contents of awareness into still another object.

Now, I admit that the concept of the distinction of objects (one of which is the body and brain of this human being, called by the name of "me") has a certain relative and limited domain of validity. But in the total field of human knowledge (which is what is appropriately studied in epistemology), it cannot be taken as a basic starting point without introducing hopeless confusion. Rather, I would say that insofar as there really is a "subject", this cannot be other than the totality of awareness. But this "subject" contains all objects. For the distinction of objects is basically the result of thought (though this thought may in some way reflect what is beyond the field of thought). So there can be no unconfused distinction between "subject" and "object". If the word "subject" has any meaning at all, it refers to the total movement of awareness.

So insofar as the "subject" is anything at all, it is the totality of its "objects". Rather than being distinct from its object, it is its object. This is not solipsism. Rather, it is a denial that the notion of "object" has fundamental significance (which necessarily denies that the distinction of subject and object has this kind of significance either). There is a reality that transcends the contents of awareness, but there is no meaning to committing oneself to the view that this reality is an "object", distinct from the awareness in which it can be perceived.

Perhaps I could make this point more clear by distinguishing between the substance of awareness and the contents of awareness. All knowledge is the contents of awareness. But in direct perceptual experience, there is a kind of control with something that transcends more knowledge. Here I use words denotatively rather than as descriptions or explanations. That is, I assume that you see and experience what I mean directly. If you do not, then there is no communication between us on this point. If you do, then I would go on to say that for all we know, the substance of awareness and the substance of the whole of reality (the universe or whatever you want to call it) may be basically one. But knowledge, (the contents of awareness) is particular and different for each person. It is rather like fire. Basically all fire is one kind of movement. But each individual flame is different, in its own way. So while awareness and existence are deeply one and the same, what we know about it is particular and different, for each human being.

This view is neither materialism nor idealism. For the distinction between matter and mind is like the distinction of subject and object, a category of thought. I suggest that reality, whatever it may be, transcends this distinction.

In many ways, we may be in agreement on these points. You wish to unite science, mathematics, art, and other things in one broader field is similar to my own. But I feel that the principal barrier to this is the confused notion of subject and object.

With best regards,

Yours sincerely,

D. Bohm.

P.S. It might be useful to answer a question raised by Szekeres: "When we make a scientific theory, do we not imply an observer, a subject, who is ultimately looking at the whole universe?" In my view, we do not. Each theory is a structure of thought, containing many complex, inter-related orders of its component parts and various aspects. There is "going on" an awareness of this thought, along with an awareness of a more directly perceived kind (e.g., the results of an experiment). If there is in thought a set of structures and orders similar to those in immediate perception, then this similarity (and difference from similarity) can also be observed. So the test of a good theory is that it contains structures similar to those observed which are ordered similarly to those found more directly in perception. Insofar as the two orders are different, the theory is said to be "falsified". So thought is a kind of "mirror" of the general order and structure of what can be perceived more directly.

Of course, similarity and difference are basically undefined terms. Indeed, an understanding of their meaning is presupposed in every language. For unless a man can perceive similarity and difference in words and their usage, he cannot even learn a language.

In this connection, I would like to indicate what it means to say that a theoretical idea is new. I would not say that this means that it is unanalyzable. Indeed, it is only by some kind of analysis that one can establish that the idea in question is in fact new. For example, suppose I assert that the word "glub" stands for a new concept, and that the basic quality of this concept is "blub", but that the words "glub" and "blub" are unanalyzable. I seems to me that such a procedure opens the door to any kind of mumbo jumbo.

I would prefer to stress that a new idea implies a new kind of order in our concepts, this order being understood mainly non-verbally (as one sees the shape, form and color of the tree). If the order is really new, it is not reducible to an already known order. Far from implying that the concept is unanalyzable, this means that it is just by means of suitable analysis that one can demonstrate the novelty of this order. For example, the geometry of Euclidean space implies a certain (infinitely rich) set of related orders of points, patterns, structures, and objects that can be conceived as being in this space. Non-Euclidean geometry implies a new set of orders that cannot be reduced to those of Euclidean space. It is by analyzing the geometry with the aid of concepts such as curvature (the result of displacing a vector parallel to itself around a circuit) that one can see that the order of non-Euclidean geometry is really new, because it is not reducible to that of Euclidean geometry. One does not do as Bohr does, by asserting that the novelty of non-Euclidean geometry is nothing more than

the imposition of a non-Euclidean "metalanguage" on the content of the Euclidean language. It can be thought of in this way, to be sure. But more fundamentally, it contains a basically new order. And by analysis, we can see that the old Euclidean order is a special case of the new non-Euclidean order.

You emphasize the concept of randomness. I would rather emphasize the concept of a creative process. This is first of all a process containing orders that are not completely reducible to any order that is known and specifiable. And secondly, it is a process in which the order of breaks in a lower order is the foundation of the next (new) higher order. What is commonly called randomness is a particular, limited, and very specialized manifestation of the creative character of universal order.

Consider for example, a series of coin throws. Unless it could have say 100 heads in a row, it would not be totally random. So first of all, a random sequence in a given field has the potentiality of all possible orders in that field. Secondly, no matter how many heads in a row one obtains, he cannot say anything at all about the next throw. This means that in a random sequence, the order of $N + 1$ terms cannot be determined by that of N terms or less. That is what it means to say that a random sequence is irreducible. None of this means that a random sequence is unanalyzable. Quite the opposite, it is only by means of various analyses that one can see whether a sequence is random or not.

So I cannot accept your idea that novelty has absolutely nothing whatsoever to do with analysis. A genuinely new order will, in my view, manifest itself both analytically and synthetically. I do not believe that analysis can exhaustively reduce a new order to an older kind of order. But analysis is able to demonstrate that a new order is in fact different from an older one.

DEC 14th 1966

[Date added – CT.]

Dear Jeff

You may find this correspondence with Rosenfeld[23] interesting.

Best regards

D Bohm

NORDITA NORDISK INSTITUT FOR TEORETISK ATOMFYSIK
Danmark . Finland . Island . Norge . Sverige

BLEGDAMSVEJ 17 . KOBENHAVN O . DANMARK

[23] On Rosenfeld see p. 22, n 11—CT.

6th December 1966

Professor D. Bohm
Department of Physics
Birkbeck College
Malet Street
London, W.C.1

Dear Bohm,

Loinger has showed me your letter to him of 10th November, containing startling considerations over Niels Bohr's philosophy. I have shown the letter to Aage Bohr,[24] who was as startled as myself. We do not, of course, take very seriously this game of putting labels with various "isms" upon Bohr. No more seriously, in fact, than he himself took it. In the hope of being helpful, however, I feel I ought to write to you about the facts of the case, because I suspect that you have been badly misinformed by Petersen,[25] or that you have perhaps misunderstood what Petersen told you. Moreover, Kierkegaard himself has been distorted out of recognition by the modern metaphysicians who got hold of him.

Kierkegaard appears on the very provincial scene of Danish theology as the advocate of an extreme irrational subjectivism and it is a strange thought, to say the least, to seek any relation between such a position and the objective rationalism to which Bohr consistently adhered and of which his writings, correctly understood, give such shining examples. In particular, the idea of mixing up God with Bohr's thinking appears most incongruous to those who have known Bohr intimately and who remember his familiar saying: "God is just a word of three letters". With regard to the alleged influence of Kierkegaard on Bohr, the truth of the matter is the following. Bohr did read some, at least, of Kierkegaard's books and rightly admired his wonderful stylistic virtuosity. But whenever he commented on this, he added that it was a pity that such a literary talent should have been wasted on such fatuous speculations. The actual philosophical influences that Bohr underwent in his youth are, on the one hand, that of Høffding who was a friend of his father's, and that of Georg Brandes, who was then the dominant personality on the Danish scene and especially enjoyed a great authority in liberal Jewish circles. Now Høffding, who has written a whole book about Kierkegaard, while paying due respect to his personality (he calls him a "Socratic personality") sharply rejects the contents of his philosophy. As to Georg Brandes, he actually started his literary career with a polemic contribution to a theological squabble about "Faith and Knowledge" which agitated people at the time, and in which he took sides against the philosophy professor Rasmus Nielsen, who defended an anti-rationalist attitude in the trend of Kierkegaard's. These are easily ascertainable facts, which could have prevented you from being led astray in your earnest efforts to understand Bohr's point of view.

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[24] Bohr's son. Awarded the Nobel Prize in Physics in 1975 for his work detailing the structure of the atomic nucleus—CT.

[25] See p. 22, n 13—CT.

I have the impression that you are looking for non-existing complications. Bohr's attitude was extremely simple-minded and dominated by a straightforward, commonsense approach to the problems. He was quite averse to metaphysical subtleties. He used to dismiss these as "trivial", by pointing out that the true epistemological problems which confront the physicist cannot be dissociated from the most refined analysis of the natural phenomena, of which the philosophers, who loosely talk about "matter" and "mind", have not the slightest understanding.

Incidentally, I am afraid that you have also misunderstood what I have said about the relation of Bohr to materialism. I certainly have never played the game of putting a materialist label on him. I leave that to the high-priests of Marxist-Leninist theology. All I wanted to suggest is that Bohr's thinking, as well as Einstein's thinking in his creative period, occupies a natural place in a dialectic development initiated by the materialistic philosophy of the French school of physics and chemistry (Lavoisier and Laplace) at the end of the 18th century.

With best wishes

Yours sincerely,

L. Rosenfeld

13th December, 1966.

Professor L. Rosenfeld,
Nardita,
Blegdamsvej 17,
Copenhagen,
Denmark

Dear Rosenfeld,

I thought it would be helpful to add a bit to my answer to your letter.

Firstly, let me say that I am not trying to minimize the significance of Bohr's work, when I say he was influenced by existentialism. As a matter of fact, I remember Petersen telling me that Bohr had "gone overboard" on Kierkegaard when he was quite young, but that his brother had got him to reconsider his views on the subject. Of course, you are right to point out that later Bohr realized Kierkegaard's deficiencies very keenly. However, a youthful experience of this kind generally "leaves its mark" in the form of a profound influence on a person's thinking, which is generally unconscious and tacit. I feel that I detect this influence in the principle of complementarity, though, of course, I may well be entirely wrong.

As I see it, the essence of existentialism, both early and modern, is to emphasize man's existence as the primary thing, having top priority. That is, one must begin with the fact of existence, and then one must, in some way, discover or choose what one is. This creates a very serious problem. For when man searches his own nature to discover what he is, he finds that his nature is ambiguous, with many contrary

and even contradictory potentialities. However, man cannot remain in a state in which all is potential. Some one of these potentialities must be realized at the expense of others. As he defines himself in one way, he loses the possibility of defining himself in other ways. In a way, this is similar to Bohr's view of the electron. The conditions needed to define its momentum (or wave-like potentialities) are incompatible with those needed to define the position (or particle-like potentialities). Of course, for the electron, the situation is determined by external conditions (e.g., the laboratory apparatus). For most men, it is also external conditions that determine which of his potentialities are realized. But for those who are philosophically inclined, this is not satisfactory. They may feel the need to make the choice, either on moral or religious grounds, or on grounds of inner necessity. The trouble is that no-one can ever find in himself sufficient grounds to determine this choice. So reflection on this subject is likely to produce a disturbing sense of "nausea" or "sickness unto death".

I cannot agree with your notion that existentialism is utter nonsense. In a way, it represents an important facet of what some existentialists call "the human condition". But it is, of course, one sided. So to go too far with it can lead to confusion.

It was only my intention to suggest that existentialist notions of this nature played a key role (perhaps largely unconscious) in shaping Niels Bohr's ideas on complementarity.

Secondly, let me repeat that Niels Bohr's ideas are very far from "common sense". This is in no way a derogatory comment on these ideas. In fact, Bohr raised profound questions, such as the relationship of subject and object, and the role of language in shaping our approach to external nature. Nowhere will you find this sort of thing being done by the "ordinary man" in his everyday routine of activities. None of the great scientists or philosophers was content with "common sense". Indeed, it was just because Bohr raised such deep questions that I was initially attracted to his ideas.

For about ten years, I studied Bohr's notions very earnestly. In this study, I found his style of expression a tremendous barrier. As someone once said to me "Bohr's statements are designed to cancel out in the first approximation with regard to their meaning, and in the second approximation with regard to their connotation. It is only in the third approximation that one can hope to find what Bohr wants to say". His style of talking was even more difficult than his style of writing. Sometimes it seemed to me that he was obsessed with the question of communication, just because he found it so difficult to communicate.

I wrote my book Quantum Theory[26] in the effort to understand the subject. But when I had finished, I saw that I did not understand it. Indeed, whatever in it that was clear was the result of an effort to think of the electron in a unitary way, and therefore profoundly opposed to Bohr's whole philosophy. The more I considered the subject, the more I saw that quantum theory was involved in great obscurity and confusion. Considered as an elaborate mathematical algorithm for computing experimental results, it was clear enough. But as a coherent logical structure, it is very deficient. For example, after I wrote my Relativity book,[27] I considered the

[26] Bohm (1989)—CT.

[27] Bohm (1996)—CT.

notion of trying to write a similar book on Quantum Theory. But I soon saw that I simply do not understand the latter topic well enough to do this, and that nowhere in the literature is there any basis for even beginning to clear up the confusion that seems to hover around the subject.

Recently, in a preprint by Peterson, I obtained new insights into Bohr's position. These insights were further extended by correspondence with Schumacher (in Cornell University). But what I learned has only served to convince me that Bohr's notions are not (at least for me) an acceptable answer to the very profound and valid questions that he was original enough to raise seriously, perhaps for the first time, in physics.

As I said in the previous letter, Schumacher's style of expression is similar to Bohr's, so that I find it difficult to understand just what he wants to say. Nevertheless, I do gather from him (as well as from Peterson) that Bohr emphasizes the question of language, with an attitude exemplified by his statement: "Man lives suspended in language". Bohr says (and Heisenberg agrees with him) that all precisely definable and therefore communicable concepts are, in effect, only variations in the classical concepts of position and momentum (space-time and causal descriptions). As long as these two concepts are simultaneously applicable, we can then obtain an objective description of phenomena, enabling us to communicate to each other what we know about them. But because of the quantum, this possibility breaks down in a very accurate treatment. Thus, we are left with an inherent ambiguity in the sharpness with which we can think about the nature of things. Within this ambiguity, experimental conditions determine which aspect (position or momentum) is to be sharply defined. The quantum mechanical algorithm is then a kind of "metalanguage" that makes statements concerning terms appearing in the primary (classical) "language", which latter is used for describing phenomena. Thus, the quantum mechanical algorithm does not directly reflect nature. Rather, it directly reflects the "language", with which we communicate what we know about phenomena.

To me, it seems that Bohr's views are a long step toward idealism. Of course, he recognizes the objective existence of the material world. But he seems to suppose that in man's dealing with the material world, he is limited by certain linguistic and conceptual structures, which are regarded (at least tacitly) as eternal and unchangeable features of the "human condition". So the form of man's perceptual contact with the world is limited permanently by his way of thinking and talking.

Now, in my view, there is nothing inevitable about classical concepts. They are merely the result of a (dialectical) historical development, so that they can change radically. As I indicated in my Relativity book, man learns to perceive the world, as he interacts with it, while being sensitive to the relationship between what he does and what he sees. Bohr also recognizes this, in a way. But he seems to say that man's direct perceptions will always have to be in the form of classical concepts (at least those that he will be able to communicate in a precisely definable way). It is only in the "meta-language" that man can think in new ways, while in the "language" that describes man's direct perceptual contact with phenomena he will never do other than think in classical terms.

In my view, even man's direct perceptual contacts with phenomena need not be in terms of classical concepts. An extreme case of this fact is the richness of response to the beauty of nature or a work of art. Of course, you may argue that in physics, this is irrelevant. But I do not think so. For example, when we look at a cloud chamber photograph, we do not really observe a particle with position and momentum. We actually observe a series of dots in a certain order (arranged roughly along a circular arc). The "particle" is projected into the photographs by our conditioning to classical ideas.

In my view, if we had a new kind of mathematics of discrete structural process, we would not have to think even of classical physics in this way. I have in fact been developing a new formulation of classical physics not based on the concepts of position and momentum, but rather, on those of distinction, relationship, order, pattern, and structure. In a way, what is needed is a more "artistic" conception of large-scale physics, one that is less technical and mechanical than the current one. We also need a new mathematics that allows us to express this new physical conception in precise terms. I have reason to believe that this can be developed. In a paper that I am preparing, these points will be explained in more detail.

Be that as it may, the key point is that both classical and quantum concepts are in need of fundamental changes. When this is done, the quantum algorithm will be able to refer directly to the phenomena of physics. But these phenomena will not be positions and momenta. Rather, they will be distinctions, relationships, orders, patterns, structures, rhythms, etc. etc.

So I do not wish to return to classical physics. My work on hidden variables was intended mainly to clarify the situation, which had been left rather confused by von Neumann's "proof" of the impossibility of hidden variables. I think that to entertain the notion of hidden variables calls attention to certain questions, which are necessarily overlooked in Bohr's point of view. But I am as dubious as anyone else that they are the right approach for a new theory.

I hope that this makes my position more clear than it was. I think it too much to hope that we will ever agree. But perhaps we can at least understand what it is that we disagree about.

Finally, will you please give my best regards to Aage Bohr, and assure him that I had the very highest esteem for his father. If I am deeply moved by the questions that moved Niels Bohr, this may cause me to be a bit sharp with his views at times. But I am sure he would have preferred such a response to the indifference with which physicists now so generally respond to these questions.

If you wish, you may show this letter to Aage Bohr.

With my best wishes,

D. Bohm.

16th December, 1966.

Dr. J. Bub,
Department of Chemistry,
University of Minnesota,
Minneapolis,
Minn. 55455,
U.S.A.

Dear Jeff,

This letter is intended to supplement the material I just sent you in the copies of letters to Schumacher, in Cornell University.

As I indicated in the material referred to above, one can regard a scientific theory as a certain kind of "artistic" creation, in the universe of discourse. Each theory is a particular reflection of nature, as perceived more directly, either with the senses, or with the senses aided by instruments. Now, to reflect is to abstract (in the sense used by Korzybski[28]). Thus, a mirror image abstracts the form, colour, and space relationships of an object, apart from its tactile qualities, its inertial qualities, its chemical and general physical properties, etc. In our own minds, the optical image abstracts from the total perceptual content of something, its "feeling", its tactile qualities, its functional properties, etc. etc. As Piaget shows, the infant tends to regard seeing, hearing, touching, as distinct experiences. Later, he learns to see what he hears, to feel what he sees, etc, so that it is all integrated. But one then overlooks that the unity of perception is based on various sensual abstractions.

Our descriptions go on to abstract from direct perception, and our inferences abstract from descriptions. We can also abstract further, to first describe our inferences and then form higher level inferences about inferences. Thus, we rise to the level of theory, which can go on to indefinitely high orders of abstraction.

The test of a theory is what Piaget calls the "circular reflex" – i.e., an action aimed not mainly at a specific function, but rather at learning. That is, guided by our theoretical reflective abstractions, we draw inferences about lower level aspects. We then do something to test these inferences and see what happens. If this latter is in accordance with our inferences, the theory is confirmed. Otherwise, it is falsified. Thus, we determine the domain of validity of a theory.

In certain ways, scientific theories are similar to what Biederman calls mimetic art. That is, they are designed to abstract from and reflect certain aspects of the perceived world. But the purpose of the reflection is different. In mimetic art, one tries to reflect on some concrete, individual perception, revealing the universal in the individual (e.g., Rembrandt). In science, one tries to reflect the general order and structure of things. The "test" for mimetic art is largely in its effect on the psyche, which is very subtle, but nonetheless, real. The test for science is through our functioning in nature (partly with the aid of instruments).

[28] Bohm took up ideas from Korzybski (1994), in which he was influenced by Charles Biederman. See Introduction, p. 10, n 59—CT.

What about the relationship of theories? This is also similar to that of successive mimetic artists. In a way, each artist must be keenly aware of how preceding artists perceived nature. Of course, he sees it in his own way. But as long as art was aimed largely at mimesis, there was a general trend of evolution toward mastering the problem of realistic presentation. Each (great) artist learned something new. He may have lost certain things that earlier artists had. But the general trend is that the later artists not only portrayed what earlier artists had not been able to do. They also could see what it was that previous artists had missed.

But, of course, the more and more complete portrayal of what the eye sees is a limited field of development. So artists had eventually to transcend this whole field. In a way, science is not thus limited, as nature is so vast and immense, so unbounded in all its manifold orders and structures, as revealed to the unlimited development of scientific instruments. On the other hand, this programme is also limited, in a certain other way. For even the indefinite probing into nature's structure raises the question of the kind of reflection of nature that man is creating. It is in no sense a movement toward complete and detailed reflection, nor is it even a steady approach to this as an unattainable limit. Rather, as I indicated earlier, it is an unending series of artistic "creations". Later creations are able to make abstractions about earlier creations.

It is like this. A given theory (e.g., Newton's) comes out. This makes inferential abstractions on the lower order descriptive facts about the field of mechanics. Later, one discovers that these inferences have limited domains of validity, and become confused when extended too far. So we have already begun to become descriptively conscious of the structure of inferences in Newtonian mechanics, as soon as we are led to criticize this structure. Then in Einstein's theory, we make inferences about our descriptions of the structure of Newtonian mechanics (e.g., that mass is not a constant at higher velocities, etc.). Indeed, certain key aspects of Newtonian mechanics are recovered as approximately valid inferences from the theory of relativity. But the situation is changed. For in Newtonian mechanics, they were the basic assumptions, determining the structure from which inferences are to be drawn. Now, in Relativity, they are merely particular inferences. (Of course, some day, Relativity must suffer the same fate.)

In this process, the older theory is not totally recovered as an inference from a later theory. Rather, only certain key aspects are recovered. For example, Schrodinger's equation contains Newton's law as an approximate descriptive inference. But it fails to infer the concept of a well defined particle state (position and momentum) even as an approximation. Einstein's theory does not allow us to infer the Newtonian concept of a rigid body. This is a serious deficiency, leading to the unsolved problem of how to describe extended structures in relativity.

So now, we are led to form newer abstractions, which recover more of the useful or correct aspects of older theories than was possible earlier. This process is generally ignored by modern scientists, who tend to suppose that they stand on "peaks" that are the unique culminating points of all earlier work. But this is, in my view, a delusion.

The factual test of a theory is a very complex story. Each theory contains distinctions, relationships, orders, patterns, and structures that are similar to those found in lower level abstractions. For example, in Newtonian mechanics, the theory

distinguishes the positions and velocities of a particle, (these distinctions being made mathematically, in the universe of discourse). When we work with actual objects, we make similar distinctions, which we describe in terms of words and numbers (coordinates). We notice the similarity of our theoretical distinctions and our lower order described distinctions. It has been verified long ago that these theoretical distinctions are faithful reflections of the corresponding descriptive distinctions. But in the theory, there are inferential relationships in these distinctions (e.g., that in free space $X_2 - X_1 = v(t_2 - t_1)$), where v is a constant, so that

$$\frac{X_2 - X_1}{t_2 - t_1} = \frac{X_3 - X_4}{t_3 - t_4}$$

We test the theory by seeing whether our descriptive distinctions are in fact related in the same way as our theoretical distinctions.

Note that the relationship between theory and fact is not a correspondence between "theoretical" objects or entities and "real" objects or entities. Rather, it is a correspondence of the abstract qualities, such as distinction, relationship, order, pattern and structure. These abstract general qualities are common to every order of abstraction, from direct and immediate perception on to the most subtle kinds of thought. Even when we see the form of an object, we do this by seeing all the distinctions, relations, orders, patterns and structures (DROPS)[29] in the outline and boundary surfaces of the object in question. When we feel the object, we feel all the DROPS in its movement and in the tactile and kinesthetic sensations that are produced by it. The "object" that we are aware of is constructed in the brain, as a provisional representation of all the DROPS that have been established about that "object".

This fact is brought out by a simple experiment. Two photographs of a scene are taken, one through a red filter and one through a neutral filter. These photographs are projected in the same screen, the first with red light, and the second with white light. So on the screen, the illumination must be everywhere a set of varying shades of pink. But you actually see all sorts of colours, such as blue, green, yellow, etc. Evidently, what the brain picks up is not colour, considered as a sort of "substance" or fixed quality. Rather, it is sensitive to differences of colour, to their relationships, orders, patterns, and structures as influenced also by their form and space relationships. All this information about DROPS is integrated by the brain into a "construction", which is what we see.

Would we say that our perception of the screen in the above case is "wrong"? Of course not. The test of its rightness, is whether the distinctions, etc., faithfully reflect real distinctions in the total pattern and structure of light, as registered in other ways (e.g., by other people or by instruments), and translated into words or mathematics descriptively.

In testing a theory, we frequently accept a lower order theory as a fact. We then use this lower order theory descriptively. For example, we may accept thermodynamics, with all its inferences, as a fact. In terms of this theory, we calculate free energy,

[29] The acronym DROPS is used a number of times in this folder and the next—CT.

entropy, and other quantities, which would have no meaning, unless the DROPS of thermodynamics fairly faithfully reflected the DROPS of the descriptions of observations based on thermometers, calorimeters, etc. In statistical mechanics, we can compute what Gibbs called analogies to the entropy, free energy, etc. But with the aid of atomic theory, we can draw inferences about these, beyond those that can be drawn in thermodynamics. Thus, thermodynamics is being used descriptively. This use is twofold. First, there is a domain where statistical mechanical inferences never contradict thermodynamic inferences. Here, statistical mechanics merely enriches the total set of inferences. But then, in another domain (fluctuations in Brownian motion or near the critical points of materials) statistical mechanics leads to inferences contradicting those of thermodynamics. Here, thermodynamics is no longer being used as a description of experimental fact. Rather, one is describing the inferential structure of thermodynamics, and making inferences about it (e.g., that its inferential structure is different from that of statistical mechanics). So we are now at yet another level of abstraction, which is abstracting from the inferential structures of several theories, and relating these structures. We must also, of course, compare all this to the described structure of the corresponding experimental facts.

In all this, there is no such thing as a fact. Rather, there is an ordered process of making the fact (which could perhaps be called "facting"). The "fact" is never finished, nor does it remain fixed. The "fact" is the momentary result of the movement or process of "facting". Although the result has a certain significance, it is evident that the process of "facting" itself must have top priority in our attention. It is like a tree. The fruits have a certain importance. But if the tree is dead, or malnourished, there will be no fruits. Unfortunately, mankind has generally been far more interested in "results" than in the living dynamic process, out of which these have to emerge. But such emphasis on "results does not in general even produce these very effectively. Truly creative and original "results" have almost always been produced by those whose main interest was in the living movement of creation (which is hardly noticed in philosophical discussions about the nature of scientific research).

One can raise an interesting question by relating the fact for art and the fact for science. In early days, these two kinds of fact were established in very similar ways. But gradually, they began to separate. In science (especially in physics), there is a systematic effort to make the observation of the experimental fact as routine and mechanical as possible (so as to remove the "personal" factor). Creative perception occurs only at much higher levels of abstraction of this fact. In art, however, the essential creative act must occur directly at the level of immediate perception. This is not to say that high levels of abstract thought have no bearing on art. They are indeed very important. But their significance is only in the way in which they are able to bear on the DROPS of the level of immediate perception. On the other hand, the aim of scientific research is not to create "beautiful" experimental results, at the level of direct perception. Rather, it is to create a theory that faithfully reflects the descriptive fact, and that does so in a coherent, logical, orderly harmonious way, that will therefore also be sensed as "beautiful".

The factual test of a work of art is very subtle and difficult. Nevertheless, it is not an impossibility. One who wishes to test it must, in certain ways, be similar to an

artist, in his creative approach to perception. If he is sensitively observant, his brain will register the DROPS, first non-verbally, and ultimately in words of description and inference. Another person, looking at the same work, can do the same. If they discuss, they can eventually arrive at a common understanding of what they see.

At first sight, it may seem that the scientist has a more reliable and simpler means of perception of the immediate fact. In certain ways, this is so. But in other ways, this "simplicity" and "reliability" are misleading, because they are purchased at the expense of sensitivity and flexibility of perception. Even in physics, we are probably "imprisoned" by our current mechanical modes of instrumentation, which channel research into certain narrowly defined directions. But if one goes on to biology, surely this limitation must become more serious. Will it not cause us to lose sight of the subtler qualities of life? And in psychology, it may well be catastrophic. For the mind is infinitely creative and dynamic. To try to "measure" its behaviour as one measures the location of a planet is evidently an absurdity. Will it not therefore be fruitful in psychology, in biology, and perhaps even in physics, to inquire into the question of whether there is perhaps a more sensitive perceptive, and artistic way of establishing the fact, at the level of immediate perception?

Of course, it follows from what has been said that the development of theories influences the structure of the fact. Once a theory has a certain domain of validity, its DROPS provide a framework, that tends to shape the development of instruments, which latter then provide facts within this framework. (E.g., to distinguish things according to the theoretical category of "spin" leads to devising instruments and experiments that produce distinctions and orderings of "particles" according to their spins.) Nevertheless, there is some limit to the possibility of such development. For theories whose DROPS do not cohere well with the kind of fact that they lead us to observe will eventually be given up. [Of course, people may be reluctant for psychological, religious, or other reasons (e.g., status) to drop theories of this kind. But this is only a failure to engage properly in the process of perception. It is like a man who refuses to look at something, that he does not wish to see.]

In spite of this mutual influence between forms of theory and forms of the fact, the distinction between what is a fact and what is not is at the basis of all perception. In any particular instance, there may not be a hard and fast distinction. Nevertheless, unless the basic intention in perception is to perceive what is a fact (rather, for example, than what would be pleasing and satisfying, even though not true), it has no real meaning. What is needed is that the inferences drawn from our theoretical notions shall apply on lower levels of abstraction, that are ultimately founded on direct perception. The partial shaping of these lower levels by our theoretical concepts does not really basically alter the case. Wherever one finds inferences that are not confirmed at more nearly descriptive levels, then the theory must alter, to accommodate this fact.

Even more, there must be a continual questioning, probing and testing, aimed at checking all possible kinds of inferences of our theories. It is here that pre-scientific ages generally failed. For example, to prepare a certain potion, they might give directions for what materials were required, how they were to be heated, etc. These treatments might include the saying of certain magical phrases. In earlier times, people were generally rather pragmatic. If the potion worked, they assumed that

all steps involved in preparing it were necessary. They did not generally think of testing the inference of necessity by varying the steps involved in preparation in some orderly fashion, and seeing what happens when these variations are made. Thus, they were unlikely, for example, to learn that the magic words might perhaps be dropped, without really affecting the activity of the potion. It may well be said that the notion of testing all possible inferences of our theoretical ideas is one of the really revolutionary things that has developed in more modern times.

It is important to remember that the whole fact is not merely external to man. It is also internal. These two aspects of the fact are actually inseparable. Indeed, in science (as opposed to art), the external aspect of the fact is generally rather mechanical and superficial, because of our current modes of instrumentation. The subtler and more profound aspects of the scientific fact are contained in the hierarchy of descriptive and inferential structures of ideas that has been developed over the centuries.

But, of course, the fact is not just a set of words and thoughts. Rather, it is, most basically, the movement of "making" the fact (or "facting"). In this movement, the mind is directly and non verbally aware of the verbal descriptive and inferential structures that I have referred to above. It is here that creative perception of what is new can take place in science. When such perception leads to some new order and structure (whether in a large or in a small context), we say that there has been a "flash" of understanding. If you are somewhat attentive to what happens in such a case, you may notice that this flash takes place only when the mind "lets go" of previously held orders and structures, thus leaving an "empty space" or a "silent period", within which there is room for creation of a new set of DROPS. The creative act is in no sense a deductive inference from previous knowledge, nor is it any other kind of inference (e.g., inductive or associative). But from the new set of DROPS, new kinds of inference can be drawn, permitting the new understanding to be tested.

It is crucial to realize that the act of understanding is non verbal, and not a reflection of anything else. This act is creative, in the sense, for example, that it creates new kinds of verbal and intellectual reflections or abstractions, which then help shape the lower order perceptual structures in new ways. The act of understanding is a very high order of awareness contact with the fact.

The act of understanding is evidently then not analytic (i.e., not a deduction from previous premisses). Nor is it synthetic. For by synthesis, we generally refer to the coming together of various constituents to make some "compound" that has new qualities, not contained in the components. Thus, there is the dialectical view that a thesis develops, and that because this is partial and one-sided, it leads to its anti-thesis. The two then lead to a syn-thesis, with new qualities, which in one sense contains thesis and antithesis, while in another sense, it puts these aside. But in my view, this is not what happens in the act of understanding. Rather, the essential point is that thesis and antithesis are both inactivated and dropped, when the mind is "empty" or "silent". The mind is then working on a faster, finer, more subtle level, far beyond that of words, with their thoughts of thesis and antithesis. It is "feeling out", this way and that, being in direct contact with the fact, "sorting it out" with tremendous speed, and coming immediately to a new set of DROPS, in terms of which the fact is comprehended without contradiction.

As a by-product, the valid aspects of earlier ideas can be recovered from the new set of DROPS as approximations, while at the same time, the limits of validity of the older ideas reveal themselves clearly. But the new ideas are in no sense a "synthesis" of the older ideas. Such a "synthesis", if it existed, would have to be confused, since it would be in essence a mixture of two or more contradictory ideas. It is necessary to stress the creative character of understanding, in the sense that this leads, in a single act, to a new total structure, with new "component" elements. This act takes place "all at once". It is not a time process, in which the older "elements" are combined and synthesized, in the hope of arriving at something new, which is not analytically deducible from the original starting point. For while it is possible to combine chemical elements synthetically in this way, ideas cannot be thus combined. For when ideas contradict each other, any effort to relate them can lead only to confusion. They must be dropped altogether – i.e., the mind must "let go" of them, and leave itself open to the unknown, which is the source of all creative movement. We generally find it very difficult to do this, because we are so strongly conditioned to hold onto the known, as if it were life itself. Very probably, the key difference between genius and mediocrity lies, not in the degree of talent, but rather, in the ability of the brain to "let go" of the known, so that it will be open to the creative movement of the unknown. And if creation is really the ability to lead to new orders, it must have its source in the unknown. Otherwise, it would merely be the adaptation of old knowledge to new situations.

I hope that by now you have "digested" your reading and your talks with Biederman. I am awaiting your comments with great interest.

With best regards,

D Bohm

Dec 20, 1966

Dear Jeff

Enclosed is a letter to Schumacher, that is relevant to the letter I sent you yesterday.

You might find it interesting to get in contact with Mr. Schumacher, who is doing a thesis on Bohr's epistemology. I have already mentioned your name to him.

You might find it enjoyable to look up an old friend of mine, Professor Melba Phillips,[30] when you are in Chicago. She is at the Physics Dep't of the University of Chicago. I have mentioned your name to her.

Best Regards

D Bohm

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[30] See Talbot (2017) for Bohm's letters to Melba Phillips in the 1950s—CT.

20th December, 1966.

Dr. D. L. Schumacher,
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Cornell University,
Clark Hall, Ithaca,
N.Y. 14850,
U.S.A.

Dear Schumacher,

This is a continuation of my letter of a few days ago. I also enclose a copy of a letter to a former student of mine, Dr. Bub, which is relevant to the topics that we are discussing.

With regard to denotation and connotation, of course you are right to say that both are necessary for communication. But I want to add something more. Denotation and connotation stand in a naturally ordered relationship. Denotation is the fundamental role of words, which must be given primary emphasis or top priority. Connotation is significant mainly insofar as it bears on denotation, i.e., insofar as it helps to make the process of denotation more flexible, more subtle, more sensitive, richer, etc. A little reflection shows that this must be so. For as I indicated in earlier letters, words in themselves have very little significance, unless they refer to something else that is non-verbal. Thus, the word "dinner" may call up the image of food, the taste of food, the feeling of eating it, etc. But it will not nourish you. Unfortunately, with regard to more subtle questions we have fallen into the habit of trying to live in a "world of words", and the images and feelings that they evoke. Thus, the whole of the "self" is evoked entirely by words. And whenever we use terms for which it is difficult to find the denotation (e.g., love, duty, honor, etc. etc.), there is a tendency to let their meanings be determined mechanically by manipulating words. For example, a child is told that it is his "duty" to love his parents. Therefore, he tries to manipulate his feelings so that they will seem to be the ones that conform to what is expected of him, while his natural and spontaneous feelings are covered up. In a way, he is then starved of real feelings, as he might be starved of real food, if he tried to satisfy himself with words, images, and their associated sensations of eating. So unless our more abstract words ultimately denote something real that is going on either inside or outside of us, they can lead to endless confusion.

Of course, you are right to emphasise that an inability to find denotation immediately does not necessarily justify us throwing out what is new and not understood as "mumbo jumbo" or "gibberish". Nevertheless, you cannot expose yourself to the tremendous totality of "new" things, indiscriminately. You did, for example, choose Stravinsky as the one to listen to, and presumably failed to put in equal effort in listening to others, whom you may have regarded as mediocre. I want to ask you what determined this distinction. Was it not perhaps a kind of perception at very deep levels that although you did not understand him, there was a new set of non trivial distinctions, relationships, orders, patterns and structures (DROPS) here, that was worth paying serious attention to? In a way, our creative responses begin very

deep, and stir in us in ways that are hard to specify. But I suggest that from the very beginning, your brain was, at deep levels, able to see that Stravinsky had a new set of DROPS, while other relatively mediocre composers did not. One has, of course, to be very sensitive and "vulnerable" to those faint "stirrings", or else they will be "drowned out" in the noise of everyday life.

I feel that it would be better to emphasize the creative character of novelty, rather than its synthetic character. As suggested in the letter to Bub, synthesis implies the coming together of older elements to form "compounds" with new qualities. Real novelty implies what is original and creative, going far beyond mere synthesis.

Also, I would not say that novelty lies in the "choice" of axioms or assumptions. The word "choice" has wrong connotations here. Choice implies the existence of a well defined set of alternatives, one of which is decided upon by the "chooser". Basically, there is nothing new in this, as all the alternatives were already present from the beginning. Creation implies the coming into being of a genuinely new total structure. Therefore, there is no "choice" in creation. On the contrary, one has a sense of logical inevitability in it, that it could not have been otherwise.

In any case, freedom is not the ability to "choose". For merely to choose among alternatives is to be determined by one's conditioning, which causes one to prefer a particular possibility over the others. Rather, freedom is born in the deep intention not to be a follower of old, familiar, and apparently safe patterns. This latter tendency is the principal barrier which impedes the mind from moving in freedom.

I still feel that you are paying too little attention to the question of factuality. Of course, the fact is often very subtle and difficult to specify. Perhaps you are conditioned to think of facts as isolated bits of information. This is unfortunately a result of our modern way of doing technology. As I indicated in the letter to Bub, the fact is something very subtle, vast, flexible, and dynamic. But it is nonetheless real, as well as the keystone of proper perception and a healthy mental life. When one ceases to pay attention to it, the result is illusion and delusion, born of the wish to present in perception something that is pleasing, rather than what is true and factual. Consider for example, our typical response to flattery. We find it very difficult not to respond semi-automatically, with an image of the "self" as sublimely beautiful, powerful, wise, good, etc, etc. This image evokes a wonderful glow of pleasure and a sense of well being and euphoria. Behind these feelings is hidden a "censorship" process, making us insensitive and "dead" to all perceptual and other evidence that the image is false. Thus, the brain deludes itself. For example, it may infer that one who flatters us always speaks the truth, while one who criticises us is a liar. In this way, it seems to "protect" itself from perceiving the fact that the flatterer is saying what he does, because he wants to take advantage of us. (Seeing this would make us "feel bad".)

It is clear that having a simple factual attitude to what people say about us is quite different from collecting hard "bits" of statistical information or "opinion polls". It involves a very subtle kind of perception, which is aware, not only of what people say, but of how we are reacting and responding. This the broad fact, both external and internal to us. Indeed, as I indicate in the letter to Bub, these are inseparably related.

You said that our ideas should be considered to be "heuristic". Could one not better use the word "provisional"?

Whenever you talk of subject and object, I somehow fail to understand what you are driving at. It seems likely that one of us is confused about this question. Which of us is it?

You say that the division of subject and object transcends anything that can be expressed in the content of scientific knowledge, because it is among the conditions required before such knowledge can even be discussed meaningfully. I wonder whether this is really a fact (in the broad sense of the word). On the one hand, it is quite easy to discuss this division within the content of knowledge, if we treat the subject as a special object (i.e. the one containing the "instruments" of perception). But then, of course, we see that the dividing line is arbitrary and freely movable. This ambiguity suggests that such a division is at best a matter of convenience, and not something of deep epistemological significance. On the other hand, in a deep sense, there is no way at all to discuss the division of subject and object, in fact, the word "subject" has no denotative significance whatsoever. While I agree that connotation is important, I also emphasize that unless a word contributes at least indirectly to determining some ultimate non verbal denotation in the total perceptual process, it has no meaning, and is merely a source of confusion. Thus, no matter what you do, you cannot prevent the mind from tacitly and unconsciously creating a "display object" out of images, thoughts, and sensations, which it calls the "subject" or the "self". This "display object" is pure illusion – it is in reality nothing at all. But it is created, because the mind cannot make sense of a word that has no denotative significance at all. Therefore, it tries to make sense by creating an illusory denotative significance. And this leads inevitably to confusion.

If you are perceptive of the whole fact, inward and outward, you will see that our theoretical constructions are also parts of this fact (as explained in the letter to Bub). So nowhere in awareness is there a division which could be denoted by the words "subject" and "object". Nowhere does one find any indication that theoretical ideas belong to a "subject" who is distinct from the "objects" that are the contents of these ideas. Rather, theoretical ideas are abstractions of high order, that reflect the general structure of abstractions of lower order, going all the way down to immediate perception. As Piaget put it, the infant learns to see what he hears and hear what he sees. Later we learn to think what we perceive and to perceive what we think. It is all one process, without a "thinker" who would be "doing the thinking". So where does the division of subject and object come in? Why do you insist that this is a precondition for all scientific descriptions? To me, it seems to be the result of certain related linguistic and social structures that have developed historically.

I must say that I do not understand the first paragraph of your letter. When you say "object" and "apparatus" are non descriptive notions, I ask you how the physicist is to know whether a given thing is a piece of apparatus or not. For example, I go to the grocery shop and buy a piece of cheese. However, the manufacturer has decided to sell it under the label "Observing apparatus". How am I to know that I should not also take this cheese to a laboratory and use it to observe an electron? Surely,

Bohr has some tacit notions in the back of his mind "describing" what an observing apparatus is supposed to do. It is not totally undefined and indescribable.

With regard to randomness, I wonder if our words on this issue are not sources of confusion. We speak of disorder. But this is impossible. Whatever we see is in some order. E.g., the throws of coin can be ordered as HTHHTHT, etc. etc. One feature of a random distribution is that there is not some "simpler" principle from which this order can be deduced. A random order is not deducible from another order with a smaller number of basic elements.

Now, you start to talk of the universe as random. Of course, this does not mean that it has no order at all. It means only that you have not been able to deduce the visible order from some simpler set of principles. In reality, we may then say that the order of the universe is known only descriptively and not as an inference from higher level abstractions.

Of course, when you look at such an "unknown" order, the brain creatively starts to "play with" higher level abstractions, to see whether it cannot find some that permit certain features of the perceived order to be inferred. If it should find them, these will then be stored up in the "reservoir" of known orders. The brain will then be able to "see" these orders relatively easily, because the whole mechanism of recognition and storage of information will be able to operate.

But notice that not every order that the brain "plays with" will turn out to be suitable. Here is where the question of factuality comes in. Only certain orders will prove to have a broad domain of applicability. Of course, men may prefer to hold onto orders that please them or appeal to them for religious, political, or other reasons. Then, as happened with the Schoolmen in the time of Galileo, they may protect their sense of security and pleasure by simply not looking at the facts. But this is, of course, the beginning of the deterioration of the mind.

I sometimes have the impression that you want to play a "game" of manipulating undefined words, such as "epistemology" and "subject", to see whether it does not produce interesting results. Perhaps this procedure may work in mathematics. But in physics, it will lead to confusion. It is true that we must use undefined words. But unless they ultimately denote something non-verbal, either external to us, or in our own mental processes, these manipulations must lead to confusion, in the way that I have described. And I insist that the word "subject" cannot possibly denote anything at all, either external to us, or in what we are aware of as going on the mind. Therefore, all it can do is to lead the mind to construct an illusory "display object", which it tacitly takes to be the "subject". And this is, of course, pure confusion.

Coming back to analysis and synthesis again, Rosenfeld's example of the potion that had to be made in a magic dish has been, in my view, wrongly interpreted by you. As I indicated in the letter to Bub, what is needed is continually to test our ideas by means of critical analysis, to see whether inferences of necessity (tacit and explicit) are really valid or not. The primitive way of thinking that Rosenfeld describes is also characteristic of young children, who very frequently fail even to think of the possibility of such test. As Piaget has shown, this is because they so often regard whole situations as "unanalyzable totalities" that "go together". When the components of such a situation have in fact no necessary property of being together,

Piaget calls this mode of thinking "syncretism" – i.e. the tendency to regard actually separable elements as indissolubly connected. Childish and primitive reasoning is full of syncretism. Nor is the civilized adult really free of such survivals of childish and primitive thinking. On the other hand, the civilized adult also tends to carry analysis too far, as he tries analytically to treat things that are in fact indissoluble wholes. One and the same individual generally reveals both extremes, in different aspects of his thinking.

I feel that in the effort to correct the wrong application of analysis, you have "gone overboard" in the direction of synthesis. Thus, it is not really right to equate the primitive man's belief in the power of magic with the scientist's belief in the power of chemical analysis. It is true that the latter's belief is also frequently wrong as he extends analysis beyond its proper scope. But to correct his error, it is not appropriate to return to the primitive syncretic way of thinking.

It is nevertheless frequently useful to combine elements of thinking synthetically, provided that they do not contradict each other. But then, these combinations must be treated as provisional. In order to test the validity of the new results that come from such combinations, however, a suitable analysis is needed. It is only with the aid of such analysis that one can criticize the inference of necessary connection of the various elements. Without analysis, one is likely to be trapped in making arbitrary tacit assumptions of necessity of connection, of which one is not even aware.

So analysis and synthesis are two sides of every process of reasoning. It is impossible to reason, unless our premisses are first synthetically combined in new ways, and then analyzed, for their results to see if the inferences drawn from the premises make sense logically and cohere with the fact.

But genuinely original discoveries transcend both analysis and synthesis. Rather, they arise in the creative perception of new total structures, involving not only new relationships, but new sets of basic "elements" that are thus related. Here is where creation differs crucially from synthesis. For the latter is just the combination of old elements in new ways, to give rise to certain new properties or qualities.

Of course, every creative discovery has to be expressed in way that involves synthesis and analysis of terms in our language. Synthesis may be compared to the composer's ability to combine already known themes in new ways. Analysis would then be the faculty involved in criticizing these combinations, to see if they "make sense" musically. But creative perception is a much vaster, deeper process, going on mostly at non-verbal levels of the mind. It is out of this process that there "wells up" an unending series of "ideas" that a great composer like Beethoven or Mozart is very probably always "playing with". Similarly, those who worked with Einstein said that in one day he tried and discarded more ideas than the average physicist does in a year.

The key point is this: Synthesis involves combining known structures in new ways. But most new combinations are "nonsense". Surely it is not pure chance that determines the possibility of ideas that are not "nonsense". Rather, there is a creative process that "throws up" combinations that tend, in some measure, to make sense. (These mast of course later be tested by analysis.) In this process, there

is not only new synthesis, but also a new set of "elements" leading into a new mode of analysis. Every creative step thus involves both synthesis and analysis.

Unfortunately, because of conditioning, most scientists are seldom able to get beyond the analytical stage. They are probably discouraged by the fact that their efforts at synthesis generally lead to nonsense. But they do not realize that useful synthesis is a by-product of a non-verbal creative movement of the mind, which is of an entirely different order from ordinary thinking and feeling. Indeed, the essential quality of what is called "genius" is very probably due to the fact that in certain individuals, this creative movement was, for fortuitous reasons, not "killed" by society in its effort to produce people who conform to certain standards or patterns, in their thinking and feeling. For evidently, if new ideas are continually surging up in the mind, society may find it hard to keep people "in line". So it generally does it's best to instill conformity to a pattern at a very early age, and with it, of course, the concomitant suppression of the creative process.

You say in your letter that it is wrong to try to analyze synthesis. I do not agree with you. Indeed, just as one can obtain synthetically formulated insights into analysis, one can also obtain analytically formulated insights into synthesis.

Analysis and synthesis are two complementary extremes that are both of the same general order. Therefore, they can reflect each other. I feel that you are confusing synthesis with creation, which is of an altogether different order of subtlety and dynamism. I would say that it is meaningless to analyze creation, because the real pre-condition for all fruitful analysis is the creative mind. This is also the pre-condition for all fruitful synthesis. So synthetic insights are also inadequate for understanding creation. Creation can be understood only with the aid of a yet higher order of creative perception. That is to say, creation may first be concerned with some particular field (such as physics, mathematics, art, music, etc.) But ultimately creation begins to work on itself, to give creative insights into the creative process. This in my view, is a still higher order of creation,

With best regards,

D. Bohm.

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