Chapter 7
Folder C135. Bohr Versus von Neumann. September–December 1968

Sept 24, 1968

Dear Jeff,

I received your two letters, and have sent off the recommendation to MIT as well as notified the College about the need for a PhD certificate.

The money situation, as described by you, does not look promising. I don't feel, at the moment, that the set up in Toronto would be appropriate. In any case, they haven't contacted me, as yet. To try to agree on a programme of research, so as to combine my interests, with those of the people who determine the grants, would probably result in a kind of self-cancellation, in which no interests would be realized. The very notion of having to report on "progress" would invalidate the basic form, needed for exploring the unknown.

Could you perhaps get some money to come here for a while (to London)?

Now, now about the contingent parameters, I want to make a slightly different proposal.

Let ψ_{ij} represent a wave function, with i indices corresponding to operations going on in a certain relatively localized region, while j indices correspond to operations going on in other region<u>s</u> (i.e., generally more than one region).

We now consider relatively localized operations, described in terms of the i index. It is important not to call them "measurement operations", as you do in your letter. My view is that these are "natural operations", which take place of their own accord. "Measurements" refer to special cases of these "natural operations", taking place under selected conditions, with arrangements of matter called "physical instruments", and with a further selection of those operations that are regarded as relevant. Thus, in a laboratory, a Geiger counter may click, a truck may pass by, and someone may switch on a light, all at more or less the same time. All of these processes are attended by vast acts of "natural operations". But of all these, only the results of operations in

C. Talbot (ed.), *David Bohm's Critique of Modern Physics*,
https://doi.org/10.1007/978-3-030-45537-8_7

the Geiger counter are regarded as relevant. And indeed, even in the Geiger counter, only one result, (i.e., the click) is regarded as relevant.

So the word "measurement" is not relevant to the fundamental descriptions of physics. It is of a status rather like that of the word "money". Money is needed to do experiments, but the word "money" has no place in the descriptions of physical phenomena or in the laws of physics. Similarly, measurements may (or may not) be important in scientific activity, but the term "measurement" has no place in the basic descriptions and laws of science.

Now I propose that in a localized operation, the contingent parameters depend only on the i index, and not on other indices. Thus I write

$$\xi_i{}^R$$

representing the contingent parameters that are relevant to localized operations belonging to the region R.

I now propose the following equations

$$\frac{d\psi_{ij}}{d\tau_R} = \lambda\psi_{ij}\sum_n J_n\left(\frac{J_i}{K_i} - \frac{J_n}{K_n}\right)$$

where $K_n = \xi_n^{*R}\xi_n^R$ and τ_R is "proper time" for the Rth region.

$$J_n = \sum_j \psi_{nj}^*\psi_{nj}$$

You will readily verify that

$$\frac{dJ_i}{d\tau_R} = \lambda J_i\sum_n J_n\left(\frac{J_i}{K_i} - \frac{J_n}{K_n}\right)$$

It follows that an algorithm will go through. If the $\xi_i{}^R$ have a random distribution, it follows that the probability of the ith result of a localized operation is

$$P_i = \sum_j \psi_{ij}^*\psi_{ij}$$

This operation will leave the wave function ψ_{ni}, and 0 for $n \neq i$.

If you work it out, you will see that for two regions, R and R', the results of the EPR experiment are consistently described.

An important question is then that of whether the parameters $\xi_i{}^R$ can be described as ordered in a way that is relevant to the general experimental conditions. For example, a particular $\xi_i{}^R$ may either be constant or change in a specifiable way for some period of time, τ_R, after which it becomes subject to contingencies that are the fortuitous results of what is outside the present field of discussion. If so, it becomes possible to reveal the parameters $\xi_i{}^R$, in terms of the order of results of successive operations. But there may be other ways to reveal the new order that is implicit in this description. For example, one may do the EPR experiment under conditions in which there would be no time for a "signal" to pass from region R_1 to region R_2. If the statistical distribution changed, this too would reveal a new order, beyond that of the "quantum" description. So it is important not to overemphasize the significance of any one way of revealing new orders, implicit in new descriptions.

When we come to the question of a "relativistic" theory, we are faced with a certain confusion. Bohr's consistent quantum description is admitted to be relevant only to non-relativistic theories (e.g., it depends on the smallness of $\frac{e^2}{c\hbar}$). Relativistic algorithms that are called quantum theoretical have been developed. But these are not consistent. This inconsistency is not merely in the infinities, etc., to which such theories give rise formally. Even more, there is no consistent description of the informal experimental conditions, similar to what Bohr gives for non relativistic quantum theory.

When Einstein criticized Bohr in terms of the EPR experiment, he began with the non relativistic formal algorithm. But in the middle, he brought in the idea of a light signal as a limiting velocity. This is an informal motion that is essentially relativistic. Thus, Einstein's discussion was not relevant to Bohr's statements, and Bohr's answer was not relevant to Einstein (because Bohr's notions are relevant only non relativistically). Bohr and Einstein did not notice that they had no way to meet. Thus, their discussion was a source of confusion to both of them.

Since relativity and quantum theory have not really even "met" thus far, one may suspect that a new language is needed, in which both "quantum" and "signal" are not fundamentally relevant terms of description. Rather, these will have to be abstracted as secondary features, relevant in some limited and contingent domain (as Newtonian conceptions are relevant in the domain of small $\frac{v}{c}$).

Now, one thing that is fundamental to a quantum description is the notion of a single wave function (inappropriately called the "quantum state"). A basic notion in relativity is the local character of all basic descriptive terms. But the wave function belongs to the totality of what is under discussion (in principle covering the whole universe). So we have a tremendous difference in basic descriptive orders.

How can this difference be brought into harmony? I suggest that to each relatively localizable region R, there is a relevant wave function, ψ_{ij}^R. But because ψ_{ij}^R depends on indices j as well as i, this means that the whole "universe" is potentially relevant to the "full" description of each localizable region. This is what we in fact find in common perceptual experience. But there is a special mechanical or dynamical mode of description, in which each element is said to be describable in terms that do not involve other elements. We are now considering that such dynamical descriptive orders have limited areas of relevance, not extending to the area now under discussion.

This means that the use of Schrodinger's equation as a kind of dynamical law for ψ_{ij}^R is also no longer relevant. What shall we do instead? I propose that it is necessary to perceive new orders of relevance.

If we go back to general relativity, we see a similar situation. Consider certain kinds of field quantities, such as the symbols $\Gamma_{\nu\alpha}^{\mu}$, describing a parallel displacement. When these displacements are integrated around an infinitesimal circuit, we are led to a new quantity $R^{\mu}_{\nu\alpha\beta}$, which is the curvature tensor. The assumption that the laws of physics are to be expressed in terms of $R^{\mu}_{\nu\alpha\beta}$ is then the assertion of a new order of relevance (i.e., of integrals of $\Gamma^{\mu}_{\nu\alpha}$ over infinitesimal circuits).

Now, now in our situation, we don't want to refer to infinitesimal circuits, because the whole notion of continuity may also be irrelevant. But we can still refer to finite circuits.

To do this, let us introduce the notion of a transformation $\Gamma^{R,R'}_{ij,kl}$, which plays a role analogous to a generalization of the notion of parallel triangles. That is, we assert that the relevant comparison of wave functions in different regions is through the "invariant difference operation"

$$\delta\psi^{R}_{ij} = d\psi^{R}_{ij} + \sum_{kl} \Gamma^{RR'}_{ij,kl}\psi_{kl}$$

where $d\psi^{R}_{ij} = \psi^{R}_{ij} - \psi^{R'}_{ij}$, which is the "simple difference".
Then, we consider "circuit integrals" of such displacements, giving rise to a "curvature", associated with a circuit, C

$R^{C}_{ij,kl}$

When we assert the relevance of the quantity $R^{C}_{ij,kl}$ to the laws of physics, we are bringing in a new order of relevance, in terms of which the ψ^{R}_{ij} for different R can be related. But these relationships will be of new kinds. Schrodinger's equation can only be a special contingent relationship, relevant in limited areas.
Now, in Einsteinian theories, there is a further relationship, such as $R_{\mu\nu} = T_{\mu\nu}$ (where $T_{\mu\nu}$ is the energy-momentum tensor) which connects geometrical relevancies ($R_{\mu\nu}$) with dynamical relevances ($T_{\mu\nu}$). In the theory of contingent parameters, we could have a corresponding relationship connecting $R^{C}_{ij,kl}$ with some corresponding function of the contingent parameters. This would perhaps be the means by which the contingent parameters would be revealed, by their implications for the directly descriptive parameters ψ^{R}_{ij}.

All this is very sketchy, of course. It still has to be developed. And anyway, it is only heuristic, at best. We may well be led to very different modes of description, in the long run. But it is perhaps useful to consider it as a content that gives one some notion of what are the real questions involved here. In other words, we have to be free to set our thinking loose from habitual "moorings", and to experiment with new forms and orders of relevance, more in an "artistic" fashion, than in the fashion of what is now generally called scientific (but which is really technological).

Now, to come to your comparison of logic and geometry. Logic: quantum theory :: Geometry: Einstein's relativity theory. First, let me say something about geometry. I propose that geometry is mainly a language – a mode of description and inference – and therefore cannot be regarded as "empirical". Rather, the real question is: "Is a particular geometry relevant or not?"

Consider the postulate of parallels, for example. It is well known that this has little meaning, apart from further assumptions on how light and matter are assumed to move, in relationship to what are called parallel lines. Thus, any facts explainable by non-Euclidean geometry plus the assumption that light rays follow a system of "parallel" lines can also be explained by Euclidean geometry plus the assumption that light rays follow suitably "curved" paths. To judge between these theories depends on a perception of their respective relevancies. And this judgement is more akin to aesthetics than to technology (i.e., it cannot be based on the results of "measurements" or other "empirical" factors).

A geometrical language is useful in helping us to perceive relevance in a visualizable form. But because geometry means "measurement of the Earth", it also tends to lead to an unconscious metaphysics of a very harmful kind. For the "geometrical objects" tend to be thought of as "actually existent", as if one could touch them and see them. In fact, of course, they are only abstractions. But sooner or later, one begins to think in terms of geometry. In other words, "What is" is Euclidean space, Minkowski space-time, curved space, Hilbert space, or whatever else one is led to consider, by current scientific theories. As a result, one loses the necessary freedom to change the description in a fundamental way.

Now, Einsteinian relativity is primarily a form of language: i.e., the assertion of the irrelevance of "absolute" frames of reference. Minkowski geometry is another language, which enables us to visualize much of the content of Einstein's language. The two language forms are relevant to each other. But if we say that Minkowski geometry is "empirical", it is like saying that the subject-verb-object relationship is "empirical". What is appropriate here is only the relevance of a language form, and not its "empirical" verification or falsification.

Much the same sort of remarks can be made about logic. Quantum theory is, like Einstein's theory, a form of language. Logic is yet another form of language. That is to say, we feel the necessity for our communications to be in a certain order that is called logical. We have tried to describe this order by means of what are called rules of logic. But recall that the exception is the rule. That is to say, the relevance of a rule is in those situations that can clearly be seen to be either exceptions or non exceptions to the rule. Thus, the rule "Keep to the right" has no relevance, either as exception or non exception, when one is eating dinner. So the relevance of our so called rules of logic is shown just as much by those areas where they fail to hold as by the areas in which they are valid. And, of course, one must consider areas where our rules of logic are irrelevant.

As soon as we say "Logic is empirical" we are irrelevantly treating logic as an object of discourse, like a chair or a table. For, we fail to notice that what we say "about" logic immediately becomes an inseparable aspect of the logical rules that we are tacitly and informally following. In other words, it is irrelevant to talk "about" quantum theory, because this very talk is quantum theory. Similarly, it is irrelevant to try to talk "about" logic. This very talk is the logic that we want to talk "about". So, we can "talk quantum theory" and we can "talk logic". Our only remaining question is: "Is there any relevance to what we are saying?" Relevance cannot be analysed, and cannot be confirmed or falsified "empirically" (i.e., by technological means).

Just as we can have alternative geometrical languages, and no "empirical" way to choose between them, so we might have alternative descriptions of what we wish to call the rules of logic, without any "empirical" way of deciding between them. But what is more significant is that the really destructive factor is the attempt to treat logic and geometry as metaphysical objects of discourse, rather than as forms of language. It is this factor that has led to the modern tendency to take the forms of inference (i.e., logical rules) as having primary relevance, while the informal description is regarded as a secondary feature, which is just taken for granted. That is to say, high-powered mathematicians (like v. Neumann and his followers) have given tremendous attention to very sophisticated forms of inference, while they just accept common everyday informal descriptions, of incredibly naive character. The naive character of their informal language is not in harmony with the extreme sophistication of their formal language of inference. But this does not disturb them, because they regard the informal language as not very relevant. Or else, if they admit its relevance, they feel that it is a fixed thing that can never be changed basically, so that all the attention is directed only to changing the formal language of inference. But what is relevant now is to change the informal language, so that informal and formal languages can be in harmony. You will never get this harmony, in my view, if you try to start with the formal language of inference, while you tacitly accept most of the extremely naive and crude informal language to which physics is now committed.

With regard to the paper of Clark and Turner,[1] I have talked with Clark (who is now a part-time research associate here, working in Birmingham one day a week, and living in London, coming to Birkbeck as "honorary" unpaid research associate). He accepts your criticisms, but points out that the words "dual space" are what confused him and Turner. They treated the contingent parameters as literally a dual space. The use of "bra" and "ket" notation encouraged them here. It would be better to say simply that the "spaces" are different, without referring to "duality" at all. (Don't use Dirac's notation, but just ψ_i and ξ_i.)

We all send you and your wife our best regards. Will write more later.

Dave.

P.S. To sum up the situation, a language form or a set of rules has areas of relevance, outside of which it is irrelevant.

In its area of relevance, it has sub-areas of non-validity (or exceptions) and sub-areas of validity (or non-exceptions). The only "empirical" questions are those that have to do with areas of exception and areas of non-exception to the rules. The deeper question of what is the area of relevance is not in essence "empirical" (though it also does not exclude the "empirical", as potentially relevant to the question of relevance). Consider for example, the Mad Hatter[2] in Alice in Wonderland saying:

"I don't know why this watch doesn't run, although I used the best butter".

One could imagine an answer saying:

"The trouble is that you actually used second grade butter."

One could then try to settle the issue "empirically", by looking at the packet of butter, and seeing whether it were "first grade" or "second grade". But there is no

[1] See Clark and Turner (1968) (information from Jeffrey Bub)—CT.

[2] See also C134, p. 205, n 3—CT.

"empirical" way of deciding whether the grade of butter used is relevant to the running of watches. It is more a question of general harmony of concepts and percepts, which is basically "aesthetic" in nature. Similarly, given a certain theoretical content, there is no "empirical" way to determine its relevance. But unless it is relevant, "empirical" observations referring to this content will be largely sources of confusion.

Therefore, one may well ask: "Is the proposal of new rules of logic relevant to the physical situations indicated by the term 'quantum'?" Unless it is relevant, attempts to test these rules "empirically" will be confused.

One interesting point is that those who propose new rules of logic are still informally using the old rules of logic, to describe their new rules of logic. This is a disharmony between content (new rules of logic) and form (old rules of logic). It is similar to the statement: "Never use a preposition to end a sentence with".

The separation between the formally prescribed rules of inference and the informal rules applying to this prescription is itself an arbitrary and irrelevant act, a kind of disharmony. For the formal rules are the informal rules, and vice-versa (as the modulation of a radio wave is the radio wave, and vice-versa).

What is needed, in my view, is a new informal language that discusses the relevance and irrelevance of formal statements, in particular contexts and areas of experience.

Nov 5, 1968

Dear Jeff

Received your article[3] and letter. I apologise for the delay in answering, as I have been rather busy. Meanwhile, I have written references for you to Kansas, Boston, and Los Angeles. I hope you get a suitable job soon.

Your article seems most interesting. It clarifies the question quite a bit. It is useful to relate our own work to my 1951 papers,[4] as you do. Indeed, one can say that in my 1951 papers, I use contingent parameters that are restricted to being delta functions in configuration space. But the probability doesn't come out naturally any more. Rather, it has, in some way to be imposed, as $P(X) = \psi^*\psi$.

Perhaps the words "hidden parameters" or "hidden variables" should now be dropped altogether. Instead, we could talk of an "extension of a given description, to include further parameters that are contingent in the context of the original description". For short, we could say "an extension to contingent parameters" or "description in terms of an extension to contingent parameters".

I would suggest that you remove the word "neo Copenhagen" from your paper. There is no Copenhagen point of view. Bohr, Heisenberg, Rosenfeld, Pauli, etc., etc., all had different and mutually incompatible ideas on the subject. Rather, I would say that my 1951 papers were "an extension of Bohr's wholeness of description to include contingent parameters".

[3] Published as Bub (1969)—CT.

[4] Bohm (1952a, b)—CT.

Firstly, because of historical reasons, I formulated the 1951 papers in terms of measurements. Now, I would say that each set of parameters (e.g, for an "electron") had to be complemented by parameters corresponding to the rest of the universe. This may include what is called an "observing apparatus". But more generally, the latter is irrelevant. Every such description (in the 1951 papers) contains a formal and contingent distinction between foreground (the electron) and background (the rest of the universe). From one instance to the next, this distinction may vary (as does that between contingency and necessity). But what is universal to this form is that somewhere it has such a distinction. However form and content are an unanalyzable whole. So without a specific content in which this form is relevant, the form would be "empty". It is our work to try to develop such a content, that could be relevant in perception beyond the word. As yet, our work has only gone a limited way toward this aim.

Would you send us the references to the work of Kochen and Specker?[5] One can say, from their work, that extensions of quantum theory to contingent parameters cannot work, if the form of the description is the classical dynamical one, of disjunction between "observed system" and "rest of universe".

This terminology is superior to saying that there are "no hidden variable theories".

Best regards

Dave

Nov 19, 1968

Dear Jeff

Thanks very much for your article[6] and for your letter. I have sent a letter to York, as you requested. I hope that by now you will have some definite offer, from one of the places at which you applied. Things seem to be getting more difficult now, with regard to jobs. But there seems to be a good chance that among all these applications, you will get one or more offers of positions.

I hope you received my previous letter, in which I gave some responses to your article. In particular, I would like to repeat that phrases like "Copenhagen" or "Neo Copenhagen" point of view don't really mean anything, because there never was an agreed "Copenhagen interpretation" (e.g., Heisenberg, Pauli, etc., all had points of view different from Bohr's and from each other).

More generally, I would say that the content of your article is a valuable contribution to the subject, but that its form is not in harmony with the content. Perhaps the enclosed article[7] will help explain what I mean by this in more detail.

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[5] See the references given in Bub (1969)—CT.

[6] See p. 231, n 3 above—CT.

[7] Probably Bohm and Schumacher,"On the Failure of Communication between Bohr and Einstein", included here as Appendix D—CT.

First of all, by emphasizing "hidden variables" so much and at the same time asserting that the term is meaningless or otherwise inappropriate, one tends to confuse the reader. For informally, "hidden variables" are being treated as very relevant in your article, while formally and explicitly, you assert their irrelevance. (This is rather like what Bohr did in his answer to Einstein, as described in the enclosed article). In particular, you say also that "hidden variables" constitute an appropriate description for theories that would fit the criteria of Kochen and Specker. But actually these are no more "hidden" than are those of our own Rev. Mod. Physics paper,[8] or my 1951 papers. Thus, in classical statistical mechanics, atomic variables are not "hidden", though they do correspond to the general requirements of Kochen and Specker.

It may be that your difficulties stem from the adoption of an "informal form" of argumentation and proof for your paper. Is this a relevant form to state what is novel? It seems to me that the argumentative form always tends informally to adopt the existent older modes of thought and to criticize them. Criticism may be positive or negative. That is, you may agree with certain points and disagree with others. But this approach begins by accepting the relevance of the older modes, so that it is not appropriate for saying something new, that implies their irrelevance. To say something new, you have to set the older forms aside, so that you cease to adopt a critical attitude to them, either positively or negatively. Likewise, to attempt proofs in terms of older theories also interferes with saying something new, which requires a different language form.

If I may make a suggestion here, I would say that what is missing in your article is a discussion of necessity and contingency. What really motivated my 1951 papers was informally and tacitly the question of expressing the contingency of individual events. For accidental and fortuitous reasons having to do with the historical development of the subject, I inappropriately tried to express contingency in terms of "hidden variables". Likewise, I was led to talk of the wholeness of observed system and measuring apparatus, when it would have been relevant to talk in terms of those parameters that one specified explicitly in the foreground of the discourse and those that are only implicit and that describe a background (usually called the "rest of the world"). In certain cases, this background may be regarded as functioning like a measuring instrument, but this is, of course, only a limited and special kind of context.

We can now say that any statistical theory can be extended by the introduction of parameters that are contingent in the context of the theory in question. One mode of extension is to describe these parameters informally as existentially disjoint from each other and from the background, or general context of discourse. This is the classical dynamical mode of description (e.g., atomic variables to explain thermodynamics). Another mode of informal description is to regard foreground and background as a whole, without existential disjunction of the kind described above. This is what I did in my 1951 papers and what we did in our Rev. Mod. Phys. papers.

In such a paper, "hidden variables" should appropriately be mentioned rather unobtrusively, perhaps in a footnote, to call the reader's attention to the historical

[8]Bohm and Bub (1966a, b)—CT.

context, and to the inappropriateness of this terminology. In this way, the form of saying it would agree with the content, i.e., that "hidden variables" are irrelevant, and therefore of no significance.

It would be useful to go over briefly the discussion of Bohr's deep philosophical insight into wholeness (the term "thesis" is however perhaps too argumentative in its form). This insight is highly implicit, tacit, and informal, but it is really what is most novel in Bohr's point of view. The basic point is the inseparability of form and content. That is to say (as in a game), the form reveals itself as a set of rules that are working in each concrete instance that constitutes the content, so that the form is the content. (See the enclosed paper for a more extensive discussion of this point.)

Now, we come to more particular modes of realizing this wholeness of form and content. Thus, Bohr considered the content to be a statistical set of experimental conditions and experimental results. These were described informally in terms of classical language. The informal incompatibility of different experimental arrangements corresponds to the non commutativity of certain operators in the formal algorithm. Your discussion (particularly of Kochen and Specker) makes it clear that no extension of this kind of theory in terms of contingent parameters can have the classical form of existential disjointedness. So, if one wishes to extend the description to include individual events as contingencies, one has to have a form of wholeness, rather than existential disjointedness. Thus, Bohr's deep and general insight of wholeness is still relevant. But his more particular insight of complementarity (described in terms of classical language) may cease to be relevant.

In particular, in my 1951 paper, it was implicit (but never stated unfortunately) that the "background" is not to be described classically. Rather the "apparatus" always had a wave packet, with a "particle" inside it. The "packet containing the particle" was the only relevant packet. But this "packet" with a "particle" inside is like a combination of Hamilton–Jacobi theory (a wave) with a particular ray (the particle trajectory). It is really a different classical description, with new potential content (e.g., a statistical "jumping" of the "particle" between packets to describe a state of finite temperature, and to include statistical mechanics as basic to the large-scale description).

One could combine the 1951 theory with certain aspects of our own theory: For example, we could add non-linear terms to the 1951 paper tending to cause the "wave function" to go to zero, except in the "packet" containing the "particle". These terms could be significant only in the "multi-parameter thermodynamic type background".

I hope this will help you to orient your modifications to the proposed article.

Best regards

Dave

Dec 5, 1968

Dear Jeff,

Thanks very much for your letter of Dec 2. I do hope that the question of your getting a job will soon be settled favorably. It must be very worrying to be subject to such uncertainties.

It is very hard by correspondence to discuss issues as subtle as those raised in your letter. I know by experience how easy it is to convey a wrong meaning in a letter, one that is not intended. In conversation, all this can be detected and corrected in a few seconds, but by mail, it leads to a growing structure of confusion and failure of communication. I shall therefore try to be as clear and straightforward as possible, and hope that you will "read between the lines" where necessary, if you should get a feeling of lack of understanding, or irrelevance, or any other sense of failure of communication (not that I actually expect this to happen, of course!)

Firstly, I was very much struck by your statement that you are the disharmony of opposing judgements of relevance (i.e., those of the group following von Neumann and those that you find in my work). Of course, there is no meaning to your trying to reduce this disharmony. But perhaps we can discuss these questions, and see more deeply what is implied in them.

Whenever there is a disharmony of form and content, communication is almost certain to be faulty. For the mind takes in the content overtly, explicitly and consciously, while it takes in the form tacitly, implicitly and sub-liminally (or unconsciously). So your reader takes in mutually irrelevant judgements as to what is relevant. And just because a great deal of this is "unconscious", he cannot help getting caught in confusion (after all, confusion is, in essence, just the result of the working out of mutually irrelevant judgements of relevance). So the almost certain result of publication of your paper will be the "confounding of confusion".

Given that all this is the case, I am led to ask: "Why do you want to publish a paper, written while you still are a disharmony of form and content? Why can't you wait until (at least as far as you can perceive) you are such a harmony? Then your paper will be a contribution to harmony, peace and understanding, rather than to disharmony, contention and confusion."

Now, let me suggest some of the main reasons why you are a disharmony at this moment. In my view, the main reason is that, in some sense, you are trying to "reconcile" my relevance judgements with those of v. Neumann, Jauch and Piron, Gudder, et al. Since this cannot be done, you are the "problem" of trying to achieve the impossible. You will inevitably continue to be this "problem" unless and until you see its irrelevance, in which case you will lose interest in the "problem" and cease to be it (i.e., interest and relevance are one whole, whatever is relevant to you will interest you and what interests you is what is relevant to you).

In this connection, it would be appropriate to say something about v. Neumann, et al. It is clear that v. Neumann and his followers rule out Bohr as irrelevant, not by saying so explicitly, but rather, by simply not mentioning him at all. Nevertheless, if v. Neumann had been genuinely serious on this point, he would have discussed the issue and stated why he regarded Bohr's views as irrelevant. By saying nothing at all on this score, he informally implied agreement between him and Bohr on all that was essential. But since their views are so different (as is evident at least sub-liminally to almost every reader), the reader is led to try unconsciously to find what assumption gets rid of the confusion. The assumption that apparently does this is: "Only the formalism is relevant". This is of course very probably just what v. Neumann wished to convey, in some deep sense. But then, this led to the "measurement problem"; i.e., how can the formalism be related to experience: And from this flowed endless confusion.

Now, I feel that because v. Neumann wrote as if Bohr never said anything at all, one is led to doubt v. Neumann's seriousness on this point, and therefore the relevance of what he says. It is only on the assumption that Bohr's work is actually irrelevant that one can justify the notion that v. Neumann was serious in these discussions. Indeed, the followers of v. Neumann do tend to imply the irrelevance of Bohr's work, as you yourself point out. But then, since you evidently feel Bohr's work to be relevant, this leads me to another question: "How is it possible for you to regard both Bohr and v. Neumann as relevant?" To me, it seems that if Bohr is relevant, v. Neumann's work is almost pointless. And if v. Neumanns work is relevant, then Bohr is pointless. Any effort to reconcile these two judgements of relevance would then have to lead to disharmony. Perhaps this would explain (at least in part) why you are at present a disharmony of form and content. For you are adopting both Bohr's and v. Neumann's mutually irrelevant judgements of relevance.

In a similar vein, let me raise a closely related question. Of course, you are right to emphasize the need to discuss the logic of J. and P., in terms of their own views as to what is a relevant aim of research in physics. And of course, you note that you have to discuss my views in terms of my judgments of relevance on this score. Here, you would probably agree with me that if Gudder compares my formal equations with those of others in terms of his own purely formal criteria of relevance, he is not discussing my views at all. He is tacitly ruling them out as irrelevant. But by making a "discussion of my theories" the form of his discourse, he also implies that he is discussing my views. Thus, he is "confounding confusion", so that what he says is worse than useless. However, in your discussion of my views, placed in parallel with those of v. Neumann, J. and P., Gudder, et al., you are forced to include all sorts of mutually irrelevant judgements of relevance, and asking the reader to accept the content of these judgements; i.e., to think in terms of the forms implied by these judgements. How is the reader to do this? Will he not become, as you have become, the disharmony of form and content? Will this not confuse the reader, rather than help him to clarity?

My question is then: "Will you continue to try to discuss mutually irrelevant judgments of relevance in the same paper, with the implication that the reader is asked to subscribe to all of these judgements, or to try to "choose" between them?" (If you haven't been able to make such a choice, how could the average reader, who is much less well informed and has thought a lot less on the subject?)

Is there any way for you to be creative, without adopting my views, or Gudder's views, or Schumacher's views, or someone else's views? I feel that the answer to this question is "staring you in the face". What you can do is to recognise explicitly what your paper already is implicitly, i.e., a new kind of historical discussion, showing how different physicists had different relevance judgements, and how their failure to take this into account properly led to a breakdown of communication, with its resultant confusion. All sorts of questions are actually raised by you, that are germane to this point. What did various people, such as v. Neumann and myself mean by the term "hidden variables"? In terms of each person's meaning, did that person actually accomplish what he claimed to accomplish? If not, why not? What probably confused him? How could his objectives have properly been achieved?

In this way, you will no longer be asking the reader to accept, or choose between, mutually irrelevant judgements of relevance. Rather, he will be learning that what is relevant is the irrelevance of holding to fixed judgements of relevance, especially when one wishes to communicate with others.

Now for some more technical points. Firstly, you say that "hidden" means "operation behind the scenes". Let us provisionally accept this definition. But then, this is just what I meant by the word "hidden" as applied to the parameters of my 1951 paper. And it is an equally relevant meaning of the term as it appears in our Rev. Mod. Phys. paper.

However, you suggest that it is "natural" to take v. Neumann's meaning for the word "hidden variables". I wonder what it means to say there is a "natural" problem of hidden variables. Usually, when people say that something is "natural", what is meant is "habitual", in the sense that it seems easier to think that way and harder to think in another way. But the word "natural" also tacitly and informally implies a kind of necessity, and thus denies the contingent character of all such meanings (i.e., they are contingent on a host of factors, historical, environmental, psychological, etc., rather than "built into" the very structure of the subject under investigation.) So I would say that habitually physicists had come to think that all descriptions had to have the character of being "potentially disjoint from the rest of the world, belonging to the observed system alone". However, I discovered in 1951 that one could have a description which did not have this character, in terms of parameters that were "hidden" in the same sense as v. Neumann could say that the parameters he talked about were "hidden".

Very probably, v. Neumann would have regarded such a form of description as irrelevant. But I could now ask a further question: "Do you also adopt v. Neumann's position that it was "natural" (and therefore in some sense inevitable) to restrict the term "hidden parameters" to mean parameters describing disjoint parts of the world, considered as objects potentially available for observation?"

I feel that if your answer to this is in the affirmative, it will be difficult for you to give a clear presentation of my views, and that you will thus enjoin on me the arduous task of distinguishing what you say about my views from what I feel that these views actually are. Indeed, in my view, it would be more appropriate to say that v. Neumann's meaning of the term "hidden variables" was the "unnatural" one. For by considering what Bohr had to say, in an informal way, it became almost trivially obvious that no "disjoint hidden parameters" are possible. The mystery is then why so much effort has gone into proving the obvious. (This is, in fact, as I recall, how I felt intuitively about this problem in 1951.) In other words I felt that it was "natural" to regard v. Neumanns kind of "hidden variables" as irrelevant to the actual nature of the quantum theory.

In my view, it would be useful to suggest, at the very outset of your article, that the confusion arose, in large measure, because the scientific community was not generally able to consider the relevance of descriptions going beyond the classical disjunction of the observed object from the rest of the world. My own confused response to this confusion was then to take this behavior of the scientific community as relevant, at least tacitly, by adopting the informal form of trying to "refute" von Neumann's "proof". However, it was hard to avoid doing this, because everybody was always tending to raise the question. It would have been better if I had started by firmly asserting the irrelevance of what von Neumann had done to what I was saying, rather than to do this in a rather implicit form, towards the end of two long articles, at a place where it could easily escape the notice of most physicists.

I feel that you give the impression that whereas I had no clear idea of what I was "up to", those who followed v. Neumann were by contrast quite clear in this regard and were indeed following "natural" lines of development (implying, that in some sense, my lines were "unnatural" or "less natural"). Your main criticism of von Neumann et al. is that they misled themselves on how successful they were in attaining their otherwise valid and clearly defined objectives. But in my view, this puts things upside down. The entire v. Neumann line was deeply confused and therefore worse than useless. The mere fact that v. Neumann ignored Bohr and put his whole "authority" behind this, did irreparable harm to physics. And if von Neumann had listened to Bohr, he could never have written as he did. He would have seen that his whole approach was a combination of mutually irrelevant notions, and therefore, a "confounding of confusion". Because you do not mention any of this in your article, then you are "willy nilly" tacitly subscribing (at least in the minds of most readers) to the current view that regards v. Neumann as relevant and Bohr as irrelevant. (Recall that the situation is such that both cannot be relevant together, in this context.)

Next, I think that there was a very significant difference between Bohr and Heisenberg. Certainly, I could never–never–never agree to having my views called "Neo Heisenberg", whereas "Extension of Bohr's views" is, in my opinion, not an entirely inappropriate way of referring to them.

Heisenberg's term "uncertainty" or "indeterminacy" or "indefiniteness" implies an informal form that is a disharmony of form and content. First, you assert "definiteness" and then deny it by saying "indefiniteness". But to deny definiteness is to make definiteness relevant. So you are making relevant just that which you wish to assert to be irrelevant. In other words, the deep meaning of quantum theory is not the absence of definiteness, but rather the irrelevance of definiteness. Indeed, to use the word "indefinite" means implicitly that lack of definiteness is only a contingency (e.g., contingent on the quantum algorithm) and that, in the informal language, room is left for definiteness in principle to be relevant. It is implicit in Bohr's statements that definiteness is irrelevant, and that words like "uncertainty" are therefore a disharmony of form and content.

Heisenberg's work is characterized by an overall insensitivity to the harmony of form and content. His "microscopic experiment" is an example of this (as explained in our paper). In this, he is far from Bohr, in whom one sees everywhere a recognition of the relevance of harmony of form and content. Even when he doesn't achieve this harmony, he shows an understanding of its importance. But Heisenberg often seems to aim purposely at such disharmony, or in some sense, to prefer disharmonious forms of communication.

It seems to me that Heisenberg was <u>influenced</u> by Bohr to talk in terms of complementarity, while deeply, his philosophy of "uncertainty" (i.e., disharmony) remained in the informal form of his discourse. Vice versa, Bohr was influenced by Heisenberg to talk in terms of "uncertainty relationships". However, in my view, this term is totally out of harmony with the informal form of Bohr's discourse, and has served greatly to confuse the meaning of Bohr for most physicists.

Finally, I do not think it would help for me to write a comment on your article. It would probably lead people to say: "After all, it must be a confused subject, because even those who work on it together cannot avoid getting into unresolvable arguments." In addition, let me say that the whole <u>form</u> of argumentation, proof, refutation, etc., is not relevant in this context. To "refute" is usually to pick on certain points of someone's discourse, regarded as relevant in your own view, and then to show that the other fellow's notions fail to harmonize with your own. Our own title for the J. and P. article should have been "On the Irrelevance of what J. and P. have to say for physics." But this would have been <u>called</u> "impolite" (E.g., as politeness prevented Einstein from entitling his article "On the Irrelevance of Traditional Notions of Space and Time".)

I shall end up with some more questions. Are you trying to make comments "about" the work of other people, such as v. Neumann, J. and P., Gudder, and myself? Or are you trying to say something new? If so, are you regarding the technicalities of current efforts to put the "axiomatic" foundations of quantum theory into better order as a possible source of novel discoveries?

In my view, the key to something new is in changing the informal language first. To go on trying to repair the present formalism is like a person who lives in a condemned building, and spends a lot of money repairing it, decorating it, etc. I would ask him why he does this. Why doesn't he look for (or try to build) a sound new building, into which he can put his energies. He may have to live in the old building for a while yet, but he need not think <u>in terms of</u> putting the old building into perfect order.

Regards

D<u>ave</u>

Dec 10, 1968

Dear Jeff

I just received your second letter (Dec 5). I hope you received my answer to yours of the previous day. Meanwhile, I think it worthwhile to add a few further comments.

First, I am glad to see how you are beginning to understand the irrelevant way in which words are often used, to create what appears to be "good feelings", rather than to communicate concerning their supposed content.

Of course, you are right to cease to use words like structure, function, wholeness, etc., in this way. As I indicated in my previous letter, your latest article does have a potentially relevant content of a novel kind. However as long as its form and content are not in harmony, it will not actually communicate its content in a useful way.

You ask "What is quantum theory anyway?" This is indeed a very good question. In the article with Schumacher, it was suggested that there is no such "thing" as quantum theory. That is, quantum theory is not an object of relevant discourse. To talk "about" the quantum theory is actually to change its content in a generally irrelevant way; i.e., without our realizing that we are changing quantum theory when we think we are only "reporting factually" what the theory is "about".

Thus, when v. Neumann, J. and P., Gudder, et al. say something like "Quantum theory is a non Boolean lattice", they are not only adding something to quantum theory (i.e., a new formal description saying something "about" the earlier formalism), but they are also subtracting a great deal (i.e., the essential features that gave the theory content and made it fruitful). For example, most of the ability of quantum theory to be useful and relevant in connection with actual physical content depends intimately on largely informal requirements of continuity and single valuedness of the "wave function" (e.g., without it, energy levels of atoms, explanation of superconductivity and superfluidity, etc., would fall to the ground). It is almost impossible to indicate these essential features, in terms of a description, such as "non Boolean lattice". I would be ready to challenge anyone to make the notions of J. and P. or Gudder relevant to any actual physical content. I think you will see it can't be done. Rather, this stuff is just like the Medieval discussion of the number of angels that can dance on the head of a pin. However because of modern conditioning of thought, whenever physicists see "densely packed" formalism, they have a pleasing emotional reaction, equivalent in meaning to "This is real, solid stuff, not just empty philosophical verbiage".

Wouldn't you say that to go on making "commentaries about" theories that other people gave physical content to is neither very relevant nor very useful? When one understands that in any case, to "make a commentary about" a theory is actually to produce a different theory (but one that is different in a trivial and irrelevant way), one sees that he might as well admit, at the very outset, that all he can really do is to "make something new and different". To make something different (as v. Neumann, J. and P., et al. do) and then to use the same name (i.e., "quantum theory") is to confuse the issue. At the very least, it could be called "v. Neumann's, J. and P.'s, Gudder's theories of lattices, propositions, etc.", which are intended ultimately to be relevant to what is studied in physics (though as yet, they have not been). They could then note that in their theories are aspects that are vaguely similar to aspects of what has been called "quantum theory". They could then assert their belief that these aspects are the "essence" of what has been called "quantum theory" and that has been left out (e.g., single valuedness of wave function, Bohr's discussion of wholeness, etc.) will be ultimately seen to be irrelevant.

If they did this, then they would show, at least, that they are in some way, serious. Otherwise, one suspects that what is mainly behind their work is to use "emotionally charged" words that give a certain feeling of satisfaction. In modern physics, one of the most "charged" set of words is to say: "We are hard headed, clear thinking scientists, whose work is based on precisely defined formulae, and not on "wooly" or "fluffy" informal "philosophical" notions." (I talked to Jauch and saw that this is just the sort of language he takes pleasure in using.) This way of talking gives one a "solid and satisfying" state of feeling.

Best regards

Dave

Dec 11, 1968

Dear Jeff

Enclosed is a paper that will help explain what is meant by communication.[9]

I feel that your own paper is potentially relevant to communication. Its essential content is:

(1) Bohr and von Neumann could never have communicated, without basic changes in their points of view.

(2) This failure of communication was never perceived, and was indeed ignored.

(3) This led to irrelevant discourse, in which people talked without actually communicating, but believed that they were communicating.

(4) Attempts to "disprove" hidden variables of a disjoint nature were irrelevant, because there was no relevant informal language, that would allow one to discuss disjoint variables consistently in a "quantum" context. (For this reason, I feel more that there is latent confusion, even in the work of Kochen and Specker). Attempts to "disprove" disjoint variables in a quantum context are like attempts to "disprove" quadrangular triangles. These attempts are in some sense, worse than useless, because they introduce confusions into our thinking.

Either Kochen and Specker have no informal context at all in which case the proof is a piece of "pure mathematics", irrelevant to physics. Or else, it has, in some way, the informal context given in Bohr's language, except that this is described in a less consistent way than Bohr did. Now, in Bohr's language, the notion of a "disjoint variable" has neither meaning nor relevance. How is it possible for K. and S. to give it enough meaning and relevance, so that "disproving it" is more than mere confusion?

[9] See p. 232, n 7 above—CT.

In other words, I wonder if any attempt to "disprove" disjoint variables is not, in itself, necessarily a source of confusion and break down of communication. Once you adopt Bohr's context, even tacitly and implicitly, (as K. and S. probably do), then there is no point in even mentioning the words "disjoint variables".

On the other hand, as I showed in the 1951 papers, it is coherent to have "hidden" parameters ("operating behind the scenes"), provided that these are not disjoint.

V. Neumann (and others) introduced confusion by tacitly equating the words "hidden" and "disjoint". If they had paid attention to Bohr, they would never even have considered trying to do this. V. Neumann tried to rule Bohr out by bringing in his "classical observables", but of course, these lead to the "confounding of confusion" (as discussed in articles by me and by me and Schumacher). How do Kochen and Specker deal with this informal context, without at least tacitly basing their work on confusion?

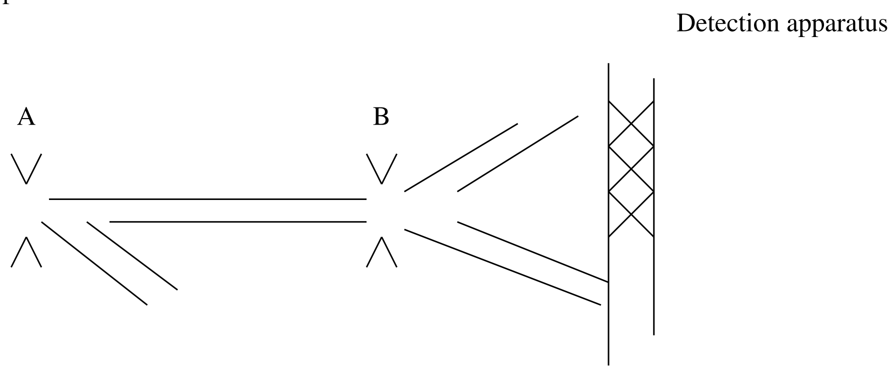
Best regards

Dave

Dec 17, 1968

Dear Jeff

This letter is mainly to comment on your proposal concerning the Stern Gerlach experiment.



Suppose that with the aid of [the] inhomogeneous magnetic field A, we split a beam of molecules and choose a particular spin, say $\frac{1}{2}$. Then we orient B at a small angle relative to A, so that the component of the hilbert space vector in the measured direction ($\bar{n}'$) is very small. That is, with regard to $\bar{n}'$, we can write

$$c_1 = 1 - O(\epsilon^2)$$
$$c_2 = O(\epsilon)$$

where $O(\epsilon)$ means "of the order of ϵ".

You then propose that it may take a long time, before "projection along the direction of $\bar{n}'$" is completed, for those cases, in which the ultimate result is $|c_2| = 1$, $|c_1| = 0$. You ask whether one could not carry out some kind of experiment, to "detect" this time lag.

The difficulty with your proposal is that our informal language is ambiguous, with regard to the meaning of those cases, in which projection is "incomplete". It gave clear meaning only to those cases in which λ is so large that projection could be regarded as "complete".

There are several kinds of ambiguity. Firstly, it is not clear how many degrees of freedom are relevant in the description that is the context of this phenomenon. If you will read my book, Quantum Theory, Chap. 23,[10] you will see that there is an ambiguity in what is to be called the "coordinates of the observing apparatus". For example, one could say that the position and the momentum of the atom are the "coordinates of the apparatus that measures the spin". Or one could say instead that they are among the coordinates of the "observed system". One must note that the division between "observed system" and "observing apparatus" is thus a purely formal one (which is however of course part of the informal form), and that it has no physical content at all. (It is only a feature of our mode of description, like the distinction between a signal and its meaning. A signal is its meaning, and the observing apparatus is the observed system.) However the von Neumann approach talks in terms of an actually irrelevant existential disjunction between "observing apparatus" and "observed system". It is as if one said that "signal" and "meaning" were two existentially disjoint "systems" that would be in "interaction".

When one wishes to apply our approach (in the Rev. Mod. Phys. papers) in this context, what is not clear is, first of all, how many degrees of freedom one should include. On the face of the matter, one would be inclined to include the coordinates (or momenta) of the atom, along with the spin. When the magnetic field "interacts" with the atom, it gives the latter "momentum" that "depends" on its "spin". I would say that it is at this moment that the "measurement" takes place (and not later, when the atoms separate in space, because of their different momenta). So, we write for the wave function

$$\psi = \sum_{in} c_{in} \psi_i \phi_n$$

where ψ is the wave function of the spin ($i = 1, 2$) and ϕ_n is the wave function of an atom with a fairly well defined momentum. Then we should have the contingent parameters, K_{in}.

Our equation becomes:

[10] Chapter 22 of the Dover, 1989, edition Bohm (1989)—CT.

$$\frac{dc_{in}}{dt} = \lambda c_{in} \sum_{jm} J_{jm} \left(\frac{J_{in}}{K_{in}} - \frac{J_{jm}}{K_{jm}} \right)$$

λ would be large only while the atom was passing through the magnetic field.

This procedure would be ambiguous in the following respects.

1. It does not determine the hilbert space vectors, ϕ_n completely. Presumably they would correspond to some partial definition of the momentum and some partial definition of the position. But there is nothing in our informal language which indicates the features of the experimental context that would actually determine ϕ_n completely.
2. It is not clear whether the coordinates of the detecting apparatus are not relevant in this context.
3. The ambiguity described in (2) becomes significant when you consider the case where λ is not large enough to bring about a "complete projection". In this situation, the "detecting apparatus" must be involved in the further process that "completes the projection". But our informal language has no terms in it to describe such a situation.

Our theory is then limited in its context of relevance, to situations in which "projection is complete in all cases". If we wish to go into broader contexts, we need a novel informal description. This cannot be deduced, inferred or implied, from the formalisms of von Neumann, J. and P., Gudder, Kochen and Specker, or anyone else. Nor can it be deduced or implied from Bohr's informal discussions. It requires a creative new step.

What I am led to ask is, however, whether this is indeed a relevant direction for further progress. The possibility of "incomplete projection" comes from our formal description in terms of a differential equation. This is one of the ways in which we are continuing the forms of classical dynamics. Do we really wish to commit ourselves to the assumption that the form of a differential equation is the most relevant feature of physics, and that all our informal language must be adapted to fit this form? Or is it not the case that the informal form is free and creative, and that the formal forms of equations must change, to adapt to new informal forms? No doubt, von Neumann and those who followed him see the creative possibilities solely in what is done with the formal forms of equations. But how do you see it?

Finally, with regard to your article, I would like to sum up the situation once again.

1. Von Neumann and his followers are deeply confused, because they assume that the "observing apparatus" and "observed system" are existentially disjoint and in interaction. Their assumption that "hidden variables" must likewise be existentially disjoint merely "confounds the confusion". Efforts to "disprove" hidden variables, based on such an assumption, are worse than useless. It is like a man who says "all triangles are quadrangular" and then tries to "disprove" the possibility of existence of triangles.
2. Others like Heisenberg and the majority of modern physicists tend to adopt a mixture (or "linear combination") of von Neumann's approach and Bohr's approach. This too is confused. Insofar as they adopt Bohr's approach, to "disprove hidden variables" is an irrelevant and meaningless effort. Insofar as they adopt von Neumann's approach, they are merely uttering irrelevancies, from the very outset.

The only relevant content of your paper is to try to reveal and explicate all this confusion, and general failure of communication. Otherwise, you will merely be combining, elaborating, and contributing to the breakdown of communication.

Best regards

Dave

Dec 19, 1968

Dear Jeff

This is just a brief addition to the letter of a few days ago.

If you seriously want to discuss the meaning of the work of Kochen and Specker (K. and S.), it is necessary to give an informal description appropriate to their formalism. Your suggestion that this is just "von Neumann's book," (which would be what one is supposed to mean by "quantum theory") is not adequate. For as pointed out in my previous letters (and in various preprints), von Neumann's book is informally a morass of confusion, on which nothing can properly be based. Nor would it make sense to use Bohr's informal language for this purpose, because in terms of the latter, the words "hidden variables" are totally irrelevant and meaningless from the very outset. Nor can we say that von Neumann's treatment is in any sense equivalent to Bohr's principle of complementarity. Indeed, the two points of view are irreconcilable, in the sense that each of them implies the complete pointlessness of the other.

What is needed in your article is first of all a thorough informal discussion of a theory, in which the term "phase space of classical dynamics" is replaced by "Boolean algebra" or "commuting algebra". To the best of my knowledge, this has never been done. In effect, mathematicians "wave their hands" implying that all this is obvious and trivial, and that only subtleties of the formalism are worth discussing. But I am willing to bet you that you will find that it is not at all easy to do this (although I think it is probably possible).

The next step is to take the non commuting algebra of quantum theory and give a corresponding informal discussion, enabling one to see that this algebra gives a relevant discussion of physical situations, and is not just a piece of pure mathematics. Once again, mathematicians seem to believe that by bringing in a few of the physicist's words (e.g., "quantum theory", "observable", "charge", "mass", etc.) they will ensure that they are "talking about physics". They "wave their hands" and piously hope that these words will make their conclusions relevant to physics. But as Bohr showed by example, it takes a serious discussion of the informal language to do this. It was done tacitly and implicitly for classical theory over the centuries. And a new theory requires new work on the informal language.

At this stage, I find myself puzzled. Without bringing in something like Bohr's language (which makes K. and S. just as irrelevant as is von Neumann) how will you do this? And if you start with von Neumann, you are bogged down in hopeless confusion from the very first step.

Assuming that you manage to pass this second hurdle (i.e., to give a consistent informal discussion of quantum theory that allows the term "hidden variable" at least potentially to have more meaning than "quadrangular triangle"), then you should discuss informally what it means to say that the embedding of the non commuting algebra of quantum theory in a commuting algebra is equivalent to what one means in classical physics by the term "hidden variable". It seems to me that this might be done by considering special cases in which non commuting algebras can be embedded in commuting algebras. (I am told that these exist, but that they violate certain further assumptions made by Kochen and Specker about the "quantum algebra".) Perhaps in this way, one could see that the notion of "embedding quantum algebra in classical algebra" is (in the physicists context) more than sheer confusion, and has at least potential meaning.

The situation here is that there is a danger in proofs of impossibility that one is just lost in confusion. In order to prove impossibility, in a coherent and rational way, it is necessary first to begin with a case where the property in question is in general possible. Then one shows that certain further contingent assumptions are not compatible with this result, so that it is generally possible but contingently impossible. The danger is that one will start with an absurdity that has no meaning, and is therefore already "impossible". To try then to prove the impossibility of such an "impossible" is confusion. Thus, if one starts with the tacit assumption that a "quadrangular triangle" is generally possible, and tries to "prove" that this assumption is not compatible with the contingent hypothesis of Euclidean geometry, such an effort is evidently worse than useless.

Similarly, it is very probable that the term "Hidden variables of a classically disjoint kind" may be just as confused as the term "quadrangular triangle". Mathematicians have great faith that this is not the case, but this faith is no substitute for a serious inquiry into the question. After all, in Bohr's language form, which is the only known consistent description of the "quantum" context, the notion of "disjoint observable" is of the same status as "quadrangular triangle". Mathematicians have tacitly ruled Bohr out as irrelevant. But this doesn't necessarily prevent them from uttering confused absurdities all the same (at least insofar as they imply that their formalisms are relevant in physics).

Best regards

Dave

Dec 27, 1968

Dear Jeff

It occurred to me that some of my previous letters may have been a bit too strong, with regard to v. Neumann and those who followed his work.

It remains true that this whole line is informally confused. However, the other side of the picture is that Bohr's coherent informal account of the quantum theory was very difficult to understand, at the time it was put out. This is probably why people like v. Neumann essentially ignored it and tried to develop their own accounts of the subject instead.

Now, if you write your own article by tacitly equating quantum theory with "what is in v. Neumann's book", you will find yourself entangled in some elaboration of v. Neumann's confusion. What you could do instead is perhaps to begin with what physicists were doing informally in quantum theory (i.e., using operators, eigenvalues, a certain probability interpretation, etc.). You could point out that there was no adequate informal theoretical discussion of what they were doing. The majority of physicists (who effectively ignored Bohr) were therefore treating quantum theory as an elaborate set of formal mathematical rules along with another elaborate set of rules for applying the formalism to experiment. (Thus, it was like a very elaborate version of the Rydberg–Ritz rules in spectroscopy, before the days of Bohr theory.) Your contribution would then be to show (using the formal arguments of Kochen and Specker) that this set of rules of computation plus rules of application was not compatible with an explanation in terms of hidden variables that are potentially disjoint from the rest of the universe.

You could also point out that v. Neumann and his followers (up to Kochen and Specker) were first of all, entangled in informal confusion, and then secondly were confused, in that even formally, they did not actually prove what they had set out to prove.

It would clarify your article if you made it clear that between Bohr's informal account and that of v. Neumann, there was no possible contact, but that most physicists tacitly believed that such informal considerations were of secondary or tertiary relevance, so that the question was never seriously considered, to any significant extent.

Finally, you could point out that my 1951 papers (and those of ours in Rev. Mod. Phys.) have no relevant relationships to the work of v. Neumann at all (including Kochen and Specker). So the main "theme" of your paper would be to reveal how little communication there has been between physicists who thought they were talking to each other. Best regards, Dave.

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