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Analysis of Some Arguments on Quantum Interpretation

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ABSTRACT. Canals-Frau [2] has provided a detailed analysis of some of the arguments in my book [1]. I point out that the work of Einstein-Podolsky-Rosen and Bell does not consist of philosophical speculation, but of analysis of quantum theory, and the authors have been able to make profound statements about the nature of the theory, and of the Universe itself. I also discuss Canals-Fraus views on the disturbance interpretation of quantum theory, on Einsteins career, on the need for an interpretation of quantum theory, and on the collapse of the wave-function.

RÉSUMÉ. Canals-Frau [2] a fait une analyse detaillee de certains arguments de mon livre [1]. Je fais remarquer ici que les travaux dEinstein-Podolsky-Rosen et de Bell ne sont pas des simples speculation philosophiques, mais des analyses de la theorie quantique. Les auteurs se sont averes capables de faire des remarques penetrantes au sujet de la nature de la theorie, et de celle de luniverse. Je traite aussi de la perturbation par la mesure en theorie quantique, du parcours professionnel dEinstein, du besoin dune interpretation de la theorie quantique, et de la reduction de la fonction donde.

1. Introduction

It is satisfying that Canals-Frau has found my recent book [1] interesting enough to write a substantial discussion [2] of some of its arguments. He recommends that the book should be read “critically” and I certainly have no argument with that. Nevertheless I do not agree with many of Canals-Fraus points and take this opportunity to reply to them. Many of his arguments concern not specifically the contents of the theories discussed, but more their nature, and he comments on the types of argument and analysis that may or may not usefully contribute to the development of science. However Canals-Frau also presents a specific scientific argument concerning the work of Bell (following largely a previous paper of his own [3]). I reply to both types of argument in the rest of this paper.

2. Types of scientific argument

Canals-Frau uses different terms for various types of physical statements or arguments. Here I briefly review these terms, and introduce a few other categories of scientific work. Naturally the discussion is limited to what is actually required for the remainder of the paper ; I do not attempt a complete account of scientific method !

Canals-Frau talks often of “physical facts” (pp .75-80), “empirical facts” (p. 78) or “physical statements” (p.76). Indeed Canals-Frau quotes de Broglie reminding us that “physics is an experimental science” (p. 77). Certainly nobody would dispute the fundamental importance of experiment in the sciences. Nevertheless, and to serve to make less pronounced Canals-Fraus distinctions between the different forms of scientific activity, it must be pointed out that, at the most basic level, “physical facts” in science are usually rather uninformative and not particularly meaningful. In quantum theory they are usually marks on screens or clicks on counters. Interpretation of these marks or clicks, again even at the most basic level, inevitably relies on the use, explicit or implicit, of theoretical concepts. It is scarcely ever possible to maintain that even the most accomplished and accepted theoretical development rests unambiguously on experimental evidence. Thus Canals-Fraus conception (p. 79) of “pictures… constructed on the results of measurements i.e. on physical facts” which “will be accepted by all physicists without discussion” must remain illusory.

Typical work in theoretical physics may consist of constructing some model for a physical system and analysing its behaviour using the laws of quantum theory or some other appropriate theory. Naturally such analysis will only be regarded as satisfactory if it agrees with experiment, and we may describe the theoretical work as “experimentally based”. We should, though, be very wary of describing a statement in the theory as a “physical fact” or a “physical statement”. Physical data which obeys classical theory, for instance, may be analysed in three completely different but equally valid ways, in Newtonian theory, relativity and quantum theory ; the statements in each analysis cannot be described as "physical" since they will differ in the three cases.

Two different types of such work may be mentioned. In one a physical system of equations is described in terms which would be broadly accepted, and then analysed theoretically ; the novelty of the work is the analysis. In the other a model is suggested or a "hypothesis" or "conjecture" made, and its consequences analysed using some broadly accepted theory. The latter procedure is, of course, quite valid. If the results of the analysis may be checked experimentally, then of course the hypothesis or conjecture will have been proved false if experiment and theory disagree. If it is not possible to check the results, then they remain to be considered as an interesting suggestion, but not, of course, to be taken as "fact" any more than the original hypothesis.

In contrast to this type of work, which may be called "analysis of physical situations", a rather different, but equally valid, and indeed particularly important type of theoretical work (central in much of the remainder of this paper) is what may be called "analysis of theories". It will typically be applied to particularly important wide-ranging theories - Newtonian mechanics, electromagnetism, thermodynamics, statistical mechanics, special or general relativity, quantum theory and so on. It aims, not to solve particular problems using the theory, but to study the theory itself, its general nature, and any general features of the predictions it may make. Analysis of statistical mechanics to see whether it predicts an arrow of time, or analysis of the theory of general relativity to see if it predicts expansion of the Universe, are just two examples of the exceptionally important topics that may be investigated in this way.

Whereas the methodology of "analysis of physical situations" is straightforward in principle (though, of course, usually difficult and challenging in practice), "analysis of theories" will be much more open-ended. Particular physical situations may be discussed, chosen with great care, not for their own intrinsic importance, but because they serve to demonstrate particular features of the theory. These may include "thought experiments". On this point I would differ from Canals-Frau. He states (p. 79) that a thought-experiment is one "we think we can perform" even though we have not done so. This would surely be a "suggested experiment". A thought-experiment is one that (probably) cannot be performed at least in the period in which it is discussed, but analysis of it provides useful or even crucial insights into the theory being analysed. Nobody would doubt the importance of the series of thought-experiments considered by Bohr and Einstein in their debates [4,1]. Of course it may be that the thought-experiment can be performed later in history with the further development of experimental techniques.

Next I turn to "speculation", again a word used by much by Canals-Frau (pp. 76, 77, 79) usually as "philosophical speculation" or "theoretical speculation". I consider this word should be used for ideas which go well beyond anything that is demanded by experiment. It may be that they solve some difficult theoretical problem, but only at the expense of causing considerable disturbance to well-established beliefs. Examples in the field of quantum theory might be the idea of many worlds, and the idea that consciousness has a part to play in quantum measurement theory. As Canals-Frau says (p. 79), speculations may certainly be worthwhile, for they sometimes lead in time to well-accepted theories. Until that time, though, their speculative nature must always be remembered, even by those who have put them forward.

3. The work of EPR

For Canals-Frau, the EPR paper [5] was a "philosophical speculation" (p. 74), a "theoretical assumption not confirmed by any experiment" (p. 75), a "purely speculative (metaphysical) work" (p.76), and "physically baseless" (p.79). It was not a thought experiment but a "so called thought experiment" (p. 74). It was based on a "hypothetical microphysical world" (p. 75), not on "physical facts" (p. 76).

I would start my own comments by pointing out that the EPR paper falls into the category described in the previous section of analysis of theories, in particular of course, analysis of quantum theory. As such I claim it was brilliantly successful, and may be described as demonstrating that the following three propositions cannot all be correct :

(1) Quantum theory provides correct predictions for all experiments.
(2) Physics is local.
(3) There are no hidden variables.

Since the view of Bohr and most other physicists, which Einstein did not accept, had been, at least implicitly, that all three propositions were certainly true, EPR had succeeded in its aim of wounding the established position, probably indeed fatally, though this entrenched position has certainly taken, and is still taking, an extremely long time dying !

Let us discuss the nature of the EPR paper a little more thoroughly. As a paper in the class of "analysis of theories" it is not essential that it studies a problem of great intrinsic interest. Rather it is essential that the problem is well-chosen to make the particular analysis possible ; EPRs problem was chosen brilliantly to this end.

It will be noted that, as set out above, the EPR analysis made no assumptions. This is, of course, a great strength in “analysis of theories” ; one wishes to make statements of the greatest possible generality concerning the theory under investigation, not statements that may or may not be true depending on circumstances or other assumptions or beliefs. Thus it is extremely difficult to understand Canals-Fraus frequent comments about “speculation”, “assumption” and so on.

It is true that EPR would probably not have considered that the experiment could actually be performed. For us, that makes it a “thought experiment”, for Canals-Frau a “so-called thought experiment”. Whatever the terminology, it may be said that quantum theory gives a clear prediction for the results of the experiment, and so it must be appropriate to study these predictions, which is what, of course, EPR do. Since the experiment had not been performed, it must not be assumed that the quantum predictions would necessarily be correct, which is why we must include proposition (1) above.

It may be said that our presentation of EPR differs from that of EPR themselves. First they talked about quantum theory being “incomplete”, which was certainly much closer to Einsteins taste than referring to a requirement for “hidden variables”. To him the latter suggested a rather trivial approach to making quantum theory acceptable ; he had more grandiose ideas [1]. As pointed out by Bell [6], though, hidden variables need not necessarily be simple in nature, and the term may cover anything Einstein would have wished to consider.

More significantly EPR assumed a “criterion” of “reality” rather than referring to “locality”. There has, of course, been an immense amount of discussion of these issues over the succeeding sixty-five years or so. There has been fairly unanimous agreement that EPRs use of the term “reality” was, at best, confusing. Commentators in the area would almost invariably use the idea of “locality” in their analysis, though they might differ on precisely what that word should be taken to mean. We do not take up that discussion here.

We do note, though, that EPR refer to reality as a “criterion”, in fact an assumption. This does not affect our argument above. To talk in terms of locality, we may either assume locality, and deduce that propositions

(1) and (3) above cannot both be true, or make no assumptions and say, as above that (1), (2) and (3) cannot all be true. The logic is the same.

It is true, of course, that Einstein did regard an assumption of locality type as inevitable. From this point of view, of course, the conclusion of the EPR argument is that either quantum theory is wrong, or it is incomplete, and either of these possibilities would have been highly acceptable to Einstein. It is also clear that the situation could be resolved by removing either of (1) or (3) above. If quantum theory did not give correct predictions for this particular experiment, clearly further analysis was unnecessary. Certainly this could only encourage theoreticians to suggest variations of EPR more tractable in practice, and experimentalists to perform these. This task was performed by Bohm [7] and Bell [8] from the theoretical side, and Aspect [9] and many others from the experimental side.

Again it was clear that, if statement (3) above were jettisoned, and hidden variables were allowed, it would be trivial to solve the EPR problem within the context of quantum theory and retaining locality. In the EPR paper, the fields in the two wings of the experiment are always along the same direction, and so one may allocate to each of the spins (from the moment of their conception) equal and opposite hidden variables giving directly the measured values. It would certainly have seemed natural to Einstein and to anybody else who thought about it that the same would apply in a more general context than the original EPR experiment - that one could retain quantum mechanics as exactly correct, and also maintain locality by questioning completeness (adding hidden variables). It is this view that Bell was able to challenge.

Canals-Frau mentions (p. 76) Bohrs response [10] to EPR. “Bohr”, he says, “has correctly (but perhaps not sufficiently clearly) pointed out that EPRs working hypotheses are not in accordance with the physical ground on which quantum mechanics is constructed”. It is difficult to know exactly how to interpret this remark. It might be suggesting that EPR do not fully understand quantum theory, in which case their conclusions would obviously be irrelevant. However this is not the case ; their analysis of the particular experimental situation they consider is quite correct. In practice Canals-Fraus statement appears to say no more than that EPR ignored the accepted beliefs of Bohr and others on the meaning of quantum theory. Indeed they did ; they challenged them and were able to show these accepted views were, in fact, incorrect.

There has been a long-accepted belief that Bohrs answer to the EPR paper showed that it was incorrect or irrelevant. It is very difficult to see how that belief could be maintained. Bohr essentially admitted the correctness of EPRs statement that a measurement in one wing of the EPR apparatus cannot affect the particle in the other wing. He wrote: “Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure”. This is again essentially to concur with Einsteins “reality” criterion, our concept of “locality” By this I mean that Bohr accepted the fundamental nature of these concepts, not that he necessarily believed they are upheld experimentally.

The most accurate comment on Bohrs response to EPR may be that he actually accepted it, but made use of language which suggested his analysis backed up his conceptions rather than those of Einstein. His use of the concept of “wholeness” in this context, was a considerable extension of its previous use where it expressed the holicity of measuring and measured systems, both located in the same region of space. In his response to EPR, the concept was extended to the two EPR particles, very much separated in space. These two particles, it was said, are not really independent, but constitute one coupled system.

This extended concept of wholeness is not without interest. However it cannot be said that it respects locality. Thus it may justifiably be said that Bohr responded to EPR by agreeing with it, that is to say agreeing with its demonstration that statements (1), (2) and (3) could not all be maintained, and removing (2).

Canals-Frau goes on to comment that: “Today, we can add that the ground on which quantum mechanics is constructed is physics pure and simple, since the quantum mechanics mathematical formulation correctly describes all physically possible situations and so it accounts for Natures micro- and macrophysical behaviour”. Two comments may be made on this. First, as shown clearly above, it was not part of EPRs argument that quantum theory would (necessarily) be proved wrong in the experiment they considered. They showed that one of (1), (2) and (3) must go. They almost certainly expected (2) to be maintained, and probably suspected that (1) was also correct, and that (3) was the most vulnerable.

Secondly, though, the fact that quantum theory has, so far at least, always agreed with experiment, does not mean that statements normally associated with the theory, statements, for example, which may have been used in arguments to make quantum theory appear plausible, are necessarily true. For example, upholders of quantum theory for many decades believed that it made the use of hidden variables impossible [11].

As is well-known, Bohm [7] showed that this belief was correct. This is why, of course, the type of work we called "analysis of theories" is so important ; it demonstrates what general statements about the theory may be made without fear of contradiction.

4. Bell

Canals-Frau makes many comments on the work of Bell similar to those on EPR. He says it is composed of "theoretical assumptions not confirmed by any experiment" (p. 75), "theoretical speculations" (p.75, 76), "working hypotheses and/or assumptions" (p.75), "a mere supposition without physical confirmation" (p.76) ; it is physically baseless (p.76) and "it has nothing to do with physics proper" (p.75).

As with EPR, it must be stressed that none of this is remotely true. Bells work [8], like that of EPR, is in the class of "analysis of theories". It aims to make as few assumptions as possible (in practice, none) and make general and fundamental statements about quantum theory, and further, indeed, about Nature itself. Typically for work in analysis of theories, it works with an experimental situation which may not be of great intrinsic interest in general, but which is chosen with great brilliance to illuminate exactly the points which the author wishes to focus on.

In Bells case, it is an extension of that considered by EPR. Whereas for EPR the fields in the two wings of the experiment are always in the same direction, Bell considers situations where they may be at general angles. With this added input, he is able to produce conclusions which go much further than EPR. In fact he is able to show that what must probably have been the preferred option of EPR is untenable.

The analysis of EPR would prohibit having all of (1), (2) and (3) above, but would allow a new grouping of (1), (2) and (3a) where we define (3a), essentially the negative of (3) as :

(3a) There are hidden variables (or quantum theory is not complete). Bell showed that this new grouping was not allowed. The most general theory using local hidden variables could not predict the experimental results that quantum theory predicts for the extended range of situations considered by Bell. If, then, one cannot have (1), (2) and (3), or (1), (2) and (3a), and recognising that (3a) is essentially the negative of (3), we cannot have (1) and (2).

At first sight it might be thought that this could directly provide a test for locality. If the experiments can be done (and, as is well-known, in essence they have been performed by Aspect [9] and others), then agreement with quantum theory would imply that physics was not local. But of course second thoughts tell us that no physics experiment can be exactly accurate, so the concept of experimental results (exactly) agreeing with the predictions of quantum theory cannot be realisable.

Bell, though, was able to substitute a much more robust test. It was essentially a test for a combination of (2) and (3a) rather than just for (2). Thus it could be said it was a test for local realism. If (2) and (3a) both held, then a certain way of combining the results from a small number of experimental tests of the type of experiment he considered, had to fall in a certain range. This was, of course, the famous Bell inequality. The quantum theory predictions lay well outside this range.

Experimental tests have established that the results lie well outside the range corresponding to local realism. (There remain loopholes principally due to low detector efficiency for photons. (See Ref. 1 and references therein.). For the purpose of this paper, I shall ignore these loopholes, and assume that the experiments have come down unquestionably against local realism.) The results are also very close to the quantum predictions in this region. It is very natural to claim then, that the combination of Bells work and Aspects experiment have demonstrated that the Universe does not demonstrate local realism

Canals-Frau disagrees very strongly with this conclusion. Part of the reason for this seems to be a belief that the fact that the inequalities are disobeyed in some way shows that Bell is wrong. The inequality "is not satisfied by Nature, but us indeed violated by the experiments performed" (p.75) ; Bells inequality "does not conform to Natures demand" (p.76). His belief is erroneous. Bell was not making a physical claim which was then tested by experiment and failed the test. He was able to make a statement about the Universe, and whichever result was obtained from experiment, the conclusion was important. If the inequality was disobeyed, one learned that we do not have local realism ; if it was obeyed, we learned that quantum theory is not universally correct. Bells work was not, as such, leading to either result (which is not, of course, to deny that he may have been expecting or hoping for one particular result).

Canals-Frau has further criticisms of Bells work. Bells inequality is "physically erroneous" (p.74) and "improperly inferred" (p. 76) ; his non-locality concept is "hypothetical, fallacious and experimentally unproven" (p. 74) and a "faulty deduction" (p. 75). In short his work constitutes a “fallacy” albeit a “subtle” one which is “concealed behind an (apparently) clever line of reasoning” (p. 75).

To attempt to justify his view, Canals-Frau presents two rather different arguments. The first is directly physical (footnote 1 on p. 77). Essentially the two photons in an Aspect experiment have from the outset related parameters, which Canals-Frau calls “initial phases”. These parameters determine which results will be obtained in each wing of the apparatus. In each wing, the result depends on the initial phase of the particle in that wing, and the setting of the polariser in that wing.

Canals-Fraus explanation, then, is precisely the hidden variable type of argument et that EPR hoped would solve the problem. But is also precisely the approach that Bell showed would not work, as discussed above. To agree with experiment in the general Bell case, the physical results in one wing of the experiment would also have to depend on the polariser setting or the experimental result in the other wing, and this contradicts locality. The hidden variables themselves are non-local. It seems clear that Canals-Fraus explanation does not work.

Canals-Frau presents a second argument against Bohrs analysis, this second argument concerning its logical basis. Canals-Frau described Bells logic as follows : “He probably thought : if a set of premises (the set of my assumptions) leads to a false conclusion (my inequality transgresses Nature), then one at least of the premises must be false.” Canals-Frau says that : “This reasoning is absolutely correct as a matter of **pure** logic, and of course he is quite right.” Yet strangely he says that Bells reasoning “is fallacious (in fact, it is a paralogism).”

Canals-Fraus argument is that there may be “an inadequate set of assumptions” by which he means “inadequate to describe the real **microphysical** situation”. It is quite difficult to interpret this remark. It will, of course, be freely admitted, as was stated in section 2, that in any argument which proceeds from assumptions to conclusion, the reliability of the conclusion can be no better than that of the assumptions, as well, of course, as depending on the correctness of the logic employed in the argument.

If Canals-Fraus point is along these lines, it is strange, though, that he says that it is Bells “reasoning” is fallacious. And it is still very difficult to obtain his meaning. Bells arguments, as described above, are remarkably simple in form ; they argue from local reality to his inequalities. If, as suggested above, we take for granted that the inequalities are disobeyed experimentally, then local reality has been ruled out. There really seem no other assumptions or "premises" to be debated.

It is true that Canals-Frau writes often as Bells conclusion of "locality". Certainly it is the case that the relevant experiments are tests of local reality, so strictly there is an additional assumption of reality to prove locality. But this has all been made very clear by Bell and others ; it may scarcely be interpreted as fallacious reasoning on Bells part !

5. The disturbance interpretation

Canals-Frau includes a section about the "disturbance interpretation" of quantum theory, contributing some arguments that, he says, are distinct from my own. In [1] I stated that the EPR argument demolishes the disturbance interpretation, a point with which Canals-Frau does not agree

To reply, I first remind the reader that, by the term "disturbance interpretation", I mean the idea that, in a measurement, the measuring system disturbs the measured system, and that *this explains the difficulties associated with the "measurement problem" of quantum theory.* It was always a loose explanation, not a detailed interpretation, and was used by Bohr to give some general physical backing to his more philosophical ideas of complementarity. Sixty years later it is still used, again loosely, when anyone "explains" the puzzles of quantum theory by remarking that the measurement disturbs the system being measured.

I consider it quite correct to maintain that the EPR paper did serve to demolish any grounds for accepting this interpretation. Quite clearly the measurement in one wing of the apparatus could not interfere with the particle in the other wing, or be responsible for the results of measuring its spin. Bohr himself admitted this point in his rejoinder to EPR, as quoted above i this paper.

Why does Canals-Frau not accept his argument ? Again he talks of EPR as a so called thought experiment which "concerns only theoretical speculations based on a hypothetical behavior of Nature". I repeat that EPR is a genuine thought experiment in the tradition of "analysis of theories". It is capable of probing subtle points in the theory, and in the EPR case I argue that it succeeds extremely well in so doing.

Of course one may agree in a broad sense with much of what Canals-Frau says about disturbance in measurement. In at least most measurements, disturbance between measured and measuring systems is an important factor. However a number of qualifying points may be mentioned. First, even in the Stern-Gerlach measurement which Canals-Frau discusses, there is certainly disturbance of the measured atomic particle by the measuring inhomogeneous field. However the disturbance is fundamentally to the momentum of the particle, not to the measured quantity, essentially the angular momentum. The measured quantity may, in fact, be undisturbed, or, at least, have a very high probability of being undisturbed, by the measurement. Indeed Paulis measurement of the first kind is precisely a measurement which does not disturb the measured system. Of course, many measurements are of the second kind, where the measured property itself is disturbed. A detailed discussion has been given [12].

Other subtle types of measurement include the negative-result measurement [13]. Here one may send a spin-1/2 particle into a Stern-Gerlach device, and provide a detector at the end of one of the paths. If one does not detect a particle in this detector, one may deduce the z-component of the spin, but without having interacted with it.

One should also mention the interaction-free measurement [14]. In this type of experiment one deduces the presence of a body at a point P by its effect on the trajectory of a particle. We may detect the particle at a position which it could not have reached if the body were not at P, although it is absolutely clear that the body cannot have interacted with the particle.

But as I have said, despite these provisos, disturbance is certainly an important topic in quantum measurement theory. The point being made in Ref. 2 is that, in itself, it does not explain any of its riddles. What is being criticised here is the common belief that saying that the measurement disturbs the system somehow removes all difficulties. Bohr often use such forms of words to back up his more rigorous idea of complementarity ; the idea was always rather unsatisfactory, but it was EPR who showed that it did not work at all.

6. Einstein and changing methodology

Before discussing Einstein himself, let me briefly reply to Canal-Fraus remark concerning Bohr at the beginning of his related section. He says that I do not believe any serious epistemologist who follows Bohrs struggles to unveil Natures behaviour would consider him a “once-for-all illuminator of quantum theory”. Yet I would argue that such a position has been almost universally taken. It has been accepted that the Copenhagen interpretation or complementarity presented by Bohr at Como in 1927 has been accepted as a once-for-all statement of the philosophical stance needed to comprehend quantum theory, a statement that may need elucidation, and detailed application in various directions, but not revision or correction. Such a stance is present in Bohrs response to EPR ; for all the turmoil it caused him, he is not prepared to admit it changed his views in any way. He wrote that : "It will be seen, however, that we are here dealing with problems of just the same kind as those raised by Einstein in previous discussions." [4] This position of the advocates of Copenhagen, not just that their ideas worked, but that any opposing ideas could only come from misunderstandings, was very harmful in the development of a more rounded position.

Let us now turn to Einstein. (A general reference to many of my remarks is the recent biography by Folsing [15]). It is generally accepted that his attitude towards physics, and acceptable strategies for its development, changed during his lifetime, from "What we can observe limits what theories can include" to "The theories we accept tell us what we can observe". It may be fair to describe that, as Canals-Frau does, as a transition from positivism to Platonism. However it seems to me that Canals-Frau may exaggerate the direct relationship between philosophy and physics.

First he comments that Einsteins early works "are characterized by an intimate contact with simple physical facts". If this implies that Einstein was working from experimental results, or making simple experimental predictions, that seems highly misleading. His ideas on special relativity came from what may be called thought-experiments in his own head that he analysed over a long period. Those on the quantum of radiation came from analysis of rather abstract theoretical expressions. Neither were particularly close to "physical facts".

Indeed it may be of interest that a "quasi-history" has developed in which special relativity came from analysis of the Michelson-Morley experiment, and the photon from study of the photoelectric effect [16]. Neither of these historical "facts" is true, but they may be said to add support to philosophies according to which theories inevitably emerge fairly directly from empirical results.

Canals-Frau mentions several of Einsteins achievements between about 1915 and 1925 ; I would suggest they may be characterised more as highly motivated examination and construction of theories rather than directly connected with experiment. Of course, Einstein would have accepted that, in the final analysis, theories are only of interest if they agree with experiment ; all physicists would accept that. But it is interesting that his most important early theories, special relativity and the photon, were neither a direct response to experiment, nor led particularly easily to conclusive experimental tests. (The photoelectric equation could be checked relatively easily, but it was not nearly so easy to convince physicists of the existence of the photon.)

I would claim, contrary to Canals-Frau that the EPR paper came very much into the tradition of these early papers. Both made great use of thought-experiments ; the arguments were fairly simple physically and mathematically, but extremely profound. (It would admittedly be fair to say that while this early work was “production of theories” or “analysis of situations”, the EPR is “analysis of theories”.)

I now turn to general relativity. The difficulty in Canals-Fraus account in which Einsteins work undergoes a degenerating change is that his theory of general relativity comes on the “degenerated” side of the fence by virtue of its methodology, though, as Canals-Frau says, it came before much work that was “undegenerated”. Thus it seems difficult for Canals-Frau not to be pushed into condemnation of Einsteins theory. He actually calls it “speculative”, a word which he usually uses to express serious disapproval, but actually “a speculative achievement”.

In contrast, I do not wish to describe Einsteins movement as from “good” to “bad”. The change from special to general relativity is quite marked, but it is not clear to what extent if any it reflected a change in Einsteins philosophical position ; it may well have just been the tractability of the problems. Certainly I am able to applaud Einsteins theory of general relativity, and its motivation, which is just as well because it is one of the greatest scientific achievements of all time. I do believe [1] that its success perhaps prejudiced him against work of other kinds, and that was certainly unfortunate.

7. Interpretations

Canals-Frau says that an interpretation is “a sort of picture of what is going on”, and adds that “if pictures are constructed on the results of experiments i.e on physical facts, they will be accepted by all physicists without discussion”. I would suggest he is being unduly optimistic. There is a very general common understanding of the mathematical structure of quantum theory, and also a pragmatic understanding of how to handle the measurement question. For many that is enough ; for others it is not, and they demand some explanation or picture for the procedures we follow, which we may call an interpretation.

The interpretation goes beyond the experimental facts, and thus has to be judged on other grounds - how many assumptions it introduces, how reasonable they may be, and so on. It seems to me very unlikely that, in the foreseeable future that any particular interpretation of quantum theory "will be accepted by all physicists without discussion".

8. The collapse interpretation

Canals-Frau does not approve of the collapse interpretation of quantum theory ; in this belief he is in common with many other good physicists. He supports, instead, a knowledge interpretation. In my book I sketched an approach to this as advocated by Peierls [17], though I do not particularly support a knowledge interpretation myself. Canals-Frau does not agree with my account and aims to "complete" it. In his account, there are two aspects in all measurements, first disturbance, and second noting of the results by a human being. The second does not affect the physical universe, except for the content of the observers brain, but is an essential part of a measurement.

Leaving aside the question of whether all measurements disturb the system being measured (as discussed in section 5), I would broadly accept Canbals-Fraus description, but do not see how it is to be regarded as an interpretation of the measurement problem. The measurement problem shows a direct conflict between the use of Schrodingers equation and what appear to be the "obvious" facts of measurement [1]. The collapse interpretation, whatever its infelicities, does succeed in solving the problem, and so, in some degree, do some of the other interpretations. (Whether they are internally consistent, whether they refrain from causing as big problems as they solve is, of course, another matter !) It is not clear to me that Canals-Fraus rather vague remarks constitute in any way a solution to the problem

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