Coexistence of several interpretations of quantum mechanics and the fruitfulness of scientific works

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Abstract

The coexistence of several interpretations of one theory is considered through the example of non-relativistic quantum mechanics. The problem considered is whether physicists manage to work properly in spite of the several interpretations. The criterion adopted is the possibility of re-using others' works for another research: this is called "fruitfulness of works". It is argued that such a fruitfulness is possible between works made in different quantum interpretations.

Quantum interpretations in conflict

Quantum mechanics (QM) is famous nowadays for having several interpretations. Here are some quotations which illustrate some oppositions:

> « People who have not grasped the ideas of [Bohm's] papers [...] are handicapped in any discussion of the meaning of quantum mechanics. »
> (Bell, 1987, p. 173)

> « The quantum theory of parallel universes [...] is the explanation — the only one that is tenable. »
> (Deutsch, 1997, p. 50)

The last quotation may be a synthesis:

> « They all declare to see the light, the ultimate light. »
> (Fuchs, 2002, p. 1)

With these interpretations, physicists understand QM in very different ways. So, perhaps they won't be able to understand each other and to work together.

Can physicists work properly?

I want to tackle here the general problem of the long-lasting coexistence of several interpretations of one theory. A first definition of an interpretation can be: its role is to give an image of the world, to say what the world can be like, according to the theory.

I want to study if there is a possibility of some “research as usual”, in spite of the existence of many different interpretations. In spite of the fact that physicists understand the theory in many different ways. So my question is: “Do scientists manage to work properly (or in a suitable way) in spite of the several interpretations?” (and how?) Of course, I will have to define precisely what is “to work properly”. I will take the example of QM (non-relativistic) because the debate on its interpretation is very lively nowadays in the literature (but I won’t have any technical discussion).

There can be another way of formulating the problem: several authors have claimed that research relies on some common framework. It is a paradigm for Kuhn (1962, 1977, 2000), a research tradition for Laudan (1977). My question amounts to asking if the interpretation belongs to this common framework, or if it can be diverse.

A last point: I don’t want to deny the existence of conceptual problems in QM, nor to minimize them. Indeed, it seems that these problems are the reasons why so many interpretations have been proposed. But it doesn’t prevent from asking a separate question: do people manage to work together when they have different interpretations? It’s a broader question of how the practice can be diverse, what kind of pluralism is acceptable or not.

Analysis of the situation: interpretations

In talking of “interpretations”, I have just followed the literature on QM. They are:
- Copenhaguen, orthodox (1920’s),
- Bohm, pilot-wave (1952),
- Everett, many-worlds (1957),
- van Fraassen, modal (1973),
- Rovelli, relational (1996)
- ...

The coexistence has last for several decades. The community of physicists, and of philosophers, is much divided on the right interpretation.

Analysis of the situation: structure of the theory

A theory is not only composed of an interpretation. I will consider three components in the structure of a theory (here, QM):
- a mathematical formalism,
- a minimal instrumentalist interpretation (which links this formalism with the results of experiments),
- an interpretation (in a broad sense, as I have been talking so far).

The first two elements constitute what I shall call the “predictive core”. It is the instrumentalist part of the theory. The interpretation is just a picture, on ontology, a fiction about it (which has to be compatible with it). In the case of QM, there is a consensus about this predictive core, but the interpretation is multiple. So all interpretations use the same instrumentalist part, and are considered as predictively equivalent.

Why should we adopt this conception of scientific theories? I don’t want to take a stand in any debate here. It just seems that physicists and philosophers are often implicity choosing this conception when they are talking about quantum interpretations. They say (1) that the formalism is well-established, (2) that the experimental use of it poses no problem, (3) its interpretation is not consensual. So here, the problem of coexistence of many interpretations is just formulated in the way is usually done in the literature.

Re-using other works

The question I'm interested in is the compatibility of several interpretations with usual scientific work. I have to specify now what is meant by "to work properly". I want to draw some conclusions from a very basic fact: a new research work (typically, a published paper) usually re-uses other works. These latter can be a theoretical law, a mathematical reformulation, previous experimental results, some experimental knowledge, etc.

For example, the experiment of Aspect in the 1980's (Aspect *et. al.* 1982) is considered as a major research result. It relies directly on many other works: Bell's theorem (Bell 1964), reexpression by CHSH (Clauser *et. al.* 1969), Bohm's proposal for the experiment (Bohm *et. al.* 1957), knowledge of the lab in optics... Another example: the scanning tunneling microscope invented in the 1981 re-used a principle discovered in 1928 (by Gamow, about alpha decay).

Here the question is how some new work, some progress, is possible. And it is often the case that a paper is actually re-used in other kinds of works, which are not just the continuation of it. This leads to a very intricate network. A "proof" of the re-use can be the tradition of citing other papers.

Fruitfulness of works is needed in research

I would like to introduce the concept of "fruitfulness of works". *Definition*: a work is fruitful when it is used (or is needed) more or less directly to produce another research result. (It is often a question of degree.) In fact, research progress needs this fruitfulness. Works have to be easily usable, compatible, so that there is one large community of research. And I can now specify what is to "work properly": for a physicist, it is when in his field of research, he can re-use any other work, when there is just one community of fruitful works. Why should I take this normative criteria? Let's go a step further in the analysis. I have talked of some "progress". I could put it more formally: I could say that the aim of science is to improve a "state of knowledge" (to be defined). I have shown that fruitfulness is the condition for new research, so it is a condition for progress, too. So, the normal situation will be when fruitfulness concerns the whole research community. This view could be specified.

Conditions for fruitfulness: a counter-example

I have given some examples of fruitfulness. I would like to give a counter-example, so that we can make an idea of the conditions it requires (I will state these conditions clearly afterwards). Imagine two competing theories. To take a contemporary example, let me consider the domain of quantum gravity (which is not entailed in standard QM I have been considering so far): there is the string theory, and the loop gravity theory. If a physicist manages to demonstrate a new theorem in the string theory, or predict a fact, this result will be absolutely useless for his competitor working with the loop gravity theory. And that is a good point, because they are competitors and they don't want to help each other. More generally, fruitfulness seems not to be possible between two different theories.

When there is a proliferation of competitors, research forces are being divided, and this is not optimal. Indeed, fruitfulness is impossible between works within the two theories, and global progress is weaker.

Conditions for fruitfulness: analysis

I would like now to specify the conditions on which fruitfulness is possible. I have adopted a three-part analysis for the theory:
- the mathematical formalism,
- the minimal instrumentalist interpretation,
- the interpretation(s)

Do some elements have to be common for a fruitfulness to take place?

I adopt a traditionnal viewpoint and I divide (published) works in three categories:
- theoretical calculations (and elaborations),
- experimental results,
- comparison between experiment and theory

Let's see what are the constraints so that they can be fruitful. For the first kind, the new work must be expressed in the same formalism (or translated in an equivalent one). It is no problem in QM, for example the equivalence is well-established between ondulatory or matrix formalisms. As far as the experimental results are concerned, there is no real constraint for a fruitfulness. In fact, an experimental result can almost always be re-used, even by two competing theories. For the last work (theoretical-experimental comparison) fruitfulness is possible provided that the minimal instrumentalist interpretation is the same. So, finally, it is the "predictive core" which has to be the same so that there can be a fruitfulness.

No condition for fruitfulness has been found for the interpretation. Interpretations, if found in the papers, should be unnecessary or translated. That's exactly what says David Deutsch, at the end of his famous article of 1985 on quantum computing (p. 113):

> « I have [...] assumed Everett's ontology [that is: Many-World Interpretation]. Of course the explanations could always be "translated" into the conventional interpretation »

So, I should be able to exhibit cases in which interpretations are different and in which there is still some fruitfulness. That's what comes now.

Examples of fruitfulness in spite of different interpretations

Deutsch and quantum computation

First, I would like to continue with the example of Deutsch. It is absolutely right that his article has been understood and re-used by everybody, with any interpretation. A good example is Anton Zeilinger, who made some experimental work on quantum algorithms, the domain invented by Deutsch. And Zeilinger is a proponent of the Copenhaguen interpretation. But he has carried on Deutsch's work on an experimental ground. So, it would be wrong to think that there is one research community for each interpretation. There is just one large.

Bell's inequality

My second example will be Bell's inequality (Bell 1964). It is a theoretical result that predicts some correlations in a given experiment. Its interpretation is not possible in a classical framework; so it is central in the debate on quantum interpretations. It has been used by every interpretation. They all have their own explanation of the result. This is due to the fact that the argument is expressed in a mathematical formalism (and the final argument is about probabilities). If there are some elements of interpretation in the original paper, it is possible to abandon it, to stick to the formalism, and to use another interpretation. The same holds for the result of Aspect's experiment.

Bohmians

I would like to say a word of the Bohmian interpretation. It is well-known that it uses a formalism which is quite different from the usual one (but still equivalent). I have argued that the interpretation could be in a paper. Is this other formalism, which is linked to the interpretation, used in papers? And are people able to translate it, so that there can be fruitfulness between works in different formalisms?

It is more simple. Roughly, only the standard formalism is used in papers. Even by physicists who adopt the bohmian interpretation. The best example can be Jean Bricmont, a belgium physicist, who is a famous advocate of Bohm. He has spent all his brilliant career in statistical quantum physics, and he has always used the standard formalism.

So, what about the bohmian formalism? In fact, the translation is only potential. People know that it is possible to express any standard result in the bohmian formalism. All they have to do is to establish the translation. There are several physicists working on it (and on the internal consistency), for example Sheldon Goldstein.

Conclusion: why do the interpretations coexist?

I addressed the question of several interpretations of a theory, on the practical viewpoint of the productivity of research and of new works. I suggested that the right concept should be the fruitfulness of works. I hope I have shown that it is possible even with several interpretations. Scientists manage to work properly in spite of the various interpretations. There is no sub-community of research with a particular interpretation. Usual research is not threatened or hampered by the coexistence. Indeed, QM is a very succesfull theory, and still in progress.

Finally, I would like to suggest a more general thesis on pluralism, which could be investigated. I have argued that the interpretation of a theory can be multiple. Are there other elements in science on which there don't need to be a consensus? My intuition would be that fruitfulness of works should be the limitative criteria.

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