The Quantum Mechanical Worldpicture and Its Popularization[1]

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0 Introduction

I think it is a common experience to be impressed, and even fascinated, by articles in the newspaper's Science Section. Reading such articles is the obvious way to stay at least reasonably well-informed. But I think it is also not unusual to be a bit disappointed by such articles. Speaking for myself — but I believe the experience is generic — the more familiar I am with the discussed topics, the likelier I am to be a bit dissatisfied. Subtle but important differences are neglected by the science reporter; he forgets to mention that certain results have only a tentative status; and he seems to be somewhat confused about the details of the physical explanation of the phenomena he discusses.

Obviously, it would not be fair to discard the work of science popularizers because of inaccuracies like that. The articles written by science reporters are not meant for the experts in the field. Their purpose is to convey the broad outline of new developments and new ideas to those outside the circle of experts: this aim can only be reached if certain details are omitted. No harm is done by inaccuracies as long as the global picture is not distorted.

This may appear a trite remark. But it nevertheless presupposes something which is not trivial, namely that there is a picture which has to be represented as accurately as possible. There are different ways in which this condition may be not fulfilled. First, there may be various schools of thought among scientists, and a lack of consensus. In that case a science journalist cannot do much better than sketching the disagreement and describing the various points of view. More problematical, however, is a situation in which there even are no clearly formulated points of view shared by substantial numbers of scientists. The popularizer is then left to his own devices and has to rely on more or less individual points of view.

Such a situation may seem hypothetical. But it actually occurs, so I will argue, in what is often considered the most fundamental of sciences, namely physics. Most physicists have no clear conception of the interpretation of their most basic theory, quantum mechanics. They are largely unaware of the exact nature of the problems in giving a detailed and consistent account of the physical meaning of the theory; and if they are

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aware, they often don't care very much. Only very small numbers of researchers have given serious thought to the interpretational problems of quantum mechanics, and have expressed more or less detailed points of view. As can perhaps be expected from the statistics of small numbers, the diversity of opinion is large. Very different ideas have been put forward, none of them supported by great numbers of physicists.

It is only natural that in those circumstances popular writers will be drawn to points of view which exhibit "spectacular" features. Indeed, an important tool in popularization consists in making scientific results exciting. A standard technique to achieve this is to make use of descriptions that are both easy to understand and unusual. This leads to a preference for using terms and examples drawn from everyday experience — because of the expectation that this will make the presentation more accessible and easier to understand. At the same time, popular writers often try to entice the reader by appealing to analogies with strange, exotic occurrences. It is an interesting topic in itself whether this style of popularization is to be recommended in general, and whether there are good alternatives. It seems clear that inadvertent use of the technique in question can distract from essential points and lead to misunderstanding — as has been argued long ago by Susan Stebbing (Stebbing, 1937; see also Dieks, 1994). But that is not what I want to discuss here. I only want to focus attention here on the fact that this common way of popularizing naturally leads to an emphasis on ideas that can easily be paraphrased in everyday language and at the same time are unexpected, unusual or exotic.

If the foregoing is right, it is not surprising at all to find that the popular literature on quantum mechanics pays a great deal of attention to views in which the possible role of human consciousness, possible links with Eastern mysticism, etc., are stressed. Rather, it is surprising to find that the ideas expressed in this literature are rejected out of hand by the great majority of physicists, whereas there is no clear alternative accepted by that same majority. Every physicist is assumed to agree that the views maintained by Capra and Zukav, to name only some of the best-known exponents of this literature, are wrong (Capra, 1975; Zukav, 1979); but what has to be said instead remains obscure.

The position defended in this paper is that the criticism to the effect that writers like Capra and Zukav misrepresent the situation in the foundations of quantum mechanics is not justified. If there is occasion for criticism at all, it should certainly also be directed at the majority of physicists who are rather nonchalant about the interpretation of quantum mechanics. If there was more interest among physicists themselves in the problem of how to associate a consistent world picture with fundamental physical theories, there would be more guidance for popular science writers and in all probability less cause for the complaint that these writers do not understand what physical theories are about. But as we will also point out, this lack of serious professional interest in interpretational matters is not just an unfortunate coincidence; it seems to be related to the very nature of empirical science.

1 The early history of quantum mechanics

The theory whose interpretation is at stake here was proposed, in its essentials, in the years 1925–1926. This episode constituted the culmination of several decades of intense work. At the end of the 19th century it had become clear that the accepted theories of mechanics and electromagnetism faced severe problems in explaining a number of well-established experimental facts. Here one had not just a puzzle of which it could easily be assumed that a solution lay waiting in some hitherto unknown detail still to be added to the existing theoretical framework. The difficulty was of a fundamental sort. Experiments revealed an element of discontinuity in nature: for example, atoms emit light in discrete spectral lines, and the atoms themselves apparently only exist in certain discrete states. But the theories of classical physics are of an essentially continuous nature and it is difficult to see how they could possibly cope with these experimental facts.

In 1900 the German physicist Max Planck made a first step on the road to a solution: in a rather ad hoc way he introduced an element of discontinuity in his theoretical calculations. He remained unclear, however, about the physical significance of his own manoeuvre. In 1905 Albert Einstein was the first to supply a concrete physical picture: he proposed that light, and other electromagnetic radiation, did not consist of continuous waves (as assumed by classical electromagnetic theory) but had a discrete, particle-like, structure.

A next important step was made in 1913 by the Danish physicist Niels Bohr. Bohr proposed that the discrete "quantum" element introduced by Planck and Einstein had wider applicability: it also played a role in the structure of atoms, in such a way that only a series of discrete states, configurations of the atom, can exist.

Einstein's and Bohr's ideas constituted the beginnings of research programmes rather than being full-fledged theories in themselves. Einstein's "light particles" could explain some facts; but the wave nature of light was manifest in other experiments, so contradictions threatened. Similarly, Bohr had to assume that transitions occur between the various atomic states, in spite of his presupposition that no intermediary states exist; this, and other strange features of Bohr's theory, also appeared to signal inconsistency.

In the 1920s two research programmes had come into being. One, connected with the names of De Broglie and Schrödinger, was inspired by Einstein's light quantum suggestions. The leading idea in this programme was that if light waves can be particle-like, particles in their turn may possess wave aspects. Schrödinger thus attempted to find a general theory in which everything is wave-like and particles are described as more or less localized "wave-crests." This led him to the formulation of "wave mechanics," early in 1926.

The other research programme, associated with the names of Bohr and Heisenberg, among others, aimed at formulating a generalization of Bohr's 1913 theory of the atom. In view of the difficulties in framing a visualizable picture of atomic transition processes, efforts finally concentrated on finding a calculational scheme, an algorithm, for predicting the frequencies of spectral lines and the energy levels of atoms. This culminated in Heisenberg's "matrix mechanics" in 1925.

Schrödinger was soon able to show that the two theories shared the same mathematical structure and could therefore be considered to be equivalent from an abstract, mathematical, point of view. But his hope that this would mean that wave mechanics, with its visualizable and intuitively satisfying wave picture, would prove superior from a physical point of view, was not fulfilled. Bohr and Heisenberg became victorious because it soon became clear that a literal interpretation of the wave picture was not tenable on technical grounds. One important reason for this verdict was that waves always spread out; but we never see anything like that happening to particles.

This brings us to the point where the early discussions about the interpretation of quantum mechanics started. The situation is truly remarkable because on the one hand an algorithm had been found that was tremendously successful in making predictions about observable phenomena, but on the other hand was completely opaque with respect to the nature of the physical processes behind those observable phenomena.

2 Niels Bohr and what followed

Niels Bohr was a central figure in one of the two lines of development that eventually led to the formulation of modern quantum mechanics: the line that went from the atomic model of 1913 to Heisenberg's matrix mechanics. The element of discontinuity inherent in quantum mechanical phenomena was a prime focus of attention in this research programme. Already in Bohr's 1913 model the discrete states of atoms played a role of paramount importance; and this feature became only more important in the matrix mechanics, which was essentially a calculational scheme to handle discrete energy levels. The notion that all matter is built up from discrete lumps, particles, seems to fit in naturally with this approach. But Bohr, perhaps in contradistinction to most of his fellow-workers, was also very sensitive to the other side of the coin. The wave theory of light had achieved enormous successes, which should not be completely dismissed in the new situation. And though Schrödinger's literal interpretation of the "quantum waves" proved to be untenable, Bohr felt that at a deeper level there was truth to the idea that matter was wave-like.

The tension between the two points of view — the particle and the wave picture, to put it briefly — was lifted in a new doctrine that Bohr developed in the years 1926 to 1928 (Bohr, 1985). Bohr proposed that a physical object, an electron for example, sometimes shows characteristics which fit in with a discrete, particle-like, picture; but at other occasions shows wave-like features. The crux of the matter is, according to Bohr, that both aspects of the object never come to the fore simultaneously. It is a question of either/or: in the circumstances in which particle-like aspects manifest themselves, wave aspects are absent, and vice versa. It is the type of experiment that is performed that determines the kind of features that will be exhibited. Though a full characterization of the object in question requires both the wave and the particle aspects, it will never be the case that both kinds of aspects will show themselves in one and the same situation. In this way contradictions are avoided.

The revolutionary idea of this doctrine of complementarity is that physical objects do not possess a fixed set of properties, which is given once and for all. An electron (taken as an example of a physical system, to illustrate the idea, which is quite general) must be described as a localized particle if it is subjected to one type of experimental conditions; whereas it must be described as a wave in other circumstances. This should be compared and contrasted to what is envisaged by classical physics. A classical particle always has a definite position, velocity, rotational motion, etc., whatever the circumstances it finds itself in. We can form ourselves a complete picture of what a classical particle is, on the basis of the set of these properties it always possesses. By contrast, a quantum "particle" according to Bohr cannot be pictured independently of a context. Different, complementary and mutually exclusive, contexts are needed to obtain a full description of what a quantum object is.

The above ideas were elaborated by Bohr into a view in which the role of our classical language was stressed. All descriptions, says Bohr, have finally to be given in terms which apply to macroscopic measuring devices; these are the terms which make communication among physicists possible, and basically the only terms we really understand. This gives the "classical language," i.e. the language of everyday experiences refined by classical physics, to some extent an a priori position. Nevertheless, justice has to be done to the peculiarities of the quantum domain. This is achieved, according to Bohr, by restricting the simultaneous applicability of classical concepts. In quantum mechanics objects cannot be characterized by the same combinations of concepts as in classical physics. Each experimental condition is associated with a particular selection of applicable terms, so that the joint application of complementary terms is excluded.

This characterization-in-a-nutshell perhaps already makes clear how radical Bohr's proposal is. Bohr essentially says that physical systems do not possess properties completely of their own, regardless of the circumstances in which they find themselves. The applicability of concepts to physical systems depends on the type of measurements performed on it. Though this doctrine of complementarity neatly manoeuvres between the Scylla of particles and the Charybdis of waves, the context-dependence of properties made Bohr's ideas hard to swallow for some of the physicists who partook in the debates in the 1920-1930s. In his famous cat-example Schrödinger attempted to demonstrate that certain familiar objects, like cats, possess properties independently of whether a measurement is made or not. A cat is either dead or alive, he argued, even if contained in an insulated box to which no observer has access. (Actually, it is doubtful whether this kind of example is really harmful to Bohr's position — Bohr never spoke of measurements as necessarily involving human observers; the presence of macroscopic objects recording the behaviour of micro-systems is enough.) Einstein and co-workers published in 1935 a more incisive criticism: the famous Einstein-Podolsky-Rosen argument. In it they consider a physical system which in the far past has been in interaction with another system, and as a result has become correlated with that other system, but in the meantime has traveled very far away from its counterpart. Bohr now seems to maintain that the properties that can be assigned to the system in question can depend on measurements that are performed on its far-away counterpart. According to Einstein this is absurd: a physical system which is not in interaction with any other system, and is far away from all other objects, surely has properties of its own, and doesn't "feel" anything of what goes on at the position of the enormously distant other particle.

In spite of Schrödinger and Einstein's objections and its very unusual and partly obscure aspects, Bohr's way of reconciling particle and wave aspects won the day. Indeed, with the help of it, it is possible to give an admittedly strange, but consistent account of many (thought-) experiments; without being obliged to assume that quantum mechanics is only a tentative theory and without the need to resort to deeper layers of reality ("hidden variables") not described by quantum mechanics. These two points, the prospect of a logically consistent quantum mechanical account plus the "self-sufficiency" of quantum mechanics, probably carried most weight in the very small circle of (eminent) physicists who delved into these questions. At the end of the 1930s the majority of these physicists shared the opinion that the doctrine of complementarity — "the Copenhagen interpretation" — was at least in principle adequate for solving or avoiding all interpretational problems. This view found its way into the textbooks and became a "paradigm," in the sense that the great majority of physicists took it for granted that although there might be difficult paradoxes, the interpretational problems in principle were solved.

As we will see, however, the further history shows that the supposed recipe of treating interpretational problems had not really found its way into the physics community. We have a clear case here of "general acceptance" of a point of view, without real awareness of the consequences involved.

3 Bell

In 1964 John Bell re-opened the case by showing that any theory which would lead to the same empirical predictions as quantum mechanics cannot be of the type Einstein had hoped for (Bell, 1964). That is, it is impossible to assume that physical systems have properties of their own, which are only affected by local interactions, i.e. by interactions with other systems in their immediate vicinity. (Remember that Bohr had argued that objects only have properties by virtue of the fact that they are part of a greater whole — even in cases in which this whole has enormous dimensions; Einstein opposed that proposal with ideas that are much closer to classical physics.) All theories that comply with these assumptions necessarily lead to predictions which at some point differ from those made by quantum mechanics.

Given the fact that the Bohrian ideas had become the "standard interpretation" of quantum mechanics, and were mentioned as such in virtually all textbooks, one would be inclined to expect that Bell's results were hailed as the definitive proof of the correctness of the standard approach. However, the opposite is true. Bell's paper caused amazement, and inspired experiments destined to check whether the quantum mechanical predictions were in fact correct (in experimental conditions of the kind discussed by Bell, in which a discrepancy with more classical rival theories would exist). Not a few physicists expected that these experiments would show shortcomings of the quantum mechanical scheme and would thus open the way for theories in the spirit of Einstein. But the experiments — which involved admirable feats of experimental technique — eventually only confirmed the quantum mechanical predictions.

This did not at all settle the discussions, however. Bell's work had given the interpretational issues a concrete form, which appealed more to many a physicist than the rather abstract and philosophical way in which the discussion between Einstein and Bohr had been conducted. As a result of Bell's investigations and the ensuing experimental work a small number of physicists and philosophers started to look anew at the problem of the interpretation of quantum mechanics. Up to the present moment no consensus has been reached among these specialists.[2]

An important point to keep in mind is that all these discussions, both the original ones between the founding fathers of quantum mechanics and the more recent ones, were and are carried on in very small circles. The great majority of physicists kept and keep at a distance, being only faintly aware of the issues in question.

4 Two paradoxes of science

In order to put the situation into perspective, two things should be observed. First, it is no peculiarity of the problems surrounding the interpretation of quantum mechanics that only small numbers of researchers are actively involved. The field of physics, like other fields of learning, has been divided up into countless specialties, in which only experts are really at home. Anyone who is not part of the limited circle of cognoscenti can only aspire to be in the position of a having a general overview, in which broad outlines are visible but details are hidden. Evidently, such knowledge will not be the result of the scientist's own investigations but is accepted on the authority of experts.

To some extent this situation is paradoxical. Philosophy of science texts often point out that a decisive difference between science and non-science is that in science nothing is accepted on the basis of authority; that everything is open to critical discussion and possible disproof. But of course that view has to be qualified. In practice almost everything a science student learns he has to accept on authority. Critical investigations, tests, etc., only play a role among experts, whose reports are trusted by the remainder of the scientific population. The testability of scientific results is a testability-in-principle, reserved to members of sub-disciplines.

Although this fact surely complicates the picture of the scientific enterprise, I think it would be a grave exaggeration to consider it a refutation of the notion that science distinguishes itself as being an endeavour in which critical analysis is important and in which a priori dogmas cannot permanently maintain themselves.

2 This is as true in 2007 as it was when this paper was written.

Anyway, normally most physicists will have a general notion of the situation in subfields which are not their own. Usually there will be differences of opinion about latest developments and details among the experts, but the "outsider" will discern what is common to the different approaches, and what is well-established.

A second and more significant paradox is that on the one hand scientists frequently claim to be motivated by the desire to know the fundamental processes, mechanisms, etc. which lie at the basis of everything that happens in our universe, but on the other hand more often than not have a tendency to be skeptical about the value of questions pertaining to interpretation, and other matters without immediate significance for observable phenomena. It should be noted, however, that this actual state of affairs fits in with how at least some philosophers of science, namely empiricists, think about the aim of science. Empiricist philosophers defend the point of view that it is the business of natural science to formulate regularities that enable us to make reliable predictions on the level of the observable — and that the success in this enterprise is the only really compelling criterion by which scientific theories can be judged. Attempts to work out this notion in the past generally led to naive pictures of science according to which reference to unobservable entities could, and perhaps should, be completely avoided. Such views are at odds with scientific practice, which is full of reference to entities and processes that cannot be directly observed. In addition, analyses made by philosophers of science in the last couple of decades have shown that the elimination of "theoretical terms" is impossible on systematic grounds. However, a modern sophisticated form of the empiricist idea has recently been formulated by Van Fraassen (1980). According to Van Fraassen scientific theories need not avoid to refer to unobservable things; but still the aim of science is only to discover "empirically adequate" theories. This is meant in the following way: a scientist who accepts a theory need only commit himself to the belief that the observable consequences of the theory are true. He doesn't need to subscribe to what the theory says about the unobservable; in other words, skepticism on this point, or disbelief, does not disqualify a researcher as a scientist.

The philosophical dispute between empiricists and their opponents; realists (who consider it the aim of science to discover a true description of the world), still rages and it is difficult to see how a decision could ever be reached in this abstract battle about the aims of science. But as already observed it seems that scientific practice is in accordance with Van Fraassen's views, in the sense that empirical adequacy is the only thing that all scientists actually require of an acceptable theory. That does not mean that other features than empirical adequacy do not play a significant part in scientific practice. As also acknowledged by Van Fraassen, *pragmatic* elements are very important in science as it actually functions. Such elements include the motivation for scientific research afforded by the world picture associated with a scientific theory. Though it may not be the aim of science per se to formulate world pictures, world pictures and interpretations of theories are important for scientists to motivate their research and to make decisions about the direction of future investigations.

Moreover, we already mentioned that the modern empiricist ‡ la Van Fraassen countenances *agnosticism* as an alternative to scientific realism. (Realism entails that acceptance of a theory implies belief in everything a theory says, also about the unobservable). The agnost suspends judgement about the unobservable consequences of a theory. This clearly only makes sense if the theory does make pronouncements about unobservable states of affairs; if the theory identifies underlying processes and mechanisms that are responsible for observable phenomena. That theories can be taken literally, as being about such underlying mechanisms, is a central point in the modern empiricist position and distinguishes it from earlier empiricist approaches that proved to be untenable.

Summing up, even modern empiricists think of scientific theories as providing a picture of what goes on behind the scenes of the observable although the empiricist only believes in what the theory says about the observable. Scientific theories can and should be taken literally, as Van Fraassen puts it; if a theory speaks about unobservable entities this should not be taken as merely symbolic, as a handy way of connecting observable phenomena. The assumption thus is that a theory comes together with an interpretation, in the sense of a picture that extends to the realm of the unobservable. An important role is envisaged for such interpretations, even if it cannot be said that it is the aim of science to provide a world picture that has to be believed in by anyone who accepts its theories. Interpretations make theories concrete, tangible and interesting; they motivate the scientist and give direction to his research.

5 The role of interpretations: an internal tension

What was just said implies the existence of a certain tension within science; a tension that is intimately related to the very nature of science. On the one hand scientists are motivated by the idea that they are discovering what the elementary building blocks of the universe are, and the processes by means of which these elementary systems interact; on the other hand they have the feeling that the only thing which really counts is the success theories have in predicting observable phenomena. The latter side of the coin is reflected in the fact that a physicist whose research effort is completely directed at developing consistent interpretations of theories, and thus at finding out what is the nature of the invisible world described by those theories, will soon get the feeling that he is not engaged in a "core-activity" of physics. On the organizational level the same thing finds it expression in that only very small numbers of researchers are actually allowed the time to devote serious attention to interpretational issues.

Summing up, notions about the interpretation of theories are indispensable in practice: physicists study not directly observable "elementary particles," for example, and are motivated by the desire to uncover their properties. But on the other hand the touchstone for success is the prediction of new facts, on the observational level; and questions relating to interpretation in this context are often dismissed as of secondary importance or as of merely philosophical interest.

6 The interpretation of quantum mechanics

The above does not automatically lead to a serious dilemma. If the interpretation of a theory is something straight-forward, there is no need to devote research effort to interpretational issues. But in quantum mechanics the situation is different. In fact, it is exceedingly difficult to develop a consistent interpretation of the theory. This feature, combined with the fact that foundational/interpretational work has a problematic status as a physical subdiscipline, gives the situation its peculiar characteristics. The problematic status of the subdiscipline is not just that only a small group of physicists is active in these issues, and that the great majority of physicists is in the position of onlookers from a distance. As pointed out before, that is something which can be said of many specialties. We have attempted to indicate that the matter is rather that interpretational studies are not clearly recognized as a core-activity genuinely belonging within physics.

Let us look at these points in more detail. The difficulty of developing a convincing interpretation of quantum mechanics can easily be understood. First, the rigorous results which have been achieved preponderantly have a negative character: they are "no-go theorems." No-go theorems show the impossibility of certain interpretations, but do not themselves provide a new interpretation. For example, Bell's theorem demonstrates that a "local" theory in which physical objects possess well-defined properties is not possible. More generally, the outcome of foundational work in the last couple of decades has been that interpretations which try to accommodate classical intuitions are impossible, on the grounds that theories that incorporate such intuitions necessarily lead to empirical predictions which are at variance with the quantum mechanical predictions. However, this is a negative result that only provides us with a starting-point for what really has to be done: something conceptually new has to be found, different from what we are familiar with. It is clear that this constructive task is a particularly difficult one, in which huge barriers (partly of a psychological nature) have to be overcome. Apart from finding a general and consistent interpretational scheme, there is the difficulty of "getting a feeling" for it; to attain a position in which one understands the interpretation. What does it really mean, for example, to say that a quantum object does not possess properties of its own but, perhaps, only acquires properties in a measurement context?

It is true that Bohr has made bold steps forward in his attempt to replace the everyday and classical conceptual scheme by something new. But it can hardly be said that his proposals are the final words on the subject. Apart from the fact that Bohr's writings are notorious for their complexity and lack of clarity, Bohr's work on the interpretation of quantum mechanics does not have the character of a polished whole but rather that of work in progress. The main intuition is clear, but the consequences of its application to many concrete cases is not. This is true, for example, for the Einstein-Podolsky-Rosen case and the associated analysis of the notion of locality. The transition from the micro to the macro level is another example of an issue that awaits further elaboration. Connected with these problems is the fact that Bohr's explanations have a rather loose connection to the mathematical framework of the theory. One would like to have an interpretational scheme which gives an unequivocal physical meaning to the mathematical expressions in the theory, in a precise and general way.

The sheer difficulty of the situation, in which the only thing that is certain is that familiar concepts do not work, surely is one central element of the particular situation in quantum mechanics. This by itself is probably already sufficient to explain that very different, sometimes exotic, suggestions have been made about how to proceed. But the situation is certainly much aggravated by the already-mentioned fact that work in the interpretation of quantum mechanics does not have immediate experimental and technical consequences. In practice this means, in accordance with our remarks about "the aim of science" in the previous section, that very few scientists have the opportunity to do serious full-time work on these problems — something which facilitates a certain amateurism that can occasionally be detected in publications dealing with the subject.

It is not so strange then, that the great majority of physicists stay at a distance from the discussions about the interpretation of quantum mechanics. The proposals which are made by insiders are often difficult to understand, and anyway have no immediate practical consequences. As we observed, this does not only apply to the suggestions which have been made over the last couple of decades. Also the "Copenhagen interpretation," though dubbed the "standard interpretation" in most textbooks, remained uncomprehended and without real influence on the world picture of physicists.

There is still another significant aspect of the situation. As commented on in the foregoing sections, being immersed in a world picture is part and parcel of scientific practice. Scientists derive motivation and inspiration for further research from global ideas of how the universe fits together. As quantum theory is the reigning physical paradigm, one would expect that physicists have a quantum mechanical worldview in the back of their minds when pursuing their research. However, it has become obvious by now that that is not the case. Still, the need for a physical worldpicture makes itself felt. The reaction of most physicists is to substitute a kind of common-sense, quasi-classical, picture for the quantum mechanical one that really would be needed. Most physicists think of quantum objects as very small copies of everyday objects, and in effect use the conceptual scheme of classical physics. Of course, they know that something is wrong here, and that a consistent use of classical ideas will lead into trouble. But that problem rarely presents itself in an acute form. There is always the mathematical formalism of quantum mechanics with which calculations that lead to observational consequences are made; the pictures associated with the theory do not really intrude into those calculations. Because the mathematical formalism is consistent, no inconsistencies will be encountered when making predictions. Paradoxes and bewilderment only occur if one wonders about *how* the calculated and predicted experimental outcomes can be realized by natural processes.

The physics community thus does not possess a definite notion of what to do with the problems in the interpretation of quantum mechanics, and even is in the unclear about the exact nature of these problems. It operates with a "common sense" interpretation which basically is wrong (and of which one knows that it is wrong). Nevertheless there are very strong feelings about some of the interpretational ideas which have been proposed. There is a general intuition that ideas which deviate considerably from the general outlook of classical physics cannot be right. "Exotic" interpretations like the many-worlds interpretation, or interpretations in which the conscious human observer plays an important role, are often ridiculed and are at best treated as not being completely serious. This strongness of feelings about the characteristics of acceptable interpretations fits in with our earlier remarks about the importance of interpretations in the practice of physics. But, of course, criticism of ideas which are deemed unworthy of notice is not very convincing if the alternative used by the critic is not consistent itself and if no attempt is made to remove the inconsistency.

7 Conclusion

The peculiar situation in the interpretation of quantum mechanics is that there is no single generally accepted worldview that is consistent with the mathematical formalism of the theory. Among the great majority of physicists there is only the general feeling that any serious interpretation should not be too distant from common sense notions; and for want of something better one operates with pictures derived from classical physics. There hardly exists a professionally recognized physical subdiscipline in which interpretational studies are a main concern. Inside the small group of cognoscenti one faces the tremendous task of framing a new conceptual scheme that should replace the familiar ideas that are impressed upon us by everyday experience. It is no wonder that in that situation very divergent suggestions are made.

The fact that interpretational problems do not receive much serious professional attention does not appear to be a coincidence. It seems related to the very nature of empirical science, in which empirical success is the ultimate goal and interpretation has at least "officially" the status of handmaiden. The paradox is that unofficially, i.e. when not the ultimate aim of science but rather concrete scientific practice is at stake, the worldpicture associated with quantum mechanics does play an important role; and that physicists have quite outspoken opinions about the general ideas of most interpretations.

For the science popularizer the implication is that there is no "scientific picture" which he can attempt to represent with as little distortion as possible. It is not surprising in those circumstances that many popularizers are attracted to the more outlandish proposals that have been made. Given this situation it seems an almost moral obligation for the physics community to pay more systematical attention to the "pragmatics" of science and to develop clear and distinct ideas about the interpretation of quantum mechanics. One should not reject unorthodox proposals out of hand, and complain about the attention they receive in the popular press, without giving serious thought to consistent alternatives.

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