HOW MANY TRUTHS ARE WE RESPONSIBLE FOR?

Mladen Pavičić

Contributors have been asked to "maintain above all the postulate of truth" in their criticism of the dominant paradigms of science and its responsibility. But reading scientific articles written over the last 60 years and their philosophical and historical interpretations, one cannot help asking: "Which truth?".

It is a well-known fact that one can cast phenomena within various rival theories all of which reproduce the same data. And as theories themselves define the very object under consideration, the fact of existing rival theories could determine, by and large, the way in which the science (and also technology - but not in the near future) will develop and its future role within society. Thus at the same time this fact brings up the problem of the responsibility of science, or, better said, scientists. What this means I shall illustrate (trying not to be too general) mostly on well discussed quantum mechanical controversies.

There are a number of rival quantum theories: the orthodox (Copenhagen, Princeton or some other version)[1], phase space[2], fuzzy phase space[3], hidden variables[4], stochastical[5], stochastic electrodynamical[6] and statistical[7], to mention just the most developed elaborations[8]. All of them reproduce the same data and all of them are by now unrefuted. Should we therefore conclude that quantum mechanics is "showing signs of aging, i.e. of a style of growth drawing closer and closer

to the styles of such other fields as philosophy and the arts... Pluralism and theoretical disagreement already so deep and so rapidly increasing in physics... - might these not all be signs ... of full maturity, the style of growth of disciplines no longer in their youth?"[9]. It might be, if the pluralism developed and increased with time. However, these rival theories have existed from the very beginning, i.e. the late twenties and early thirties.[10] Nevertheless, even today, only one of them is officially and generally accepted, taught at universities, widely known and most developed - the orthodox one. Does this mean that we still have, if not empirical, then at least semantical grounds for rejecting all the other theories? No![11] But attempts which should apparently lead us to such a conclusion have constantly been repeated throughout the history of quantum mechanics (an untrained reader can ignore the following intended part of the paper):

- Statistical theory, for example, has frequently been "disproved" with the claim that there is nothing beyond the statistical-like laws of quantum mechanics, i.e. that quantum mechanical statistical laws are not proper statistical laws. However, this is an ad hoc and quite unwarranted conclusion, for, we can apply proper statistics to individual quantum objects either by differentiating abstract, statistical objects from real ones[12], or with the help of "propensities"[13], or by limiting its interpretation to preparatory conditions[14]. In fact, there is an important implication of the statistical approach which explains why it was "met with strong opposition by physicists and particulary by advocates of the Copenhagen interpretation"[15]. Namely, the statistical interpretation allows the existence of observables which cannot be included in the existing formal apparatus and this automatically implies the incompleteness of quantum mechanics.[16]

- Further, the phase space formulation is to be rejected because it does not, it is claimed, use only positive definite probabilities but also negative and complex ones, what is physically unacceptable. More precisely, on the basis of Wigner's proof, it has been stated that positive definite marginal distributions do not exist. This, however, is not true, for, Wigner formulated the proof only for bilinear forms, and non-bilinear forms (of the distribution in the wave function) with positive definite marginal distributions do exist.[17] Also, the phase space formulations are usually wider than standard formulation and, restricted to the standard domain, have not only positive definite marginal probability distributions, but also reproduce all the results identically.

- All that has just been said about the phase space formulation is still more valid for the fuzzy phase space formulation in which marginal distributions are always positive definite.

- Stochastic as well as stochastic-electrodynamical theories are to be rejected because they assume some kind of underlying vacuum structure, but can offer no empirical confirmation of it. This again is not quite true, as we need not suppose such a structure. Stochastical electrodynamics, for example argues as follows: "the electron [or any other charged particle - the neutron does not cause adequate problems] is <u>not</u> isolated; [it] is bathed in a stochastic radiation field [of all the other charges in the universe]"[18]. If we, on the other hand, do suppose some vacuum structure, we are faced with the dilemma: is it more natural that a moving quantum object has no trajectory at all, or that it is influenced in its movement by some underlying vacuum structure?

- Finnaly, the hidden variables theories should be dismissed,

> because hidden variables are theoretically and experimentally disproved. This again is far from true. For, the so-called "disproofs" do not at all refer to the hidden variables as formulated by the advocates of these theories[19]; they do, however, refer to those formulated by the orthodox opponents (Why? For the purpose of being "disproved"?).

A proper decision has thus not been reached to date, although many onlookers, including many physicists, think it has been. (All the refutations cited above were not as clear cut in the minds of their propounders and we can even say that they were deeply convinced of the objectivity of their research. However, such a conviction is a matter of self-censorship and prejudice[20] which we will go into in greater detail later.) Of course, it would not be economically viable to develop all the theories at the same time, but what demands a closer examination is the physical community's intolerance of the alternatives and criticism. The more so as the rival theories presuppose mutually quite incompatible characteristics of a quantum object (according to some theories it has trajectory - according to others not, according to some it obeys causal laws - according to others not, according to some it interacts with "some" subquantum structure - according to others not, etc.) - i.e. they presuppose (or rather define) semantically and ontologically "rival" quantum objects (e.g. electrons). And if physics as a science strived for truth it would take the alternatives into account, i.e. the physicist would bear them in mind, at least when exploring new possibilities. Instead, physics embraces the orthodox theory which just covers what is measurable and <u>postulates</u> that there is nothing beyond. Is it only a simplicity principle or something more?

P. Forman has tried to explain the situation, i.e. the choice

physicists have made, in the following way: "suddenly deprived by a change in public values of the approbation and prestige which they have enjoyed before and during World War I, the German physicists were impelled to alter their ideology and even the content of their science in order to recover a favorable public image. In particular, many resolved that one way or another, they must rid themselves of the albatross of causality."[21] This explanation can help us to understand why this particular choice has been made and this particular interpretation embraced - apart from an objection: physicists have not altered their ideology. For, it was not the same physicists who "altered" their views. The quantum physicists were new physicists who were searching for a new place and positions in the new social structure. To achieve this this they were expected to be homogeneous and reliable, i.e. to unify their subject and to adopt a language which would characterize them. In fact, such self--definition of a group through a special language of their own is a very old way of inner control:

- We can find this in Plotin:

> "[I]n any one science the reduction of the total of knowledge into its separate propositions does not shatter its unity...; in each distinct item is latent the entire body of the science...: when a man has become this unity in the best, he is in that other realm"[22]

- we can trace it throughout history (sometimes it is called "mysticism" - but the only mystical thing about mystics is that their language is incomprehensible to others),

- and find it still in Wittgenstein's "private language",

- as well as among any other political or professional group today.

It is the same with quantum mechanics.

However, for lack of space, I will now just give a few hints and a general overview later on.

Bohr (1934): "In our description of nature the purpose is not to disclose the real essence of phenomena but only to track down ... relations between the ... aspects of our experiance".[23]

"As early as 1934, the American Association for the Advancement of Science discused the possibility of making nuclear weapons."[24]

In March 1935, Einstein and his collaborators submitted for publishing the article[25], in which they have shown that quantum theory is essentially physically incomplete (this argument is still fully valid and the so called Bell's counter-argument has nothing to do with the main physical objection in the article[26]).

In April 1935, "Science service of Washington supplied The New York Times [with] the article[27,28] in which the readers are informed that "Professor Albert Einstein will attack science's important theory of quantum mechanics"[27] and that he concluded its incompletenes although not its incorrectness.

In May 1935, Einstein's article[25] was published. Numerous objections were immediately raised - the best known one (but nevertheless unsuccessfully[28]) by Bohr[29]. In short, he claimed that we cannot experimentally check Einstein's doubts and consequently should not take them into consideration.

In 1936 Einstein replied: "To belive this is logically possible without contradiction; but, it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception."[30]

"During the winter [of 1939] Bohr and John A. Wheeler of Princeton University collaborated to produce a theory of the mechanism of nuclear fission that is still the basis for research in nuclear energy. Bohr returned to Denmark in 1940."[31]

"After December 1941, Einstein was carefully kept insulated from research on the A-bomb - 'in view of the attitude of people here in Washington who have studied his whole history'... Einstein got wind of what went on, enough to worry greatly about a postwar

weapons race. His plan was to inform and and rally scientists in major countries..."[32]

"Niels Bohr himself hurried in Princeton to swear Einstein to silence in order not to 'complicate the delicate task of the statesmen'."[32]

"From 1943 on, Bohr was in the United States where he served as an adviser to the the physicist working on the development of the atomic bomb. Bohr was opposed to the use of the bomb [?!?], however, and he never worked on it directly [?!?]."[31]

J.A. Wheeler (1949): "[A]ware as always of political realities - [Bohr] emphasized again what he had often said before: "How could Western Europe possibly have remained free and at peace after World War II if America had not had the atomic bomb?""[33]

After World War II the physical community continued to consolidate itself (1958): "For each new generation of students there is less resistance to being broken down before they feel at home with quantum ideas."[34]

Bohr: "[I]n physics we carry out propaganda. When we believe we have seen something more clearly than others, we try to spread our new insight, and that is propaganda ... I had to argue for two years with Heisenberg and Bloch before I could convince them that the new quantum theory depends altogether on correspondence... It was also hard to make them and others accept the notion of complementarity."[35]

However, work on hidden variables, stochastic and statistical theories was also continued. Hanson (1959): "It has become fashionable amongst philosophers of science to attack the 'Copenhagen Interpretation' of quantum theory as being either unrealistic (Mehlberg), unreflective (Bohm), or unnecessary (Feyerabend)."[36] (Bohm, of course, was not a 'philosopher of science' but a physicist; only, he was temporarily freed of his community; namely, he was suspended from the position of assistent professor at Princeton University and expelled from the United States by Senator J. R. McCarty. Up until that moment he was a supporter of the Copenhagen Interpretation). It became increasingly evident that only he-

terodox theories can encourage any further development beyond quantum mechanics. Hanson (in the afore mentioned article) finds the way out by simply declaring them to be non-existent, and adds that "no one should think that because most quantum physicists are unperturbed by the type of question brought to prominence by Bohm, that therefore they are unreflective, resigned, Berkleyan, computer-ridden predicting machines."[37]

At the same time (1959) in Soviet Union statistical and stochastical interpretations supported till then, fell into disfavour, and the orthodox one was embraced.

In the last two decades a lot of heterodox ideas have been considered (some of them leading beyond orthodox quantum mechanics, e.g. stochastic electodynamics). Simultaneously, a lot of theoretical and experimental disproofs of hidden variables appeared in journals. But, what is particulary entertaining about them is that they do not refer, at all, to the hidden variables theories as formulated by the advocates of these theories.[38]

Not realizing that the situation just mentioned is also a kind of schooling the untrained onlooker is still tempted to overestimate the role of reason: "I suspect, however, that most physicsts ... are influenced more by the tradition in which they are schooled than they are by these rather recent and sophisticated arguments. That tradition is the deeply positivistic legacy of Bohr and Heisenberg, from which contemporary science as whole is struggling to free itself. I am not much worried that my philosophical colleages will be seduced by positivist considerations coupled with insubstantial reasons, for we are differently schooled."[39] Unfortunately he has no reason to be optimistic in connection with his "philosophical colleagues". Namely, on a "metalevel", i.e. in the philosophy of physics we find just the same situation as in physics itself.

Official philosophy of science (e.g. Lakatos) just describes the situation in a basic science: "If we have to rival research programmes, and one is progressing while the other is degenerating, scientists tend to join the progressive programme."[40] We can "stick to a degenerating programme and try to turn it into a progressive one"[40], but it is not at all

clear how we can do this when already in 1953, 5 out 225 colleges in the United States got the same amount of money as all other 220 put together, for scientific contracts[41]. In time this situation is exacerbated[42], and it is no wonder that until very recently (in Italy) none of the numerous proposed experiments, which might decide between the rival quantum theories has been financed and performed.

Apart from the official approach we do find heterodox views within philosophy of science as well as within basic sciences, but they are handled in the same way as heterodox basic theories are. One example is Feyerabend who maintains that there is, historically, no connection between the truth and acceptance of observation sentences (the latter being always analitically true within a particular theory) and that the ground for the acceptance or rejection of a theory is a fundamentally irrational procedure. His view has recently been declared to be "bizzare, implausible and unattractive"[43]. This has not, of course, been objectively confirmed but something else has:

> In a study of seventy-nine eminent scientists, Warren O. Hagstrom stressed that a key element of internal social control in science is peer recognition. It is essential to keep in mind that the risk of ostracism or isolation is dificult to bear for scientists who depend greatly on a somewhat distant and abstract form of peer approval... Receiving adequate recognition is less likely when traditional role behavior is violated... anything that isolates a scientist tends to lower his reputation and productivity...[44]

Thus we are close to understanding both sides in a coherent way. The ground for the acceptance or rejection of a theory is a fundamentally "irrational" procedure, as Feyerabend says, but only if by "irrational" we mean "non-rational" with regard to a theory itself, i.e. so far as the acceptance or rejection of a theory is not based on the internal structure of the theory, but on external social factors. On the other hand such a point of view is

evidently "bizzare, implausible and unattractive". For what is stranger than to employ progress to preserve the status quo; what is, for a scientist, more unbelievable than the possibility that he himself is just a bureaucrat employed in a Department for truth codification; and what is, for the state, more unattractive than Feyerabend's conclusion that the separation of state and church must be supplemented by separation of state and science, that most recent, most aggresive, and most dogmatic institution!"[45], for in this case, if the analogy is to be complete, we should have to change the state as well. However such a "science" would not be a science in today's sense of the word and consequently is out of scope of the present considerations.

So far we have indicated that the institution of science, being a state's means of ensuring its own technological basis, is externally conditioned in order to fulfil the task most economically. In this way our cognition of Nature is but a by-product of scientific production in the course of which, this particular cognition is selected out of many possible ones. This means that our truth is also constantly being selected out of many possible truths and if we are to choose truth for the postulate mentioned at the begining of this paper then such a postulate would have to include all possible truths. This would, however again imply a new kind of science.

And in the end are we to take science as being responsible for itself? Of course,not. Here, again Lakatos was quite right claiming that science, as such, has no social responsibility. For this would immediately imply that it would be in opposition to somebody, and that "somebody" can only be the state. But how on earth can a "state department" be opposed to the state. And what about scientists themselves? They, in principle, could be responsible but only if they were aware of social influence on their subject, and of the general consequences of such a situation. In fact one is tempted to conclude that "from World War II on 'scientists are being forced to consider the social repercussions of their work'. But the characteristics of early career development and personality formation show the built-in

obstacles to the easy fulfilment of that hope."[44]

There is one thing we could still do. We could try to reconstruct our own scientific and general historical possibilities analyzing not only the official theories and truths of particular periods but those that have been abandoned and suppressed as well. This would help us to understand our own cultural development much better.

Mladen Pavičić,
Department of Mathematics, Faculty of Construction Sciences,
University of Zagreb,
Fakultet gradjevinskih znanosti, Zavod za matematiku,
Janka Rakuše 1,
YU-41000 Zagreb,
Yugoslavia.

NOTES

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