A DEMARCATION IN THE ONTOLOGY OF THE NATURALISTIC WORLD-VIEW

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Within the realm of a naturalistic world, we are used to thinking that our ontology is given after completing an over-all conceptual scheme into which disordered fragments of raw experience can be fitted and arranged. However, this is valid only on the assumption that concluding on parts of a conceptual scheme does not differ in kind from the considerations which determine a reasonable construction of the whole. If it does differ we have to broaden our scope and move from the theories themselves to some wider point of view. Such an attempt was undertaken by Kuhn, Popper, Feyerabend, and other representatives of the so-called Weltanschauungen (W) approach when they started to treat science, its objects, and its environment as a whole. Nevertheless, the opinion was recently expressed, e.g. by Suppe,[1] that the W analysis is ceasing to be accepted as an adequate understanding of science. The W view is now being attacked, often vigorously, and ultimately, in Feyerabend's exposition, accused of rejecting or accepting a global theory on a fundamentally irrational basis, and of denying that a legitimate aim of science is to find out how the world *is*. Altogether, "this view is bizarre, implausible and unattractive";[1] must be rejected for the sake of "a demarcation of scientific progress from intellectual degeneration";[2] and we should instead, following Lakatos, be concerned with science as actually practised and as giving correspondence between theories and reality. On the other hand, strangely enough, the proponents of the W approach continue to develop their theories which are still very much discussed. Clearly there is something wrong here. One cannot first find something to be very significant and influential which would later be invalid, if nothing has changed in the meantime. Science has not changed.

So what has? To say that it is the technological and economical situation which has changed, would seem, at first, to be a completely misplaced statement. But let us proceed more gradually. What we should stress above all is that the opposing approaches only apparently contradict each other. In fact they are just speaking about different things. Kuhn is right in claiming that a "normal" science consists of unique theories and that in a "normal" science there are no rival schools—but only if we judge it according to the standard textbook, university lectures, popular books, etc. Popper is right in holding that the constant and simultaneous proliferation of a variety of theories, subjected to falsifiability by crucial experiments, is responsible for the growth of scientific knowledge—but only if we add that not only falsified theories are rejected. Feyerabend is right in maintaining that there is, historically, no connection between the truth and acceptance of observation sentences (the latter being always analytically true within a particular theory) and in maintaining that the ground for the acceptance or rejection of a theory is a fundamentally "irrational" procedure —but only if we understand scientific objects as not existing-in-themselves but being defined by a particular theory and if we understand "irrational" procedure as that which is simply indefinable within a particular theory, i.e. determined by external factors. And, finally, Lakatos is right in disapproving of the W approach—but only if he does it from within a particular chosen theory (which can be one of many equally possible theories of the same phenomena) of a particular science, taking only empirically confirmed parts of its theoretical structure as relevant.

What, then, connects the preceding approaches and why do they appear to contradict each other. Looking more closely we can resolve each of the numbered disfavoured models into two parts: a "regular" one and a "painful" one. On the other hand the official model also

consists of two parts: a "regular" one and a "suggestive" one. "Regular" parts of all models coincide. Namely, when we restrict various models to the common level of elaboration and application, then empirically equivalent syntactical and semantical parts fuse together into a "regular" one. In the aforementioned models it is a part which elaborates the development of each particular scientific theory while paying attention only to the formal structure and empirical confirmation of the theories. The "painful" part of disfavoured models is their integral and proper part, but unutterable from within the "regular" one or unprovable on the basis of bare empirical data. An example of this is a non-scientific fact responsible for the acceptance of a theory (Feyerabend) which is, on the other hand, considered unimportant for the application of the theory—or a simultaneous existence of two different theories reproducing the same data. This part is disturbing and painful for both supporters of an official model: financiers and scientists. A scientist must not wander around. Technology demands the monolithism of sciences. A financier demands the economy of scientific researches. How can one on top of this accept a casual or at least polyvalent structure of sciences? Is it not "bizarre, implausible, unattractive", and distracting? But to be sure, the representatives of official models have also added an *ad hoc* and external "suggestive" part, often rendered as 'progress', which is rather different for each particular case and will be exampled later on.

The above elaboration so far has been based on a specific scientific field: the philosophy of science—a kind of meta-theory. But it is also valid for the basic sciences themselves. So, we will give two more concrete examples of the elaboration on both levels. The first one is an extremely bare form of the philosophy of science as presented by Wittgenstein in his *Tractatus*. The second example is the official quantum mechanical world-view in contradistinction to other existing ones.

The *TLP* offers a model of that part of our world to which natural sciences are applicable ("T-world").[3] Constituents of the T-world are those formal concepts of natural sciences which satisfy some definite conditions imposed by particular scientific theories.[4] Consequently all the corresponding elementary propositions are true,[5] and the corresponding logic is a tool for manipulations with elementary propositions in an empirical domain.[6] On the other hand, within such a formal language of a natural science, the sense of the very same science is unutterable, or, which is the same, the sense of the world—the T-world—has to lie outside of it.[7] Now, the "regular" part of the *TLP*, within which Neopositivists tried to cast the complete work at the time, was the elaboration of the statement that "only connections that are subject to law are thinkable" (*TLP* 6.361). Further, Neopositivists added a "suggestive" part: the progressive antimetaphysical dictum: "*TLP* 7", in hope of annulling the "painful" part: "even when all scientific questions have been answered the problem of life remains untouched" (*TLP* 6.52).

In quantum mechanics (QM) there are—contrary to the usual belief, even by physicists—a lot of existent, elaborated, and consistent theoretical formulations (f.), which are mutually irreducible one to another and which have completely different interpretations, but all of which reproduce the same standard observable values. The main groups are: Copenhagen f. (the one presented in most textbooks, university lectures, laboratory work, popular books etc.), statistical f.,[8] hidden variables (HV) f.,[9] QM on fuzzy phase space,[10] QM in phase space with negative and complex quasi-probabilities,[11] and stochastical QM.[12] But if there are so many varieties of valid theories why is only one of them officially accepted? Because for every possible, now conceivable application (A-bomb, medicine, power plants etc.) the statistical features of quantum phenomena are all we need. Well, but why, then, is not the statistical interpretation the leading one? Statistical interpretation inclines to the conclusion that there is something we still do not know and cannot register by means of current experimental procedures, and in any case claims that the standard formal f. of QM do not adequately describe an individual quantum object. And then? Then scientists would search further on a quite general level, reexamine the theoretical structure etc., and this is not economically viable.

But who can control all this? That is the point. The scientists themselves. In the following way: They adopt the Copenhagen interpretation which has put forward a "suggestive" but unprovable golden rule: let a statistical description be considered a complete and exhaustive description of individual quantum objects. In the light of this they "disprove" all other formulations as follows: 1. statistical f. has no sense for there is nothing beyond statistic-like laws and consequently they are not proper statistical laws—but they do not mention that this is an *ad hoc* conclusion; 2. stochastical theories have to be rejected because they suppose some kind of underlying vacuum structure but can offer no empirical confirmation for it—but Copenhagenists do not mention that they also cannot offer an empirical confirmation for their additional hypotheses; 3. f. in phase space has to be rejected because it does not use positives definite probabilities—but they do not mention that this f. is wider than standard QM f. and, restricted to the standard domain, has not only positive definite probabilities, but also reproduces all the results identically;[13] 4. HV should be dismissed because HV are theoretically and experimentally "disproved"—but they do not mention that the so-called "disproofs" do not refer, at all, to the HV theories as formulated by the advocates of these theories.[14] And finally we should mention that, so far, none of the numerous proposed experiments, which would decide between the theories, has been financed and performed.

So we have come back to the old ontological question: "On what there is". Namely, basic physical concepts which determine the quantum object as, e.g., state, transition, trajectory, causality etc., are quite differently defined in one theory than in another (or even completely absent in some, as, e.g., trajectory and causality in Copenhagen f.). The consequence of such a situation is that with adopting a particular theory we also change the very object under consideration. Apparently there would be a lot of ontologies determined by what an old phenomenalist would have called "the one and same set of objects", i.e. theories would multiply the objects if we stayed in Lakatos's naturalistic world. However, applying our model, we can easily recognise the formal part of standard f. of QM (to which all the other f. are reduced if limited to orthodox measurements) as a "regular" part of the quantum theory. The above mentioned further differences between formulations, which cannot be reduced one to another, then "painfully" reveal—not that there are a lot of ontologies—but that there are still none; i.e. that we still do not know the proper object of QM and that only by taking all these alternatives and doubts into account can we get a better insight into QM. Being aware of this fact would greatly help us to correct a highly distorted naturalistic world-view (a situation similar to the one in QM is recognizable also in other theories, e.g., in the theory of relativity), and dispense with one-dimensional paradigms and prejudices which obstruct the further development of science and reduce it to technology.

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NOTES

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