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GianCarlo Ghirardi and the interpretation of quantum physics

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Abstract
This paper comprises a few notes illustrating the impact of GianCarlo Ghirardi's achievements, even on the thinking of a 'non-realist'.

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Some quantum physicists are not in the least puzzled by quantum mechanics as it now is. They know its basic rules and how to calculate with it and—they say—what more could be needed? But others *are* puzzled. GianCarlo Ghirardi clearly is one of the latter and, in a way, so am I and have always been. I still remember the perplexity I felt when I started studying quantum physics and reading textbooks and articles concerning it. True, such documents neatly and adequately described the relevant facts and the theoretical formalism. But they were quite unclear, I thought, as to the actual meaning their authors attributed to the formulae they wrote. Were they just good recipes for predicting outcomes of experiments? Were they supposed to describe the very *things* the experiments bore on? Quite often, the argument seemed to go through only by virtue of Schlick's celebrated axiom that the meaning of a scientific statement *boils down* to its method of verification . . . so that, when all is said and done, correct prediction of what will be observed is all we may expect from science. But in their prefaces, conclusions, general comments, applications to solid state physics, thermodynamics or cosmology, the authors of these texts expressed themselves as if the probabilities and other items they dealt with did refer to things as they really are and events as they truly happen, quite independently of us. I hardly dared ask questions about this for I soon noted that, in the rare occasions in which the subject was raised, both my teachers and my young physicist colleagues kept quite silent. Obviously, as students all of them had been told that, to this puzzle, the 'founding fathers', in Copenhagen and Göttingen, had, long ago, proposed a solution. And clearly they took it for granted that the latter could not but smoothly match with the kind of commonsense 'realist' philosophy that the rapid growth of the scientific community had by then already reinstated as the scientists' 'received view'. By 'realist philosophy' I mean here the intuitive and 'classical' idea that human beings do possess, or are able to elaborate, the concepts with which Reality, conceived of as existing by itself quite independently of us, will eventually be described 'as it really is'. As to the way the matching in question had been achieved my teachers and young colleagues were not definite, nor were the physicists with whom I worked some years later. But good taste prevented us all from entering into detailed exchange of views about such matters, which obviously were not 'physics proper'. Indeed, during the first ten years or so that I worked —full or part time—in CERN I was sincerely convinced that I was the only physicist on the staff who found such questions worth pondering on.

It was not before year 1964 (or 1965, I do not remember) that I discovered I was wrong: in fact, in this respect I had a like. His office in CERN was rather close to mine and of course we already had had many occasions to discuss problems pertaining to our 'official' field of research that is, high energy physics. But each one of us was greatly surprised on discovering that, privately, the other one entertained, concerning the deep basis of our whole activity that is, the ground principles of quantum mechanics, the kind of puzzlement I just described. John Bell was his name.

Between Bell and I a decade or so of frequent exchanges of views and even sporadic collaboration on practical matters then began. John Bell had just published his by now classical paper on the incompatibility between quantum mechanics and local causality, and of course we were both yearning for experiments to be done for testing whether or not his inequalities are violated, as quantum mechanics predicts they should. In this spirit he greatly helped me organize the 1970 Enrico Fermi Summer School in Varenna, devoted to the foundations of quantum mechanics, where important steps were taken in this direction. And he and I jointly organized, in 1976, a workshop in Erice that, in particular, gave Alain Aspect the opportunity to get informed in detail about what had already been done in the field, thus providing him with some of the basic elements he so pertinently made use of in order to conduct his seminal experiments proving local causality violation.

Why on earth were we so anxious that convincing such tests should be made? The question, by now, seems ridiculous but at that time it sounded pertinent to a great many physicists, for indeed most of them tended to consider that since quantum mechanics had been found to yield correct predictions in an extreme variety of cases there was no point in striving to test it in one more case. To explain to them that there nevertheless was a problem and that testing the Bell inequalities would shed light on it was not easy. And in striving to do so John Bell and I were clearly 'on the same side': in the exhilarating situation of a minority that has a message to carry.

Does this mean that we had the same views and shared one and the same incentive? Not necessarily and, in fact, it was not the case. John Bell was at heart a realist. He simply could accept neither the view that basic, ultimate Reality is theoretically unknowable, nor—*a fortiori*!—the idea, central to radical idealism, that the very notion of an underlying mind-independent Reality is merely a 'limiting concept' quite irrelevant to knowledge. As for me, while also rejecting radical idealism and granting that realism is, *a priori*, the world view that all of us most naturally adhere to, I still entertained a motivated scepticism concerning it. More precisely, for reasons that date back to Descartes and were further developed by many prominent philosophers I did not take it for granted that the human mind is able to know and rationally understand 'reality as it really is', or even elements of reality 'as they really are'. Indeed, I had come to consider such views to be, not only unprovable but also rather unlikely to be true. I thought therefore that our efforts at elaborating scientific knowledge can hardly reach beyond mere phenomena and will always leave some free space for grand intuitions devoid of testable consequences; and, I must say, I took such a state of affairs to be, on the whole, satisfactory. But I nevertheless considered that we must do everything we can in the direction of rationally determining the boundaries of this free space, so as not to engage in naive creeds susceptible of being falsified by reason and facts. Now, clearly, to this end it is necessary to form a consistent view of what science actually is about and genuinely shows. And this is why, notwithstanding the differences between us, John Bell and I totally agreed concerning the deep inconsistency of a conception that was then most common amongst the practitioners of quantum physics (including high energy physics and so on) and which is still fairly general: I mean the one, alluded to above, that consists in granting that the basic laws of this science—taken to be of universal validity—are grounded on notions such as 'measurement' and other human-centred concepts and in believing nevertheless that it progressively lifts the veil of appearances and yields a more and more accurate description of 'reality as it really is'.

Of course, John Bell and I were not, by far, the only physicists whom such a state of affairs made uneasy. The reason that I centred the above considerations on him is that it was the reading of one of his papers that got me interested in GianCarlo Ghirardi's works, and that GianCarlo, besides being the physicist to whom this book is dedicated, is precisely one of those who most clearly justified the just mentioned uneasiness. These two points need be explained separately, which I shall presently strive to do.

Concerning the first one I would like first to note that John Bell's reaction to the outcome of the Aspect experiments is the best refutation I know of the sometimes heard contention that scientists finally manage to 'prove' what they were, at the start, prejudiced in favour of. For indeed Bell's realism incited him to believe in locality, it was with the view that they would prove it that he encouraged Aspect and others to perform the relevant experiments, and yet, even at an early stage in their development he frankly admitted that what the latter then indicated was just the opposite.

However, he did not give up realism for all that. He pointed out that the Broglie–Bohm model (of which he had given quite a remarkable reformulation) violates locality, is not at variance with the outcome of the Aspect-type experiments and, still, preserves realism. For a while I had the impression that, for lack of a better candidate, he inclined to really believe in it. But still, as many others, he had reservations, for he knew better than anybody else the difficulties it generates. I remember that once, having in mind the reformulation of it I just alluded to, I mentioned 'his' theory to him, and he answered, in a sad tone: 'I have no theory; I wish I had one!'. So, when I read his most illuminating paper [1] on the Ghirardi, Rimini and Weber model [2] I realized that the altogether new idea this model put forth was endowed, in his eyes, with a great value, just because it gave him anew some hope of salvaging realism in a scientifically credible way.

It goes without saying that this favourable opinion of him got immediately transferred to me. Not that I take the model to be 'true'. Not being committed to realism I do not consider the fact that a model is ontologically interpretable to be something that particularly speaks in its favour, so that, same as the bulk of the physicists, I weight the GRW model against standard quantum mechanics without taking account of this peculiarity it has . . . and then I find that the latter theory is simpler and appreciably more general. But still, my disapproval of the widespread inconsistency I characterized above results in that I have a particular regard for the minority of physicists who are deeply aware of it and reject it, and among them for the ones who, being convinced realists, show ingenuity in constructing realist models that are as free as possible from the difficulties inherent in reconciling realism and quantum physics. In this respect I could not but agree with Bell in judging that the model in question constituted quite a novel and most interesting approach to the problem.

For many convincing reasons that cannot here be expanded on, quantum mechanics is nowadays generally considered to have provided physics with a universally valid framework. And I just alluded to the inconsistency inherent in the fact of interpreting the content of a universal theory the axioms of which basically refer to measurements and similarly human operations as describing 'things as they really are', independently of ourselves. But is it really impossible to formulate standard quantum mechanics without thus referring to observations made by human beings? It is true that one of its 'axioms'—the Born rule—refers to measurement outcomes registered by either human minds or 'non-quantum' apparatuses, but is such a reference final and ineluctable? In the theory in question the state vectors yield the finest possible descriptions of physical systems, and *a priori* it might be conjectured that the generalized quantum measurement process should be describable as a particular case of the Schrödinger-like evolution law, without the pointer (more precisely, the composite system: 'observed system, pointer and environment') ever getting in a quantum superposition of macroscopically distinct states. In view of the immense complexity of macroscopic systems such as pointers and the practical impossibility of measuring most of the microscopic physical quantities pertaining to it, the hypothesis is not *a priori* absurd and, in fact, it was put forward in the past. The quantum measurement process could then be considered to be a purely physical phenomenon. Is it possible to prove that such an interpretation is at variance with the fundamental quantum laws? Long ago, elaborating on a basic idea due to Wigner [3], I showed [4] that, indeed, this can be done. My proof, however, was rather involved and, though general, perhaps not quite as general as could be requested by some. I was therefore extremely pleased when, some years ago, I read the paper by Angelo Bassi and GianCarlo Ghirardi entitled *A General Argument Against the Universal Validity of the Superposition Principle* [5]. For in this article the impossibility of reconciling the above stated hypothesis with standard quantum mechanics was indeed proved, and in a very elegant and, I think, truly general manner.

At any given time physics is composed of established results on the one hand and theories or models—that is, conjectures—on the other hand. Both are essential. The presence in physics of 'established results' is what differentiates it from philosophy. Hence its importance. However, rarely do established results yield, by themselves, anything resembling a world view or even a component of a world view. For yielding anything of the kind they have to be supplemented with, roughly, theories or models and, at that stage, divergent opinions may coexist. In the present case the negative result that has been arrived at shows that standard quantum mechanics—which had been constructed, in Copenhagen and Göttingen, along the lines of a philosophy of, say, the Machian variety—simply cannot be taken over just as it is and reinterpreted along the lines of realism. More precisely, it shows that the state vector, about which it is quite correct to assert that it embodies the most precise observational predictions that are susceptible of being made concerning a system, still cannot be interpreted as being a faithful and exhaustive representation of the ontological state of the said system. Or, at least, it cannot if, in conformity with what commonsense suggests, we consider that a quantum superposition of two or more state vectors corresponding to macroscopically distinct ontological states of a system cannot represent an ontological state of that system. On the other hand, as Bassi and Ghirardi pertinently pointed out, the result in question does not apply to the hidden variable interpretations of quantum mechanics such as, for example, the Broglie–Bohm model, in which the state vector is *not* considered an exhaustive representation of the ontological state of the system.

This is why, significant as it is, the result in question still leaves room for options. And that it does is all the more true as, contrary to what—fortunately!—is most often the case in science, among the here available options none is clearly freer than all others from features justifying some scepticism.

In fact, up to now three such 'options' are known to exist. One consists in holding to the view that state vectors are exhaustive representations of the ontological states of systems and giving up the universal validity of the superposition principle. Another one is to accept the existence of the (so-called) hidden variables. And a third one, somewhat in the spirit of Bohr's approach, is just to give up, or considerably water down, philosophical realism.

The first option was taken (and, to my knowledge, initiated) by Ghirardi, Rimini and Weber in their above quoted pioneering work, in which they showed that it is indeed possible to add to the Schrödinger equation nonlinear terms small enough not to alter in any measurable way its consequences relative to microsystems and which nevertheless entail frequent spontaneous reductions of the centre of mass wavefunctions of macroscopic systems. In the same line similar models were put forward by others, some of them linking the process to gravitation. All such models account for what is or can be observed during a (generalized) measurement process in a way very similar to that in which decoherence does, the difference being that decoherence theory accounts only for the impressions we have, whereas the models at hand may be claimed to describe what takes place 'in reality'. This is not the proper place for discussing in detail the unpalatable features that, to repeat, this first option has, same as the other two ones. Let it just be mentioned that they have to do with relativity theory and also the question of energy conservation.

The second possible option, the one that consists in believing in the existence of supplementary (often called 'hidden') variables, is best exemplified by the Broglie–Bohm model. It also has, as is well known, unpleasant features, due on the one hand to the difficulties its relativist extension meets with and on the other hand to the fact that most often the physical quantities it takes to be real are not those that are observable (for more details see e.g. [6]).

These two first options smoothly fit within the 'realist' framework, a trait that, in the eyes of most physicists (who tend, on the whole, to be realists) compensates, at least to some extent, for their just mentioned unpleasant features. The third option that I have in mind consists, in contrast, in giving up the view that physics must be aimed at yielding an ontology, that is a description of 'things as they really are, independently of ourselves' (or at least at validating the Kantian view that 'everything takes place as if' such a description were available). And *this* giving up is what counts as its unpleasant feature in the eyes of most physicists. Note however that it has no other one (if it is one!). For as soon as we switch to such a point of view, we no longer find it unacceptable that physics, rather than being descriptive, should merely be predictive of observations. And then the interpretational difficulties of quantum mechanics just simply vanish since the set of the quantum mechanical basic laws is in fact nothing else than a set of predictive rules, yielding the probabilities that, in such and such circumstances, we shall observe this or that (again, details concerning the structure and internal consistency of this option are to be found in [6]).

As already stated, though I am not an idealist (I think the notion of a basic reality is indispensable), this third option is the one that, personally, I favour (to repeat, I willingly give up the view that basic reality is discursively reachable as it really is). However, I have no difficulty in understanding, and sympathizing with, the supporters of the two other ones. For the task that, to me, appears to be the most significant is that of demarcating the options that are admissible ones from those that are not.

And, obviously, to the advancement of this enterprise our friend GianCarlo outstandingly contributed.

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