John Bell's Varying Interpretations of Quantum Mechanics*

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1. Varenna 1970

For the first time I met John Bell at the Varenna conference of 1970.[1] I had been invited by Bernard d'Espagnat on suggestion of Eugene Wigner, who had helped me to publish my first paper on the concept of what was later called decoherence (to appear in the first issue of the Foundations of Physics a few months after the conference). This concept arose from my conviction, based on many applications of quantum mechanics to composite systems under various conditions, that Schrödinger's wave function (or more generally the superposition principle) is valid and applicable beyond microscopic systems - for example in the form of wave packets (and not only in a statistical sense).[2] Superpositions are known to define novel individual physical states or properties, but they can easily be dislocalized (distributed over many degrees of freedom) by means of the unitary dynamics described by the Schrödinger equation, and thus become unobservable. So I had never felt any motivation to think of "hidden variables" or any other physics *behind* the successful wave function.

Therefore, I was very surprised on my arrival in Varenna to hear everybody discuss Bell's inequality. It had been published a few years before the conference, but I had either never noticed it or not regarded it as particularly remarkable until then. As this inequality demonstrates that the predictions of quantum theory require any *conceivable* reality possibly underlying the nonlocal wave function to be nonlocal itself, I simply found my conviction that the latter suffices to describe reality confirmed. Although the first results from crucial experiments (presented at Varenna by Clauser, Horne, Shimony and others) were still preliminary, they assured me that everybody would share my opinion as soon as Bell's argument had become generally known and understood. I certainly did not expect that almost fifty years later many physicists would still be searching for "loopholes" in the experiments or for other forms of nonlocality than the wave function, or even deny any microscopic reality in order to avoid contradictions or absurd consequences that result from the prejudice of a local reality.

So I was quite happy to hear John announce a talk "On the assumption that the Schrödinger equation is exact" one or two years after Varenna at a meeting d'Espagnat had organized in Paris. I will have to come back, though, to what he really meant with this title.

Before he published his inequality in 1964, Bell had refuted von Neumann's disproof of hidden variables that was often cited in defense of the Copenhagen interpretation as a closed and final theory. (The publication of this paper had been delayed until 1966 by some accidents.) In Varenna he began his talk by arguing that all physical systems are described by means of two different concepts: classical properties Λ and a wave function ψ. The latter he suspected to be merely "subjective". Today we would then call it an epistemic concept, representing incomplete information, but "information" would only make sense for him with the possibility to add "about what?" and "by whom?". It was this kind of clarity in pointing out misconceptions that always impressed me in discussions with John, or in his talks and publications.[3] He never shared the "pragmatic logic" of many physicists who consider any argument that leads to the expected or empirically known result as correct. Another example was his repeated objection against operational arguments used by some axiomatic quantum theorists at the conference, who suggested to replace certain superpositions by ensembles whenever the formal observables required to confirm them appeared not to be realizable for some reason ("superselection rules"). He insisted that not being able in practice to distinguish between a superposition and an ensemble consisting of its components does not prove them to be the same. This conceptual confusion may also occur in connection with decoherence when one uncritically interprets the reduced density matrix as representing an ensemble rather than entanglement (see below). A related third example that comes to my mind is his (very politely formulated) criticism in Ch. 6 of Ref. 3 of Hepp's attempt to justify ensembles of measurement outcomes by means of the purely formal but insufficient limit of an infinite number of subsystems or degrees of freedom.

The existence of two different realms of physics (quantum and classical) represented consensus among most quantum physicists at that time - even though one knew from the early Bohr-Einstein debate that classical variables Λ, too, had to obey the uncertainty principle in order to avoid contradictions. However, in contrast to the majority of physicists, most participants at the conference agreed that the absence of a well defined border line between these realms represented a severe defect of the theory that called for new physics. Decoherence was not yet known as a possible *effective* border line, while mesoscopic quantum physics had hardly been seriously considered. In fact, when I began presenting decoherence arguments to my colleagues, the usual objection was that “quantum mechanics does not apply to the environment”.

John then continued his talk by explaining his arguments against von Neumann’s exclusion of hidden variables, gave an outline of David Bohm’s theory (that had motivated these arguments), and finally derived his inequality whose violation, predicted by quantum theory, would exclude *local* hidden variables if confirmed by experiment.

This conclusion seemed to form a great surprise and to appear almost inacceptable to many participants. Some young and also some not-so-young physicists there were strongly motivated by Marxism (this conference took place two years after 1968!). They could not accept any “idealistic” interpretation of physics, and sometimes tried to propose very naïve classical models that somewhere had to be in conflict with quantum theory. However, Bohr had correctly concluded already in 1924 (after his attempt with Kramers and Slater had failed) that “there can be no simple solution” to the problems presented by the quantum phenomena. Nonetheless, in Bell’s (and my) opinion this is no reason to abandon the concept of reality altogether, which can perhaps be understood as a synonym for a consistent, universally valid, and successful description of Nature. For him, the renouncement of reality would be the end of physics (as I understood him). Very probably this conviction was the major motivation for all his efforts regarding the foundation of quantum mechanics, but his theorem demonstrated that microscopic reality has to be far more unusual than one might naively have expected.

At Varenna, I was particularly interested in Bryce DeWitt’s talk on the Many Worlds interpretation, because I had mentioned Everett’s work myself as the only remaining (but possible) solution if the Schrödinger equation was exact and complete. I felt a bit confused when I saw him translate Everett into the Heisenberg picture. For me, Everett’s main point was an evolving wave function of the universe. He had attended lectures by von Neumann, who used to describe the measurement process in purely wave mechanical terms, assuming the pointer position to be represented by a moving narrow wave packet rather than a classical variable. This “Princeton school” of quantum mechanics (always called the “orthodox interpretation” by Wigner) seems to have also influenced Richard Feynman.[4] Only much later did I understand, mainly from David Deutsch’s writings, that for him and DeWitt Many Worlds meant many classical trajectories (or Feynman paths), while for Everett and me this concept meant many branching wave packets in configuration space. For example, while Deutsch regards a quantum computer as an example for many worlds in action, in Everett's sense they must all remain part of one branch world in order to lead to one quasi-classical result that can be recognized and used by humans. Only if there were *macroscopically* different intermediate states, could their superposition give rise to different "worlds" by their decoherence - but this would also ruin the quantum computer. These different representations of "reality" are also relevant for Bell's various interpretations of quantum mechanics.

2. Bell on Bohm's Theory

Although Bohm and Hiley were present and gave talks at Varenna (as well as Andrade e Silva, who represented Louis deBroglie), I first understood Bohm?s theory when studying Bell's Varenna contribution. He presented it as a "simple example" for hidden variables, even though it was in contrast to his introductory remarks: it neither *replaced* the wave function ψ nor explained it in terms of an ensemble of hidden variables. In more recent language, this theory is ψ-ontic, but in addition assumes the existence of hidden variables λ that are identified with the pre-quantum variables (such as particle positions and field amplitudes). So these variables are isomorphic to the arguments of his wave function. This allowed Bohm to assume the Schrödinger equation to be exact (the same as in Everett's theory!), and a classical configuration of the world to be dynamically guided by this wave function instead of obeying Hamilton's equations. Bell meant essentially this model by the title of his talk that I first heard in Paris, where the Schrödinger equation is not only assumed to be exact, but also to be universal. There are no additional classical variables Λ any more (they are simply functions of the λ's), but Bell regarded t as an important advantage that Bohm's theory does not need the "notoriously vague concept of a reduction of the wave packet".

However, he also remarked that "what happens to the hidden variables during and after a measurement is a delicate matter". In my opinion this is a serious weak point of the theory, since the λ's have to be *postulated* to form a statistical distribution with probabilities given by $|\psi(\lambda)|^2$. This postulate is dynamically consistent under Bohm's dynamics, but (1) no plausible motivation for this statistical assumption (in contrast to the individually treated wave function) is given, and (2) the probabilities must change by a change of information by measurements, although no physical carrier of this information is taken into account ("information by whom?"). This comes close to the crucial assumption of an external (human?) observer in the Copenhagen interpretation.

Supporters of Bohm's theory usually discuss only special applications, combined with a much too optimistic view. However, *all* its applications must trivially lead to results in accordance with quantum mechanics simply by construction of this theory (and thus cannot serve to confirm it), but they appear plausible in some sense only in simple cases, such as single-particle scatterings. Whenever several variables are entangled in the wave function, the hidden variables follow entirely "absurd" trajectories that have nothing to do with what we would expect or what we *seem* to observe.[5] For example, particles may pass the detector that does *not* click in an experiment, and vice versa. In my opinion, this behavior eliminates any motivation for this model. Its trajectories can neither be observed nor remembered: they are meaningless.

On the other hand, Bohm was perhaps the first physicist to take entanglement seriously beyond microscopic systems. Shelly Goldstein even claimed that Bohm anticipated the decoherence concept when discussing measurements in his theory. This is a bit of an overstatement and a misunderstanding. In order to describe *successions* of measurements, Bohm had to discuss how the probability distribution of his classical configurations λ has to be restricted to some small "effective" component of the wave function (essentially "our" Everett branch), and this means first of all that these branches have to remain dynamically autonomous for some time (the way we calculate in practice). This is similar to Mott's analysis of α-particle tracks in the Wilson chamber, where decoherence of the droplet positions by entanglement with an unbounded environment was *not* yet taken into account. Decoherence explains the required autonomy "forever", and it demonstrates that macroscopic variables seem to form narrow wave packets which resemble definite classical states in each of these components. In measurements, this leads to apparent irreversible quantum jumps (see Sect. 3). Bohm would then have noticed that his presumed variables λ become obsolete. However, the understanding of decoherence requires detailed calculations for realistic environments, which were performed only during the eighties by Wojciech

Zurek, Erich Joos, and others.

During the decade following Varenna, John Bell presented various versions of his talk about the "assumption that the Schrödinger equation is exact". Just as many other fundamental papers at that time, they were often first published in the informal Epistemological Letters, since established journals were still reluctant to accept papers on interpretational issues of quantum theory. Only after his inequality had become known to allow crucial experiments to be performed in laboratories did this situation slowly change - one of John's historically most important achievements.

A slightly modified version of these talks (for a special purpose) was published in 1981 under the new title "Quantum mechanics for cosmologists" (Ch. 15 of Ref. 3). It contains a number of important statements. Talking about Bohm, he says that "nobody can understand this theory until he is willing to think of ψ as a real objective field rather than a probability amplitude". This is in clear contrast to his previous interpretation of ψ as a "subjective" concept. As only one set of λ's is assumed to be real (located somewhere in the myriads of branches of the universal wave function), he compares ψ with the Maxwell fields, which are similarly assumed to exist even where no charged particles are present, but adds that "it is in terms of the λ" (that he now calls x) "that we would define a psycho-physical parallelism - if we were pressed to go so far". Therefore, he now called these formerly "hidden" variables "exposed" - but their exposure (together with their very existence) remains a model-dependent hypothesis. The λ's may appear "more real" than ψ to the traditional mind because they are locally defined and *apparently* observed. This is also the reason why quantum nonlocality is often understood as a spooky action at a distance rather than a kinematical nonlocality represented by ψ itself. (In classical context, we similarly prefer to believe seeing objects rather than - more realistically - the light reflected by them, or even the nerve cells excited by the light in the retina and in the brain. In this classical picture, however, all these physical elements and their interactions can be regarded as empirically well established.)

When mentioning Everett's interpretation as another possibility for the Schrödinger equation to be exact, John usually disregarded it for being "extravagant" - not for being wrong. This is an understandable and very common emotional position. For example, Stephen Weinberg declared in an interview about his recent book on quantum mechanics (for Physics Today Online of July 2013) that "this effort [of not conceptually distinguishing the apparatus or the physicist from the rest of the world] may lead to something like a 'many worlds' interpretation, which I find repellent. ... I work on the interpretation of quantum mechanics from time to time, but have gotten nowhere." There exist in fact many emotions but hardly any arguments against Everett. In Ch. 11 of Ref. 3, Bell raised the objection that Everett's branches are insufficiently defined or even arbitrary. This problem has been overcome by properly taking into account decoherence (Sect. 3).

After John had given a version of his talk at Heidelberg in about 1980, we had a brief correspondence, where I tried to point out to him that Bohm's theory is just as extravagant as Everett's in the sense that its wave function contains precisely the same components that are regarded as "many worlds" by Everettians. The only difference is that most of these components are called "empty" by Bohmians, since only one set of λ's from their presumed statistical ensemble is assumed to be real, while all the empty parts of the wave function are still assumed to exist! We also debated the relation between the concept of reality and that of "heuristic fictions" on this occasion, but the correspondence led to no obvious result. Nonetheless, it may have had some consequences a few years later (see Sect. 3).

When re-reading Bell's "Quantum theory for cosmologists" for the preparation of this paper, I discovered another astonishing remark about Everett. Bell initially points out not to be quite sure whether he understands Everett correctly, but then claims a previously unknown "close relationship between Everett and Bohm". He says that "all instantaneous classical configurations λ are supposed to exist" in Everett's theory (his extravagance) "with probabilities according to $|\psi(\lambda)|^2$". This would indeed come close to Deutsch's identification of (many) "worlds" with trajectories in configuration space. Deutsch has repeatedly called Bohm's theory a "many-worlds theory under permanent denial". In particular, Bell's remark indicates that he, too, prefers to understand physical reality in traditional classical terms - probably a major motivation for his favor for Bohm's theory. So Bohm and Deutsch *presume* classical concepts. This explains why they do not *need* decoherence to justify them, but if we defined "worlds" as consisting of trajectories for macroscopic objects plus wave functions for electrons in atoms or solid bodies, we would be back searching for Bohr's border line between two different realms of physics.

In contrast, Everett himself interpreted the world completely in terms of wave functions (he was von Neumann's student). A relation to classical concepts may then be provided only in terms of wave packets in configuration space. This means that Everett is conceptually not closely related to Bohmian mechanics with its essential variables λ, but rather to Bell's favorite-to-come: collapse theories.

3. Collapse Theories

In 1987, John Bell surprised the world of his admirers by a drastic change of mind. In- spired by a paper by Ghirardi, Rimini and Weber (GRW),[6] he now advocated for collapse theories (see Ch. 22 of Ref. 3). That is, he supported what he had previously called the "notoriously vague collapse" - though in a newly specified and hypothetical form. This proposal would avoid all those myriads of "other" branches of the wave function which he found extravagant in Everett's interpretation, and which had to be regarded as "empty" in Bohm's. It does not necessarily mean that he abandoned Bohm completely. He may simply have started another, independent attempt to search for a solution of the quantum problems in terms of a realistic theory, but his radical change of concepts may also indicate that he was not quite happy any more with his previous favorite.

When John von Neumann first suggested his collapse or reduction of the wave function, he felt motivated not only by the need to explain definite pointer positions, but also to facilitate a psycho-physical parallelism with respect to local observers in spite of nonlocal wave functions. These two different though related intentions reflect Bohr's and Heisenberg's slightly different understandings of quantum measurements. While the former insisted that indeterministic measurement outcomes have to be objectively described in terms of classical pointer states (which could thereafter be observed in a traditional way by interaction with classical media and observers), the latter had regarded measured properties (including particle positions) as being *created by their observation by humans*. This difference left many traces in the history of quantum measurement theory, but both aspects seem to be relevant in some way (see below).

The GRW collapse was clearly meant to describe an objective physical process (for a phenomenon that Bohr had regarded as *not* dynamically analyzable). Therefore, these authors concentrated on a process of "spontaneous localization" for the wave functions of macroscopic variables. For this purpose, they postulated a general nonlinear, nonunitary and irreversible "master equation" for the density matrices of all isolated physical systems. It was to replace the unitary von Neumann equation, which is equivalent to the Schrödinger equation that they now assumed to apply only approximately in the microscopic limit. They also assumed tacitly that this density matrix describes an (ever-growing) ensemble of *possible* wave functions, but the problem is that such an ensemble is neither uniquely defined by the density matrix, nor can the latter distinguish between ensembles and entanglement of the considered system with other systems.

Indeed, immediately after their paper had appeared, Erich Joos was able to demonstrate [7] that their master equation can be well understood within unitary quantum mechanics as a consequence of the unavoidable interaction of macroscopic systems with their environment - a process now called decoherence. However, this decoherence describes growing entanglement rather than a transition from pure states into ensembles (such as those representing different measurement outcomes). Therefore, two questions arise: (1) how can GRW's master equation be understood as describing measurements, and (2) what does the undeniable environmental decoherence, that can hardly *accidentally* lead precisely to the required density matrix, mean for the measurement process?

In order to answer the first question, John Bell proposed a stochastic ("quantum Langevin") equation for the dynamics of individual wave functions. It would have to complement and modify the Schrödinger equation. The thus dynamically arising ensemble of potential future wave functions can then be represented by a density matrix that may indeed obey GRW's master equation. His specific model postulated spontaneous jumps of single-particle wave functions into slightly more localized partial waves with Born-type probabilities. He assumed the time scale for these jumps to be of the order 10^{15}sec, but this time would then have to be divided by the number of contributing par- ticles, and so become sufficiently short and efficient for macroscopic objects. However, he also noticed and listed a number of problems, such as the entanglement between particles and the generalization of his proposal to QFT (others have later been added), but he expressed hope that they can be overcome. I doubt that this has ever been achieved for this model, but there exists a wealth of similar and also of quite different collapse models, which can be falsified only one after another, and only when defined sufficiently precisely. They share this property of being falsifiable with the Everett interpretation, which could be ruled out by the discovery of appropriate deviations from global unitarity. So the possibility of a well defined and successful dynamical collapse is still around, but none of its proposed versions has ever been verified by experiments so far.

The major reason for this undecided situation is that any realistic collapse dynamics that is to describe measurements would have to be carefully shielded against all competing decoherence effects caused by the environment in order to be confirmed - an almost impossible requirement in the macroscopic realm. As the reduced density matrix arising from decoherence cannot be locally distinguished from that of an ensemble, it is sufficient FAPP (for all practical purposes - an often misused term that Bell heavily used in his last paper "Against measurement",[8] written in 1989). Interaction with the environment can indeed never describe the transition of a global pure state into an ensemble of possible outcomes (or even into an individual outcome) - it merely describes the dislocalization of all macroscopic superpositions by means of the spatially spreading entanglement. If the environment is described by an *ensemble* of initial states, this conclu- sion holds for each of its members (as often emphasized by Eugene Wigner); the density matrix representing this ensemble merely hides the lasting entanglement that gives rise to a superposition of "many worlds" in each case. Therefore, no kind of classical "noise" (such as represented by an uncertain or fluctuating Hamiltonian) would be sufficient to *explain* a collapse, while arising entanglement with a gravitational field is just a special (though not very relevant) form of decoherence. A genuine collapse would have to be *postulated* as a fundamental deviation from unitarity. The omni-present formation and spreading of initially absent entanglement, on the other hand, seems to form the general "master arrow of time" and the basis for the concept of a time-directed causality.

In contrast to Bohr's above-mentioned understanding, collapse theories assume the wave function (though not the Schrödinger equation) to apply universally, and thus to form an ontic concept again. The wave function must then in principle also describe the brain with its expected specific role in a psycho-physical parallelism. In the absence of Bohm's λ's, and under his new assumption of spontaneous jumps, Bell now suggested that consciousness be related to such (again model-specific) "events", while von Neumann had related consciousness to the physical states of observers that he assumed to arise from his vaguely defined collapse. Such a relation is certainly essential in order to understand how the world that we observe is related to the hypothetical objective world (that is, how observations come about in objective terms). Einstein called it the "whole long way from the object to the observer" that we must understand in order to know what we have observed.

In my paper of 1970,[9] I had already pointed out that entanglement with the environment - although it can explain the absence of certain *local* superpositions (often regarded as superselection rules) - can *not* explain objective ensembles. Therefore, I suggested a solution similar to Everett (see Sect. 4 of Ref. 2, for example). This very possibility is sufficient to demonstrate that Bell's claim that "the wave function must either be incomplete or not always right" cannot be true. In several subsequent papers I even tried to learn more about the conditions for consciousness in quantum mechanical terms by using the single-sum Schmidt canonical representation for entangled states (assuming a fundamental local observer system in the brain, for example), but this attempt did not turn out to be very helpful. We can argue objectively only in terms of robust proper- ties, such as memory (physically realized in data storage devices or in decohered parts of the brain - conceptually not very different from pointer positions).

Most workers in the field of decoherence have restricted their interest to the objective effects of the environment on the density matrices of local systems. This is usually sufficient FAPP. Most existing proposals for a fundamental collapse are therefore simply attempts to mimic decoherence, and are thus based on a prejudice (for what has to be achieved) that arose in pre-decoherence times. There are far more *other* possibilities for the collapse (if it exists) along Einstein?s "long way". As decoherence describes *apparent* transitions into ensembles and *apparent* quantum jumps, which can even be experimentally confirmed to form smooth processes, it has indeed often been misunderstood as a derivation of the probabilistic collapse from the Schrödinger equation. (I remember authors claiming that decoherence prevents us from the consequence of many worlds, although precisely the opposite is true!) This misuse of the density matrix has a long tradition in measurement theory, and John was certainly right to object against it also in connection with decoherence. In order to understand what happens "really", one has to analyze the consequences of the environment on the individual universal wave function (just as he did for the collapse mechanism in order to give it a precise individual meaning).

The original argument that led to decoherence was to point out that entanglement must be far more common for dynamical reasons than had ever been envisioned and taken into account. However, not the formal diagonalization of the arising reduced density matrices is essential for an effective ensemble, but the fact that different "branches" of the wave function become more and more dynamically independent of one another, and hence "autonomous". This autonomy includes the impossibility to relocalize nonlocal superpositions ("recoherence") for reasons that are analogous to classical arguments which explain why local effects are extremely improbably (impossible in practice) to be caused by nonlocal statistical correlations that were previously created in chaotic Boltzmann collisions. The permanent "branching" of the wave function into autonomous components is a consequence of the Schrödinger equation - it does not have to be postulated in any way. One may easily recognize that different pointer positions, dead and alive cats, and different states of awareness of an observer can only exist within such separate autonomous branches of the wave function. Therefore, different "versions" of an observer can only define independent subjective "identities". In this Everettian sense, Heisenberg's subjective interpretation of measurement outcomes (as being created by their observation) may thus be justified - although not in terms of fundamental particle or other classical concepts. When some colleague of mine, Dr. X, say, keeps asking me: "If different components of me really exist in many different components of the universal wave function, why am I aware of only one of them?", I like to ask back: "Why are you Dr. X, and not the whole world?" It is the same kind of question.

Unfortunately, John Bell never seriously considered this version or variant of Everett (as far as I know). He may still have regarded it as "extravagant" because of the myriads of versions of each observer that have to arise according to the Schrödinger equation. Collapse models may *postulate* similarly defined branches of the wave function to disappear all but one from reality, but an observer does not have to bother whether many other versions of himself do exist in autonomous branches or rather have disappeared from reality - unless he is one of those rare non-pragmatic quantum theoreticians like John Bell. A physicist will in any case *use* the collapse FAPP as soon as decoherence (understood as the dislocalization of an originally microscopic superposition) has become irreversible in practice after a measurement-like process. This environmental decoherence defines a natural position of the Heisenberg split FAPP. However, we may learn from Everett that we do not have to expect this collapse to be an objective physical process that may some day be confirmed and located. It is just *convenient*, and may thus even be defined to act superluminally! This pragmatic convention seems to be the origin of all that confusion about reality and "information", counterfactuals, and similar concepts which John Bell never found very helpful.

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