Van Fraassen, Everett, and the Critique of the Copenhagen View of Measurement

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Abstract

Bas van Fraassen advocates a "Copenhagen variant" of the modal interpretation of quantum mechanics. However, he believes that the Copenhagen approach to measurement is not fully satisfactory, since it seems to rule out the possibility of providing a physical account of the observation process. This was also what John Wheeler had in mind when, in the mid-1950's, he sponsored the "relative state" formulation proposed by his student Hugh Everett. Wheeler, who considered himself an orthodox Bohrian, tried to convince Bohr to accept the improvement of the Copenhagen approach represented in his eyes by Everett's proposal. This attempt gave rise to a lively debate, which has been only recently documented, and which provides an interesting framework for the appraisal of van Fraassen's own programme.

1. Why is measurement a problem?

In the Copenhagen interpretation of quantum mechanics, the probabilities deduced from the state vectors via the Born rule do not refer to the values that an observable *has* in the absolute, but rather to the values that it *can take given that a measurement is carried out* (van Fraassen 1980, p. 175). Accordingly, as van Fraassen emphasizes, "on this interpretation there is no implication at all that the observable has any particular value, or indeed, any value at all, when no measurement is made." For van Fraassen, such a "conservative" view has the merit of dissolving many paradoxical aspects of quantum mechanics. This is why his own approach, which he names the *Copenhagen variant* of the modal interpretation, borrows from the Copenhagen interpretation its "central tenet" that observables do not necessarily have a value in all situations (van Fraassen 1991a, p. 280). However, van Fraassen observes that the explicit reference to measurement in the statement of the Born rule raises a problem of consistency. Since "a measurement is itself a physical interaction, and hence a process in the domain of applicability of quantum theory", the question arises as to whether "what quantum theory says about such processes cohere[s] with the role they play in the Born rules linking states with measurement outcomes." (van Fraassen 1980, p. 177; see also van Fraassen 1991a, p. 246).

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In a paper of 1972, van Fraassen distinguishes two main orthodox ways to deal with measurement in quantum mechanics, both of which involve state reduction. The first is the approach based on thermodynamics, which was initially prompted by Jordan and subsequently developed for example by Daneri, Loinger and Prosperi (1962). In the 1960's, as we will see, this approach received the support of Léon Rosenfeld, who was one of Bohr's closest collaborators. The second approach is that which, following Hugh Everett (1957), van Fraassen calls the *external observation formulation*. Referring to John A. Wheeler's "comments on Everett's initial paper",[1] van Fraassen summarises such an approach as the view according to which "quantum theory was devised to describe only situations in which an observer (or at least, the measuring environment) is involved, while leaving that part out of the description." In other words, in this view, "a measurement is an interaction *incompletely described*, by leaving out something or other." (Van Fraassen 1991a, p. 273.)[2] Van Fraassen relates the external observation formulation to "some early Copenhagen texts" (*Ibidem*), and among its spokesmen, he seems to include Hip Groenewold, who was a former student of Rosenfeld (van Fraassen 1972, p. 333). In the paper of 1972, both the external observation formulation and the approach based on thermodynamics were considered to be unsatisfactory by van Fraassen, and he took their criticism as the starting point for developing his own proposal.

When such a proposal appeared, however, the challenges to the orthodox view that it contained were not new. Indeed, some of van Fraassen's criticisms were similar to those outlined in the PhD dissertation of Everett (1957). Like van Fraassen's modal interpretation, Everett's "relative state" formulation aimed at providing a complete characterization of measurement in quantum mechanical terms while at the same time dismissing the postulate of projection.[3] Notwithstanding some crucial differences, both Everett and van Fraassen pointed out the empirical redundancy of such a postulate, and claimed that the objective features of measurement results "are already settled on the level of [unitary] quantum theory itself, before we enter upon interpretation" (van Fraassen 1991a, p. 255). Like van Fraassen, Everett distinguished between the "popular" approach to measurement generally associated with von Neumann's formulation and the "dualistic" approach inspired by what he took to be Bohr's view. And, like van Fraassen and for reasons similar to his, he was equally unsympathetic to either. Nevertheless, amazing as it is, John Wheeler, who was Everett's advisor at Princeton and an orthodox Bohrian, thought that Everett's ideas were not at variance with the Copenhagen approach, but simply generalized it. More specifically, he held that such ideas provided a brilliant way to get rid of what he considered as the only shortcoming of the Copenhagen interpretation, namely that it prevented one from treating the observer as a quantum system when describing a particular experiment.

Wheeler's efforts to convince Bohr to accept Everett's ideas gave rise to a lively debate, whose existence and content have only recently been documented (Freire 2004; 2005; Freitas 2007; Byrne 2007; Osnaghi, Freitas & Freire 2009). The interest of such a debate is not merely historical, since the analysis of the current controversy on the measurement problem shows that the root of the misunderstandings that underlay the discussions between Everett, Wheeler and the Copenhagen group has not really been removed. To borrow van Fraassen's words (1991b, p. 503), "it is as if some single mystical dragon is slain and always again resurrects itself in a new form." The purpose of the present paper is to analyse the import of the early debate on Everett's thesis for the current discussions of the measurement problem, and, more specifically, for any account and solution of the problem along van Fraassen lines.

2. The untold story of Everett's thesis

The fact that Wheeler was persuaded that Everett's proposal was "not meant to question the orthodox approach to the measurement problem"[4] is puzzling. For if Everett's relative state formulation has become popular as one of the most heterodox interpretations of quantum mechanics, this is due mainly to its non-conventional approach to measurement. Yet, since 1955 (that is, almost two years prior to the end of Everett's PhD), Wheeler considered the possibility of showing Everett's work to Bohr in order to obtain his blessing. In 1956, when Wheeler left Princeton to spend a semester in Leiden, he carried a draft of Everett's thesis with him. A few days after his arrival in Europe, he went to Copenhagen expressly to discuss it with Bohr. They had several intense discussions and, after Wheeler's departure, the draft remained in Copenhagen for further scrutiny. In the subsequent months, Wheeler tried to convince Bohr to publish Everett's dissertation in the Danish Academy of Science ("That's the perfect place for it", he says in a letter to Everett[5]). Furthermore, he organised a visit of Everett to Copenhagen (he wrote to Everett: "Unless and until you have fought out the issues of interpretation one by one with Bohr, I won't feel happy about the conclusions to be drawn from a piece of work as far reaching as yours."[6] Everett's visit, however, did not take place, and a second attempt made the following year aborted as well. Only in 1959 was Everett eventually able to spend a few weeks in

Copenhagen, but his meeting with Bohr was quite disappointing.[7] Thus, notwithstanding Wheeler's reiterated efforts, the Copenhagen group remained not only unsympathetic to Everett's ideas, but also reluctant to attach any relevance to them.

Everett had enrolled himself at Princeton University in 1953, a few months after the departure of another young and brilliant critic of Bohr's complementarity: David Bohm.[8] Even though Princeton hosted some of the most distinguished experts of the foundations of quantum theory, like Einstein, von Neumann and Wigner (who was Everett's professor of mathematical methods), from Everett's recollections the context was not one in which investigation into the interpretation of quantum mechanics was encouraged.[9] Everett's papers demonstrate a good command of the post-war literature on the subject. For example, he was acquainted with Bohm's work on hidden variables as well as with Schrödinger's attempt to develop a non-naïve "wave interpretation" of quantum mechanics. Everett remembers in an interview that a decisive contribution to his reflections on quantum mechanics came from discussions with Aage Petersen, who was Bohr's assistant, and was then spending one year at Princeton.[10] At the end of 1954 or in 1955 Everett approached Wheeler, who became his advisor. Wheeler was then involved in the research on cosmology and collaborated with the Chapel Hill group headed by Bryce DeWitt. He had a great admiration for Bohr, whom he had met twenty years earlier when he joined the Institute of Theoretical Physics of Copenhagen with a two-year postdoctoral fellowship. Like many physicists of his generation, Wheeler was deeply impressed by the atmosphere at the Institute and was strongly influenced by Bohr's charisma. In matters of interpretation of quantum mechanics, Wheeler considered Bohr as the supreme authority (in a paper of 1956 he describes Bohr's complementarity view as "the most revolutionary philosophical conception of our day"; Wheeler 1956, p. 374). Hence, the fact that he decided to discuss Everett's ideas with Bohr in person shows how valuable and important he considered them to be.

Everett's dissertation was submitted in March 1957 and published a few months later in the *Reviews of Modern Physics*, within a collection of papers prepared in connection with a conference on gravitation held at Chapel Hill. Everett's work remained almost unknown until the late 1960's, when DeWitt mentioned it in a couple of widely read papers (DeWitt 1967; 1970). In 1973, a longer version of Everett's dissertation (Everett 1973) was published in a collective volume edited by DeWitt and his student Neill Graham. In this volume, DeWitt presented his famous many-worlds interpretation of Everett's ideas, which explains why the relative state formulation was subsequently identified with many-worlds (one finds this identification for example in van Fraassen 1991a). Those who are familiar with Everett's writings know that the paper published in 1973 was actually written before the dissertation. However, what is less known, and can be inferred from Everett's and Wheeler's correspondence, is that the dissertation of 1957 was the *third* version of the original thesis. This means that we lack one of the versions, presumably the first one. Nevertheless, the archives contain further relevant manuscripts, which are helpful when attempting to understand the origin of Everett's project.

In these manuscripts, which were written in 1955, i.e. the year the American edition of von Neumann's *Mathematical foundations of quantum mechanics* appeared,[11] Everett outlines his critique of the standard formulation. Even though Everett claims that he has "no preference for deterministic or indeterministic theories",[12] he nonetheless rejects the conventional probabilistic interpretation of quantum mechanics because, in his opinion, such an interpretation implies that the physical state of a system changes in a non deterministic way *as a consequence of observation*. (This is how Everett understands von Neumann's postulate of projection.)[13] Based on a "Wigner's friend"–type argument (Wigner 1961) which appears both in the second version of the thesis (Everett 1973) and in an earlier manuscript (Everett, 1955?), Everett argues that the consistent application of the projection postulate within the standard theory entails the commitment to the solipsistic hypothesis that there is only *one* observer in the universe who is responsible for the collapse of the wave function of all (observed) systems. The typical way to avoid this conclusion is, according to Everett, to drop an essential premise of the argument, namely the hypothesis that quantum mechanics provides a *complete* description of the world. This leaves one two options.

The first option is to postulate hidden parameters whose values are not captured by the state vectors and yet can account causally for the outcomes of measurements. In his writings, Everett acknowledges the "great theoretical importance" of hidden variables theories (Everett 1973, p. 113), but he emphasizes that such theories are unnecessarily "cumbersome and artificial" as compared to his own.[14] The second option is to deny that measurement interactions belong to the domain of quantum mechanics. In Everett's words, this amounts to assuming that "not every physical system possesses a state function, or put in another way, that even in principle quantum mechanics cannot describe the process of measurement itself." Everett considers this option "somewhat repugnant, since it leads to an artificial dichotomy of the universe into ordinary phenomena, and measurements." (Everett 1955?) Even though in the paper published in 1973 Everett makes a distinction between this view and Bohr's approach, it is quite clear that the two in fact coincide in his eyes. The criticisms he addresses to the Copenhagen interpretation (which is the label he uses to denote what he takes to be Bohr's view) are summarized and developed in a letter written in response to Petersen's appraisal of his paper. In this letter, Everett says that, although the paper addressed "mostly" von Neumann's formulation, he finds Bohr's approach "even more unsatisfactory", although "on quite different grounds".[15] In the next section I will discuss some of the criticisms that Everett addressed to Bohr's view. Also, I will analyse the reasons why the Copenhagen group considered them irrelevant and could not understand the motives of Everett's challenge. ("Most of us — Petersen wrote — [...] don't feel those difficulties in quantum mechanics which your paper sets out to remove."[16])

3. Everett read in Copenhagen

The discussion with the Copenhagen group revolved around four main themes. For each of these themes one can point out genuine divergences as well as misunderstandings, the latter not less interesting and revealing than the former.

3.1. Formalism

The first theme is the nature and scope of theoretical symbols. For Everett, a good theory must provide a "complete *model* for our world":[17] he proposes to take the universal wave function as such a model or, as he says, as the "basic physical entity" (Everett 1957, p. 142). This conception is obviously at odds with Bohr's instrumentalist view, according to which the wave function is a mathematical tool that "expresses the probabilities for the occurrence of individual events observable under well-defined experimental conditions" (Bohr 1948, p. 314). For the Copenhagen group, even on the hypothesis that theories must provide a complete *description* of the world, there was no reason to identify the *predictive* symbols of conventional quantum mechanics with such a *descriptive* model.

Everett contrasted his holistic formulation with the alleged dualism of both von Neumann's and Bohr's approaches. Yet, while Everett's formulation stemmed precisely from the effort to save the link between state vectors and physical states, which was the cornerstone of the "popular" reading of von Neumann's formulation (see van Fraassen 1991a), the Copenhagen view overtly rejected such a link. It would be incorrect, nevertheless, to attribute Everett's dissatisfaction with Bohr's instrumentalism to a naïve realist stance. Indeed, contrary to a widespread opinion, Everett's view had a pragmatic rather than ontological connotation. What inspired Everett's attempt to interpret the quantum theory as an "objective description" was not the yearning to capture the structure of the "real" world putatively underlying the quantum phenomena,[18] but rather the concern to avoid what he regarded as the subjectivist implications of the projection postulate. This is apparent from the methodological appendix in Everett (1973), as well as from the following passage in a letter he wrote to DeWitt:[19]

> When one is using a theory, one naturally pretends that the constructs of the theory are "real" or "exist". If the theory is highly successful (i.e. correctly predicts the sense perceptions of the user of the theory) then the confidence in the theory is built up and its constructs tend to be identified "elements of real physical world". This is however a purely psychological matter. No mental construct (and this goes for everyday, prescientific conceptions about the nature of things, objects, etc. as well as elements of formal theories) should ever be regarded as more "real" than any others. We simply have more *confidence* in some than others.

3.2. Properties

Both Everett and Bohr considered it an important lesson of quantum mechanics that physical systems cannot be endowed with properties "in the absolute". Yet Everett thought of his relative state formulation as the only way to take into account the fundamental relativity of properties without introducing subjective features in physics. Like the "popular" approach, this solution hinged on the assumption that there must be a correspondence between the state vector of a system and its "physical state" (i.e. its "objective properties"; Everett 1973, p. 63). Such a correspondence, as we have seen, is relinquished in the Copenhagen approach. From an epistemological point of view, the Copenhagen solution is more radical: it dismisses the paradigm in which measurements reveal, through a causal chain, pre-existing properties. Indeed, in quantum mechanics, the attribution of properties to a system is consistent with the values taken by the corresponding observables upon measurement only insofar as the observations are confined to certain sets of "compatible" observables, i.e. to *certain* experimental contexts. In contrast, the state vector attributed to a system works effectively as a *meta-contextual* predictive tool, i.e. it predicts the (statistical) *results* observed in all possible contexts. One can understand this situation by assuming that the state vector does not reflect the putative properties of a system, but rather summarises our expectations about what we can observe by performing a measurement on the system.

It should be stressed that *both* kinds of relativization — Everett's relative state formalism as well as Bohr's explicit endorsement of the contextual nature of empirical statements — neutralise the alleged subjectivist implications of the projection postulate. In the former case this happens because, according to the relative state formulation, after a measurement there is no outcome that is more "actual" than the other a priori possible outcomes: all are "actual" relative to some branch of the universe, and this is supposed to eliminate the need to resort to a " 'magic' process"[20] that projects the state vector on to the subspace corresponding to *the* specific property allegedly revealed by the measurement. In the latter case a similar conclusion holds because state vectors are merely predictive symbols that serve to anticipate the results obtained in a well-defined context: if the context undergoes an objective change, as it does *after* an observation, so does the state vector to be used for predicting the results of further observations.[21] This point was emphasized by Groenewold in a letter to Everett:[22]

> Now one can introduce the statistical operator, which just represents in a very efficient way all the information which already has been obtained and which may be used to calculate the conditional probability (with respect to this information) of other information which still may be obtained or used. Thus also the statistical operator is conditional and depends on the standpoint from which the system is described. It is relative like the coordinate frame in relativity theory. It seems to me that this conditional character has been overlooked in your papers (as well as in many others).

3.3. Classical concepts

Bohr's conception of formalism involves a two-level pragmatic dependence on the context. In order to make sense of the formal treatment of a physical problem, it is essential to relate it to a set of experimental operations. But to define these operations in a way that can be communicated and reproduced it is necessary to rely on a suitable conceptual framework, which, according to Bohr, is supplied by the ordinary language and classical physics. This argument is meant to emphasize that physical knowledge cannot be dissociated from the conditions for its possibility. Everett, however, understood it as a *physical* assumption that macroscopic objects behave as classical systems. In the aforementioned letter to Petersen, Everett argued that the Copenhagen interpretation postulated "that macrosystems are immune to quantum effects." And he continued: "The basing of quantum mechanics upon classical physics was a necessary provisional step, but now [...] the time has come to proceed to something more fundamental." (See also Wheeler 1957, p. 151.) Everett's conclusion was that "classical" *concepts* could and should be replaced by "quantum" ones. In this reasoning, there is no room for the *transcendental* status of the conceptual framework: rather than being *presupposed* by any experimental activity, our concepts have to be *derived* from physics. Anticipating a trend that has now become fashionable, Wheeler went as far as sketching an evolutionary argument to the effect that human communication, being an outgrowth of (the complex physical processes underlying) biological selection, could be expected to be described by Everett's model:[23]

> thinking, experimentation and communication — or psychophysical duplicates thereof — are all taken by Everett as going on *within* the model universe.

The way such attempts were regarded in Copenhagen is summarised by Rosenfeld in a letter of 1959:[24]

> Everett's work [...] suffers from the fundamental misunderstanding which affects all the attempts at 'axiomatizing' any part of physics. The 'axiomatizers' do not realize that every physical theory must necessarily make use of concepts which *cannot*, in principle, be further analysed, since they describe the relationship between the physical system which is the object of study and the means of observation by which we study it: these concepts are those by which we give information about the experimental arrangement, enabling anyone (in principle) to repeat the experiment. It is clear that *in the last resort* we must here appeal to *common experience* as a basis for common understanding. To try (as Everett does) to include the experimental arrangement into theoretical formalism is perfectly hopeless, since this can only shift, but never remove, this essential use of unanalysed concepts which alone makes the theory intelligible and communicable.

3.4. Observers

Everett says in a letter that the Copenhagen interpretation, while "safe from contradiction", is to him "hopelessly incomplete".[25] The main reason for this incompleteness is, according to Everett, that the Copenhagen interpretation rules out the possibility of including the observer in the quantum description of phenomena. It is quite clear that Wheeler agreed with Everett on this point, although he was definitely more cautious. In a letter to Bohr, in 1956, he says:[26]

> But I am more concerned with your reaction to the more fundamental question, whether there is any escape from a formalism like Everett's when one wants to deal with a situation where several observers are at work, and wants to include the observers themselves in the system that is to receive mathematical analysis.

In the final version of the dissertation, Everett and Wheeler tried to make clear their worries about completeness by contrasting the relative state formulation with what they called the *external observation formulation*, a label which alluded, if only obliquely, to Bohr's approach. In the external observation formulation, state reduction is brought about by an "observer" (that can be the measuring apparatus itself), which is left out of the theoretical description (by means of an *ad hoc* "cut" in von Neumann's measurement chain). Everett points out that this approach runs into critical problems "in the case of closed universe", for then "there is no place to stand outside the system to observe it. There is nothing outside it to produce transitions from one state to another." (Everett 1957, p. 142.)

For the Copenhagen group, however, the assertion that Bohr's approach ruled out the possibility of treating observers quantum mechanically stemmed from plain misunderstanding:[27]

> I don't think that you can find anything in Bohr's papers which conforms with what you call the external observation interpretation.

The abovementioned letter of Rosenfeld provides some insight into the way this delicate issue was dealt with in Copenhagen:[28]

> The fact, emphasized by Everett, that it is actually possible to set up a wave-function for the experimental apparatus and Hamiltonian for the interaction between system and apparatus is perfectly trivial, but also terribly treacherous; in fact, it did mislead Everett to the conception that it might be possible to describe apparatus + atomic object as a closed system. This, however, is an illusion: the formalism used to achieve this must of necessity contain parameters such as external fields, masses, etc. which are precisely the representatives of the uneliminable residues of unanalysed concepts.

As I will discuss in the next section, however, besides and beyond the argument sketched by Rosenfeld, there is a deeper reason for which including the measurement interaction within the quantum model of a phenomenon is "terribly treacherous". Indeed, this move does not eliminate the reference to a virtual observer (or more precisely to a virtual context of observation) which, in Bohr's approach, is implicit in *any* theoretical description. This is perhaps what Groenewold had in mind when he made the following remarks:[29]

> ... the observer [...] not only "observes" the object system, but also describes it with some theory and "interprets" if you like.
> ... I do not see how your automatical observer included in the described combined system also could be used for describing the activities of reading the recorded measuring result and of assigning statistical operators to the object system on the ground of the obtained information.

As has been shown for example by Dugald Murdoch (1987, Ch. 5), Bohr makes no arbitrary assumption about the *physical* behaviour of systems depending on their size or their position in the measurement chain. What he does make is rather a *functional* distinction between the apparatus (or the observer) *qua* physical system and the same *qua* measuring instrument. This distinction is not *imposed* upon the description of the phenomena, but rather *presupposed* by such a description.

From this succinct account of the debate, one can form an idea of the distance separating Everett's view from Bohr's. Bohr's epistemological analysis, in tune with the Kantian aspects of his philosophical reflection (see e.g. Murdoch 1987; Faye 1991; Kaiser 1992), focused on the *preconditions* of physics. For Bohr, the very possibility of drawing an objective picture of the phenomena presupposed an adequate conceptual framework and it was pointless to try and derive such a framework from the picture itself. His idea of completeness had little to do with the possibility of building a "model of the universe". What counted, instead, was that the theory could answer all the questions that can be meaningfully framed in an experimental context. Everett regarded Bohr's position as extremely dogmatic and conservative, due to the limitations that, in his eyes, it imposed upon the scope of quantum theory. From Bohr's point of view, however, these alleged limitations did not *deprive* us of any relevant information about the phenomena. On the contrary, they expressed a *constitutive* constraint of scientific knowledge. Bohr could see no reason to subordinate the scientific inquiry to the task of providing a *unified symbolic representation* of reality. From his standpoint, it was *this* approach — and not his own — that imposed undue restrictions upon physical theories, and did so in order to satisfy the expectations raised by some "*a priori* philosophical conceptions" (Bohr 1935, p. 696). Petersen appears fully aware of the incommensurability of the two points of view when he writes to Everett:[30]

> Of course, I am aware that from the point of view of your model-philosophy most of these remarks are besides the point. However, to my mind this philosophy is not suited for approaching the measuring problem. I would not like to make it a universal principle that ordinary language is indispensable for definition or communication of physical experience, but for the elucidation of the measuring problem [...] the correspondence approach has been quite successful.

4. Discussion. A cleavage between the micro and the macro world?

Whereas in several respects, van Fraassen's position appears closer to Bohr's than to Everett's, it is clear that on the issue of completeness he shares the doubts raised by Everett and Wheeler. We are now in a position to address the question as to whether these doubts are justified.

Van Fraassen's main concern with the Copenhagen interpretation is that "the appearance of the term 'measurement' in the Born Rule bears its anthropocentric connotations essentially", and seems to imply that "we cannot think of quantum theory as a putative autonomous description of the world in neutral physical terms." (Van Fraassen 1991a, p. 284.) Indeed, even on the minimal hypothesis that the theory only deals with measurement outcomes, one must assume that *there are* definite outcomes. This seems to require that certain macroscopic observables *have* values (exactly those values that correspond to the "pointer states" of a measuring device). The problem is that, in the Copenhagen interpretation, the possibility of attributing a value to a quantum observable is in general contextual, i.e. it depends on the set of experiments that one considers. Hence, in order to fulfil the above requirement, one must conjecture that certain macroscopic observables always *appear* to us as if they had a value, which, in turn, amounts to assuming that the observers who look at the readings of the measuring instruments are bound to choose certain "observation contexts" and not others. Unless one manages to describe the observers as part of a "closed system inside of which the contextual selections are determined by purely physical factors" (van Fraassen 1991b, p. 499), the consistency of the Copenhagen interpretation seems therefore to rest on a formal partition of the physical world into a portion obeying quantum mechanics and a portion, including the measuring instruments, in which the phenomena fit *de jure* the conceptual framework of human beings. Moreover, the "reduction" of the state vector seems to be necessary in order to provide a coherent link between these two kinds of description.

The analysis of Rosenfeld's and Petersen's letters related to the Everett affair supports the hypothesis that the Copenhagen group endorsed, at least partly, the foregoing argument. For example, in a couple of letters to Fred Belinfante,[31] in which he comments on Everett's approach, Rosenfeld asserts that there is "no choice whatsoever about the necessity of applying the [state] reduction" and that "*no* equivalent procedure" can replace it. The reason put forward by Rosenfeld is that "the reduction rule is nothing else than a formal way of expressing the idealized result of the registration"; without it "the phenomenon is not well defined":

> The point is that the measurement is our only way of attaching a meaning to the mathematical symbols of the theory, by associating such symbols with some direct observation (position of a pointer, spot on a photographic plate, and so on).

It must be stressed, however, that neither Rosenfeld nor Petersen excluded in principle the possibility of providing a quantum mechanical account of the process leading to state reduction. Indeed, referring to Wigner's allusions to a special role played by consciousness in the dynamics of observation, Rosenfeld observed:

> However, I suspect that he [...] somehow implies that [the] recording process is not entirely describable by quantum mechanics. This opinion, I maintain, is simply wrong.

For Rosenfeld, the "reduction rule" did not require an *ad hoc* postulate: it could be deduced (in principle) from thermodynamic considerations that applied to *macroscopic* systems. Since the registration is necessary, and since it requires state reduction, which can only be established for macroscopic systems, Rosenfeld concluded that nobody "can avoid committing himself to accepting the necessity of macroscopic measuring instruments." As previously mentioned, in the early 1960's, Rosenfeld supported, against Wigner,[32] the theory of measurement proposed by Daneri, Loinger and Prosperi (1962). In his opinion such a theory, in which the measuring apparatus receives a fully quantum mechanical treatment that takes into account the macroscopic number of degrees of freedom involved, provided a rigorous framework for Bohr's heuristic approach.

> Now, the crux of the problem which worries Wigner so much is that the reduction rule appears to be in contradistinction with the time evolution described by Schrödinger's equation. The answer, which was of course well known to Bohr, but has been made formally clear by the Italians

> [Daneri et. al.], is that the reduction rule is not an independent axiom, but essentially a thermodynamic effect, and accordingly, only valid to the thermodynamic approximation.[33]

However, even admitting that the partition between classical and quantum phenomena does not depend on the experimenter's choices, but is instead determined by some "objective" mechanism, the whole argument is still unsatisfactory from van Fraassen's standpoint, for the following reason (Van Fraassen 1991a, p. 270):

> ... If an interpretation of quantum mechanics resolutely pegs measurement interactions at special macroscopic processes alone, does it not say that quantum theory makes no predictions for what happens in micro processes in the ionosphere? It is one thing to point out that all *our* practically relevant expectations concern macroscopic phenomena; it is quite another thing to interpret the theory as attaching probabilities only to those phenomena at the anthropocentrically important level.

Van Fraassen agrees with the Copenhagen interpretation that "if you ask a quantum physicist to give you some empirical information, he always does the same thing: *he calculates probabilities for measurement outcomes.*" (Van Fraassen 1991b, p. 502.) From this observation he concludes that *if quantum mechanics is to give us information about microprocesses at all*, at least some of these microprocesses must be qualified as measurements. This is exactly what Rosenfeld's approach fails, or rather refuses, to do. But should we take Rosenfeld's as an exhaustive account of Bohr's view?

Let us first observe that in order to understand Rosenfeld's position properly, one should keep in mind that in Bohr's approach, the state vector associated with the measuring apparatus does not represent its putative properties, but rather the probabilities of finding the pointer in any given position *if* a measurement *on* the apparatus is carried out. Rosenfeld's argument is therefore meant to show that the structure of these probabilities is compatible with the assumption that the pointer *has* a well-defined position at any instant. This, in turn, is supposed to ensure that the predictive algorithm of quantum mechanics is compatible with the use of a "classical" conceptual framework to account for measurement results and for the experimental conditions under which these results are obtained. In other words, it is supposed to provide a physical justification for Bohr's doctrine, or at least to show that such a doctrine does not run afoul of the universality of quantum mechanics.

However, as has been emphasized in the preceding section, the deepest part of Bohr's doctrine deals with the *preconditions* of physical modelling. This means that such a doctrine cannot derive its legitimacy from any model whatsoever. Indeed, if one assumes Bohr's instrumentalist interpretation of formalism, a quantum model has meaning only insofar as the predictions deduced from the state vectors refer to the possible measurements carried out by a virtual observer in a well-specified context. The existence of intersubjectively acknowledged facts which indicate a particular result is therefore *presupposed* by any such predictive model, and this applies in particular to any model that is supposed to picture the measuring process itself (see Bitbol 2001).

This simple remark has important implications. First, contrary to Rosenfeld's assertion, and in agreement with the modal account of measurement, no state reduction is required in order to have "definite outcomes". Since state vectors are merely *predictive* tools, insofar as the results of well-defined experiments (involving either micro or macro systems) match the predictions of the (generally entangled) state resulting from unitary evolution, state reduction is superfluous. In general, even the physical model of a particular measurement M will account for the "stability" of the results of M by means of an entangled state, which will predict the results observable in a well-specified meta-context. The Everettian flavour of these conclusions is not purely accidental. Indeed, the project underlying the relative state formulation, namely to provide a naturalized account of the cognitive and social processes by which we come to identify the "facts" on which experimental activity relies, could be regarded as an original way to address the new epistemological issues raised by Bohr's approach. Unlike the thermodynamic approach supported by Rosenfeld, such a project recognized the fact that the conditions for the possibility of experimental activity can be fulfilled irrespective of the hypothesis that macroscopic systems *have* well defined properties.[34]

The second relevant feature of the proposed interpretation of Bohr's approach is that, while it disposes of the postulate of projection, it does so without relying on micro-macro dualism. As in the modal interpretation, the assignment of probabilities to the outacomes of "micro-processes" requires no explicit reference to "macroscopic apparatus". In the modal interpretation, such a reference is avoided by arguing that the standard assignment of probabilities *to measurement outcomes* via the Born rule is only a particular case of the more general assignment of probabilities *to independent physical events* (including microscopic ones). In the approach outlined here, instead, what makes the reference to macroscopic apparatus superfluous is precisely the fact that we *never* assign probabilities to *independent physical events*. All probability assignments refer (if only implicitly) to some possible *result*. And results are defined in terms of standard experimental operations and acknowledged facts, which a physicist will in general *denote* by means of expressions that involve ordinary as well as atomic objects, macro as well as micro processes.[35]

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Keywords

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Resumo

Bas van Fraassen defende uma "variante Copenhagen" da interpretação modal da mecânica quântica. Contudo, ele acredita que a abordagem de Copenhagen à medição não é inteiramente satisfatória, uma vez que exclui a possibilidade de fornecer uma descrição física do processo de observação. Isso também era o que John Wheeler tinha em mente quando, nos anos 50, patrocinou a "formulação dos estados relativos" proposta por seu estudante Hugh Everett. Wheeler, que se considerava um bohriano ortodoxo, tentou convencer Bohr a aceitar o aperfeiçoamento da abordagem de Copenhagen representado, a seus olhos, pela proposta de Everett. Essa tentativa deu origem a um vivo debate, que só recentemente foi documentado, e que fornece um referencial interessante para a avaliação do programa do próprio van Fraassen.

Palavras-chave
Mecânica quântica, problema da medição, interpretação de Copenhagen, interpretação modal, Everett.

Notes

[1] The reference is to Wheeler (1957).
[2] In van Fraassen (1972, p. 332), the external observation formulation is described as the view asserting that "quantum theory always provides a model for a system under study, leaving out the ultimate measuring apparatus (which can just be the observer himself)." Van Fraassen emphasizes that, according to such a view, "any system can be part of the system under study, but the line between system under study and (ultimate) measuring apparatus must be drawn somewhere."
[3] For a thorough discussion of Everett's programme and of its contemporary developments, see Barrett (1999).
[4] John A. Wheeler to Alexander Stern, letter of May 25th 1956. Wheeler Papers, Series 5 — Relativity notebook 4, p. 92. American Philosophical Society, Philadelphia, Pennsylvania, USA.
[5] John A. Wheeler to Hugh Everett, letter of May 22nd 1956. *In* Wheeler Papers, Series I – Box Di – Fermi Award #1 – Folder Everett.
[6] *Ibidem.*
[7] A detailed reconstruction of the meeting can be found in Freitas (2007) and Osnaghi, Freitas & Freire 2009.
[8] Bohm, who was a Marxist, had been forced to leave in the context of McCarthyism; see Freire (2005).
[9] Hugh Everett interviewed by Charles Misner, May 1977. Everett Papers, Series I-3, [CHP-AIP]. Niels Bohr Library, Center for History of Physics, American Institute of Physics, College Park, Maryland, USA.
[10] *Ibidem.*
[11] Von Neumann (1955). For a discussion of von Neumann's own interpretation of the projection postulate, see Becker (2004).
[12] Hugh Everett to Bryce DeWitt, letter of May 31st 1957. *In* Wheeler Papers, Series I – Box Di – Fermi Award #1 – Folder Everett.
[13] In order to screen off the projection postulate from the probabilistic interpretation, Everett should recognize the central role played by the eigenvalue-eigenvector link in the conventional formulation (see van Fraassen 1991a), which he does not. The fact that Everett does not put into question the existence of a straightforward link between the state vector and the physical state of a system is central to understanding why the relative state and the modal accounts of measurement, while both dismissing the postulate of projection, still differ substantially from each other. As summarised by van

Fraassen (1991, p. 494): "The many-worlds interpretation 'modalizes' the collapse of the wave packet, while the modal interpretation 'modalizes' the ignorance interpretation of mixtures." For a penetrating analysis of the analogies and differences between the two strategies, see Bitbol (2000, p. 271–85).

14 Everett to DeWitt, *op. cit.*

15 Hugh Everett to Aage Petersen, letter of May 31st 1957. Wheeler Papers, Series I – Box Di – Fermi Award #1 – Folder Everett.

16 Aage Petersen to Hugh Everett, letter of April 24th 1957. Wheeler Papers, Series I – Box Di – Fermi Award #1 – Folder Everett.

17 Wheeler to Stern, *op. cit.*

18 It is worth noting that the terms "real" and "reality" appear in quotes all throughout Everett's writings (including letters).

19 Everett to DeWitt, *op. cit.*

20 Hugh Everett to Max Jammer, letter quoted in Jammer (1974, p. 508).

21 The *pragmatic* nature of the constraints from which the collapse of the wave function originates is discussed for example in van Fraassen (1972). As we will see in the next section, Bohr's view can be construed in a way that does not require any collapse at all, just like the modal interpretation. Bohr was indeed reluctant to express his opinion on the projection postulate (see Teller 1981), arguably because he thought that focusing on such an issue in discussions about measurement was misleading (see Bohr 1939; see also Rosenfeld's remarks on von Neumann's presentation in Osnaghi, Freitas & Freire 2009).

22 Hip Groenewold to Hugh Everett and John A. Wheeler, letter of April 11st 1957. *In* Wheeler Papers, Series I – Box Di – Fermi Award #1 – Folder Everett.

23 Wheeler to Stern, *op. cit.*

24 Léon Rosenfeld to Saul Bergmann, letter of December 21st 1959. Rosenfeld Papers, The Niels Bohr Archive, Copenhagen, Denmark.

25 Everett to DeWitt, *op. cit.*

26 John A. Wheeler to Niels Bohr, letter of April 24th 1956. Bohr Scientific Correspondence, reel 34, Archives for the History of Quantum Physics, American Philosophical Society, Philadelphia, Pennsylvania, USA.

27 Petersen to Everett, *op. cit.*

28 Rosenfeld to Bergmann, *op. cit.*

29 Groenewold to Everett & Wheeler, *op. cit.*

30 Petersen to Everett, *op. cit.*

31 Léon Rosenfeld to Frederik Belinfante, letters of July 24th 1972 and August 24th 1972. Rosenfeld papers. I am grateful to Olival Freire Jr., who called my attention to these letters.

32 See Rosenfeld (1965). For an account of the controversy between Rosenfeld and Wigner, see Freire (2007).

33 Rosenfeld to Belinfante, July 24th 1972, *op. cit.*

[34] This does not mean that the statistical predictions deduced from quantum models of macroscopic objects cannot exhibit structural features that are consistent (for all practical purposes) with the predication of ordinary properties, as Rosenfeld claimed. But in the pragmatic-transcendental reading of Bohr's approach (Bitbol 1996; Osnaghi 2005), this remarkable result should be understood as an indirect consequence of the constraints imposed by the ordinary conceptual framework upon the mathematical structure of predictive models. This means in particular that the convergence of the state vector of macroscopic systems towards a mixture of pointer states should not be regarded as a fortuitous empirical fact, a one which would explain the "emergence" of our ordinary concepts from quantum mechanics (Zurek 1991). Indeed, since the structure of the latter is not a priori independent of the former, they can be expected to be consistent with each other "by construction".

[35] Two remarks are in order. First, the proposed interpretation of Bohr's ideas in no way purports to provide a faithful and historically consistent account of his thought (though, of course, it is meant to capture and to develop some of Bohr's philosophical intuitions). Second, even though the account of measurement which results from such an interpretation manages to avoid the dualism pointed out by van Fraassen, one may still object that it is strongly anthropocentric. Indeed, in such an account, the paradoxes are dissolved by explicitly endorsing the central place of human practice at any stage of the process of acquisition of knowledge. Thus, for example, experimental facts are *defined* by their concrete implications within the well-established practice of physicists (Osnaghi 2009). Likewise, theoretical models have meaning only insofar as they refer to the possible situations that a virtual observer can experience within such a practice. This approach might hardly appear attractive from the point of view of van Fraassen's "model-philosophy". Even though a constructive empiricist would not deny that the scientific enterprise, as well as its models, its criteria of success or even its objects, depend ultimately on our conceptual and pragmatic framework, he would probably not let such a dependence enter the scientific game explicitly. For him, any decent solution of the measurement problem should take for granted the criteria that science itself is supposed to use to define its goals and methodology, including, I guess, what Pauli called the "ideal of the detached observer" (letter to Bohr of February 15th 1955; Pauli 1994, p. 43).