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A Story Without an Ending: The Quantum Physics Controversy 1950–1970*

OLIVAL FREIRE JR.
Physics Institute – UFBa, Salvador, 40210-340, Brazil; E-mail: freirejr@ufba.br

Abstract. For many decades there has been controversy about the epistemological and ontological implications of quantum mechanics. This article will make some preliminary remarks about how physicists managed the scientific, philosophical, and even political aspects, of the controversy in order to establish a *modus operandi* for their work. The existence of such a lasting controversy, and its history, is relevant to the teaching of quantum physics because the choice of the interpretation to be taught impacts on students' understanding of quantum concepts and on their understanding of the goal of science.

1. Introduction

Thirty years ago, Max Jammer (1974, p. 521) finished his book on *The Philosophy of Quantum Mechanics – The Interpretations of Quantum Mechanics in Historical Perspective*, calling that history "a story without an ending". This label remains adequate to this almost eighty-year-old controversy, to which Jammer applied the advice of the French moralist Joseph Joubert: "It is better to debate a question without settling it than to settle a question without debating it". In this paper I would like to investigate, from a historical point of view, since when and why physicists could not settle that question; or, in other words, since when and why that controversy has become a legitimated and institutionalized scientific controversy without foreseeable termination.

A first approach to the history of the controversy about the interpretation of quantum theory may suggest that it is as old as the creation of that physical theory itself. Through a closer investigation, however, one can identify, *grosso modo*, three different periods in that history. The first and the third ones can be grasped more directly than the second, intermediary, one.

The first period goes up to the end of the 1940s. It can be characterized, according to Jammer's (1974, pp. 247–251) words, as the epoch of the "almost unchallenged monocracy of the Copenhagen School in the philosophy of quantum mechanics". In spite of the first disputes (1925–1927) and of Einstein's and Schrödinger's criticisms in the 1930s, physicists largely adhered to Niels Bohr's complementarity interpretation. Besides, those criticisms were considered by most physicists as a strictly philosophical quarrel. Max Planck, for instance, saw quantum controversies as the "determinism quarrel".[1] That monocracy around Bohr's ideas plus the diffuse idea of a philosophical quarrel prevent us from talking – in a strong sense – of a constituted scientific controversy in the physicists' community.

The third period, which includes the present time, can be identified as the physics community recognising and legitimating the existence of a scientific controversy about the interpretation of quantum physics, and recognising that the controversy has philosophical implications. This recognition implied an institutionalization of the controversy, with the creation of scientific journals and meetings aimed at supporting the debates. So, there was actually a transition. The physics community on the whole changed its attitude towards the problem of the interpretation of quantum physics, shifting from considering it as a 'metaphysical' controversy to admitting its legitimacy as a scientific controversy with philosophical implications.

The second period, is a transition one, it stands between the first and third periods. My interest in the history of this transition is twofold: first, to understand when it occurred, and second, to understand how the physics community managed with the scientific, philosophical, and even political aspects of the controversy, in order to establish a *modus operandi* for their work.

About those political aspects, Jammer (1974, p. 251) remarked that the manner in which the opposition to Copenhagen interpretation "was fomented and supported by social-cultural movements and political factors, such as the growing interest in Marxist ideology in the West, deserves to be investigated just as diligently as the influence of the 'Weimar culture' on early quantum theory has recently been studied". I can synthesize the problem I want to study in a more general way than Jammer's remarks. I want to know how the physicists, in that intermediary period, managed the connections between science, philosophy and politics, and how, and when, this process came to constitute the "quantum controversy" as a scientific controversy with philosophical implications.

The existence of such a lasting controversy, and its history, is relevant to the teaching of quantum physics. Research on quantum physics teaching and learning is quite a recent matter (Greca and Moreira 2001). Some of these recent results (Greca and Herscovitz 2002; Greca and Freire 2002) suggest that – in introductory quantum physics teaching – the choice of epistemological and ontological interpretations could be effective in enhancing students' understanding on quantum concepts. As this choice is a philosophically loaded choice, physics teachers can make that choice on a better basis if that controversy becomes intelligible for them, and history of science should help them to acquire intelligibility on that subject. Besides the challenge of learning quantum concepts, the education of physicists and physics teachers should enable them to grasp the most important controversy in 20th Century physics, and one of the most important since 17th Century's dispute between Newton and Leibniz, or between Newtonians and Cartesians. The controversy is a chapter of science in which science, philosophy and politics are singularly blended. Historical study is clearly required to recognise and understand the blend.

2. The First Period: The Monocracy of the Copenhagen School

This period begins in 1927, when the Solvay Council is closed with wide agreement with the complementarity interpretation, elaborated by Niels Bohr; and it goes up to the end of the 1940s. At that time, in spite of the disputes around 1927 and the ongoing opposition of Schrödinger (1935) and Einstein (1935), the ambiance was marked by the large adhesion to complementarity interpretation, but we must recognize that the main epistemological questions, and even theoretical-scientific questions, which will constitute the agenda of the controversy, were posed during this time. This way, for instance, in Schrödinger's cat argument we found an essential evidence of the measurement problem and of the quantum-classical transition problem. In the same way, in Einstein's works we found his dissatisfaction with the defense of the completude of quantum theory by Copenhagenners and with the implicit non-locality in quantum mathematical formalism. The dissatisfaction with the abandonment of classical determinism was more diffused, whence the Planck's terms: the "determinism quarrel".[2]

To understand the spirit of those times, one needs to take into account that physicists in general, even those complementarity supporters, saw those criticisms rather as expressions of a strictly philosophical dispute concerning ontology (the constitution of real physical objects – waves, particles, the role of the space-time description) and epistemology (the status of determinism in physical theories, the completude of theories). In that context, 'metaphysical' was used as an adjective defining disputes without implications for the development of physics. The physicists of the time did not think, for instance, of submitting Einstein's and Schrödinger's criticisms to experimental tests. So, it was in this way that, at the end of that period, Wolfgang Pauli and Léon Rosenfeld described Bohm's (1952) proposal as 'metaphysical', while Werner Heisenberg termed it 'ideological'.[3]

Thus, naming this period of the controversy as the 'metaphysical' stage has a double meaning: it was considered by physicists at the time as a philosophical controversy, and thus as a controversy without implications for physics. The supporters of complementarity presented dissatisfaction with the foundations of quantum theory as 'metaphysical' because this could put critics outside debate in physics.

Two instances illustrate the ambiance of the period. In 1932, the Hungarian mathematician Janos von Neumann published his book *Mathematical Foundations of Quantum Mechanics*. In this book he revealed the mathematical structure that has become the reference to an axiomatised view of this physical theory. The book also includes a mathematical 'proof' of the impossibility of including additional variables in quantum theory. This result of von Neumann's was seen as making impossible the creation of an alternative – but consistent and empirically equivalent

– interpretation of quantum theory.[4] Twenty years later, the Belgian physicist Léon Rosenfeld (1953) – who was Niels Bohr's assistant for decades, even while he was out of Copenhagen, and who was one of the first to react against Bohm's causal interpretation – considered complementarity as a direct consequence of experience. He claimed that a physicist that "refuse de se rendre . . . l'évidence de la complémentarité, abandonne l'attitude rationnelle de l'homme de science pour prendre, qu'il le veuille ou non, celle du metaphysicien'. So, in that period, physicists denied that the quantum controversy was a scientific controversy, it was seen as a metaphysical or philosophical quarrel.

3. The Third Period: Institutionalized Scientific Controversy with Philosophical Implications

The third period, which includes the present time, can be characterised by the physics community recognizing and legitimating the existence of a *scientific* controversy about the interpretation of quantum physics, and recognising that the controversy has philosophical implications. There has been an institutionalization of the controversy, with the creation of scientific journals and meetings that supported the debate.

If one consults specialized literature in physics published in the last five years, and looks up the keyword 'Bohmian mechanics', one will find a significant number of works. However the physicists who work in this domain use a more technical and less philosophical term than the term – 'causal interpretation' – used by Bohm himself in the fifties. As a matter of fact the historical continuity between these new works and Bohm's first paper is not so strong. One can even speak of a renewal of interest in Bohm's interpretation since Philippidis, Dewdney and Hiley, in 1979, used new computer resources in order to simulate the trajectories predicted by the causal interpretation.[5] Since then, in a stronger way in the last decade, getting those trajectories have motivated physicists, who argue that one could get a better intelligibility of quantum phenomena with these simulations.

Further evidence of the institutionalisation of the debate is provided by the existence of two fields of research in theoretical and experimental physics - Bell's inequalities and Decoherence – that are related to the controversy, and which mobilize a large number of physicists. Even the occurrence of an invited talk, by J. F. Clauser, on the "Early History of Bell's Theorem", at the Eighth Rochester Conference on Coherence and Quantum Optics, June 2001, has a symbolical meaning for the status of the subject in the mainstream of contemporary physics.[6] The confirmations, some years ago, of the quantum decoherence effect (Haroche et al. 1997), can be inscribed in a more general field of physics: the measurement problem. An essential reference in this last field was the volume organized by Wheeler and Zurek (1983) gathering together the most important papers in the field. That volume included the English translation of papers originally written in French or German. The organization of this volume is a typical procedure of managing a controversy already legitimated: putting at the disposal of the researchers the basic documents that constitute the corpus of the controversy.

Concerning Bell's inequalities, the analysis of a recent paper, with new experimental results, can help us to understand what I have named a scientific controversy with philosophical implications. Take the letter published in *Nature*, by M. A. Rowe (2001) and collaborators, entitled "Experimental violation of a Bell's inequality with efficient detection". First of all, nowadays a letter in a prestigious scientific journal is bound to be noticed in the mainstream of physics. The authors start the paper defining "local realism", then they write, "many experiments have since been done that are consistent with quantum mechanics and inconsistent with local realism. But these conclusions remain the subject of considerable interest and debate, and experiments are still being refined to overcome 'loopholes' that might allow a local realistic interpretation". The many experiments refer to are eleven experiments carried out between 1972 and 1998. So, we have a genuine scientific controversy about whether or not the existent experimental results are enough to exclude the possibility of a "local realistic interpretation"; and this controversy has philosophical implications because concepts such as realism and locality are at the centre of the controversy.

4. The 'Intermezzo' Period: The Transition from a 'Metaphysical' to a Scientific Controversy

The transition began in 1952, when David Bohm suggested an alternative interpretation to the then orthodox Copenhagen one. He called it the hidden variables or causal interpretation; it was meant to recover determinism in science. As a matter of fact, not only did he suggest a new interpretation, but he also modified the quantum formalism in order to build a model of particles with well-defined trajectories, something which was prevented by standard quantum theory. Although it gathered adherents such as Louis de Broglie, Jean-Pierre Vigier and, for a brief period, Mario Bunge, the causal interpretation by no means became dominant.[7]

Bohm's proposal had, however, an important side-effect: it motivated John Bell to re-examine von Neumman's proof against the possibility of introducing new variables in quantum theory.[8] Because of some vicissitudes, Bell only resumed this problem ten years later. He showed the flaws of the proof, evidencing that in effect there is a contradiction between quantum theory and hidden variables, but not hidden variables in general (as the one suggested by Bohm). The contradiction occurs only with a certain family of hidden variables, namely local hidden variables. Interesting enough, locality was precisely the criterion Einstein considered, in 1935, inherent to fundamental physical theories.

Bell's work (1964) pointed to an important change in the controversy: the possibility of including experimental physics in order to reject some theories and preserve others. Bell's inequalities carried with them the end of any reminiscence of the metaphysical stage. However, only some years later this possibility was grasped by physicists. It was with a paper by Clauser et al. (1969) that this possibility was explicitly announced. This paper stimulated, in the next decade, the conduct of several different experiments. As physics is a discipline with strong experimental appeal, this paper signalled to the physics community that the quantum controversy could be shifted from the boundaries to the mainstream of the field. The possibility of use of new and sophisticated techniques reinforced that view.

It is thus interesting to analyze the case of the French physicist Alain Aspect, who performed the most accurate experiments, the results of which were published in 1982. Two characteristics of Aspect's work deserve our attention: First, Aspect had announced his plans to perform this experiment some years before, in 1976, as part of his doctoral thesis; second, he considered himself as a physicist insensitive to the epistemological questions related to the interpretation of the quantum theory.[9]

After staying in Africa as *coopérant*, Aspect returned to the Institut d'Optique, in Orsay, and needed a subject for his thesis work. Bernard d'Espagnat and John Bell suggested the experiment, and helped him to get the funds to carry it out [see D'Espagnat, interview, 2001, American Institute of Physics]. I see this case as evidence of a legitimated scientific controversy, even with philosophical implications, because the themes in dispute could mobilize fresh talents of a scientific community to perform an experiment that had been considered as an essential advancement because of the experimental accuracy it would require. In effect, such a scientific task does not demand epistemological inclinations but can appeal to fresh talents because it can open the main doors to a successful scientific career.

It is easy to explain the change of attitude among physicists as a consequence of the impact of Bell's work and its related experiments, because Physics is a discipline with a strong experimental appeal and it could not be insensitive to Bell's inequalities experiments. We need to ask, why in a milieu that considered questions concerning the interpretation of quantum theory as 'metaphysical', was Bell's work elaborated, analyzed, and developed? One must take into account, for instance, the five years between the publication of Bell's seminal paper and the realization of its full experimental implications, by Clauser, Horne, Shimony and Holt in 1969.

A good question is to ask why, how and when physicists such as Bell, Clauser, D'Espagnat, and Shimony put their effort into the subject. In order to answer the question, we need to move back on the time line to search for the evidences of attitude changes among physicists before the explosion of experimental tests with Bell's inequalities from the second half of the seventies on. Clauser (1992), who was one of the protagonists of the change, has suggested, in a reminiscences' paper, that one should see the conference held in 1976, in Erice, Italy, and devoted to the analysis of Bell's inequalities experiments,[10] as the turning point of that change. He wrote: "the sociology of the conference was as interesting as was its physics. The quantum subculture finally had come 'out of the closet' and the participants included a wide range of eminent theorists and experimentalists. The consensus was that QM is now far more perplexing than it was before 1964".

Some years before that conference one could already find evidence of the existence of a genuine, legitimated, and even institutionalized scientific controversy. I would like to suggest 1970 as the year when the transition finished, because two events, in that year, one in the USA and the other in Europe, already seem to evidence the change in the controversy. The first one was the creation of *Foundations of Physics*, and the second one was the theme of the International School of Physics 'Enrico Fermi'.

The scientific journal *Foundations of Physics* appeared in 1970 with the aim of being the vehicle for debates in the field designated by its title, and above all, theoretical debates related to quantum physics. The journal's editors were Henry Margenau from Yale University and Wolfgang Yourgrau from the University of Denver. Its Editorial Board comprised physicists who, two decades before, were on opposite sides in quantum disputes. Thus we find David Bohm and Louis de Broglie, former causal supporters, side by side with V. A. Fock, who was near the complementarity interpretation. Besides them, the philosopher Karl Popper and the physicist Eugene Wigner, who had such different epistemological views on quantum physics, were in the Board. Even while the journal was aimed at foundations of physics in general, in the first volume, 16 out of the 18 papers dealt with quantum themes. The purpose of the Editorial Preface was to define what the editors understood by 'foundations', evidencing how far this word was from dominant preoccupations among physicists at the time. The editors presented some arguments meant to convince physicists that a journal as *Foundations of Physics* could be useful to physics in general and not only to the foundations of physics. They argued that "scientific understanding can be stalled because of infelicities in its foundations"; and, after reminding readers of the role of the problem of "observability" in both theory of relativity and quantum theory, they concluded that this threat was actual. They went on to say that: "Some believe that the risk of overlooking such defects is great in our days when the need and the actualities of public support place an excess of emphasis upon the pragmatic aspects of science. If this is true, compensating attention to matters of foundations is an important necessity".[11]

It's interesting to compare the last paragraph of the Editorial Preface with the dominant ambiance in the fifties, when the causal interpretation faced the criticism of not leading to new experimental results. In *Foundations of Physics* pure speculation would not only be tolerated but even stimulated. The editors finished the preface writing: "very few scientific journals today encourage speculation not tied to hard and demonstrable facts. One wonders whether brilliant ideas are not lost by this restrictive attitude. *Foundations of Physics* will publish with suitable frequency disciplined speculations suggestive of new basic approaches in physics".

Also in 1970, between 29 June and 11 July, the Italian Society of Physics, with Toraldo di Francia as its president, organized one of its traditional courses of the International School of Physics "Enrico Fermi". Such courses had been regularly carried out since 1953, in the summer, in Varenna, on Como Lake. The 1970 course had the title "Foundations of Quantum Mechanics". The French physicist Bernard d'Espagnat was invited to be the head of the course, which had 84 participants. The "Proceedings" (D'Espagnat, 1971), with the lectures, reveal a diversified spectrum of themes organized in three great topics: Measurement and Basic Concepts; Hidden Variables and Nonlocality, and Interpretations and Proposals. A similar diversity is found among the lecturers, including some of the protagonists of the controversy, such as Wigner, Jauch, Shimony, d'Espagnat, Bell, De Broglie, Selleri, and Bohm. It's interesting to remark that if in the first volume of the *Foundations of Physics* the analysis of "Bell's Inequalities" did not deserve attention, the opposite happened in Varenna, where several lectures focused on the subject.

In choosing d'Espagnat to lead the course, Toraldo di Francia could not have made a better choice, because it meant the right man at the right place. His interest in Philosophy and Science come from the high school in which he got a *Baccalauréat* in Mathematics and Philosophy. After succeeding in a Physics career, he began to spend time on the subject concerned to the foundations of quantum physics. As early as 1965 he published his first book on the theme, and the next year he published a paper on quantum measurement in the Italian journal *Nuovo Cimento* (D'Espagnat, 1965, 1966). [12] He and Bell became very close since they recognized each other, while working in CERN, as being strongly interested in the foundations of quantum physics. As we will see, d'Espagnat had diplomatic skills, besides scientific ones, to deal with the controversy.

Bernard d'Espagnat (1971, pp. xiii–xiv) sent a preliminary letter of invitation to the participants, which is an exemplary evidence of how the controversy was being constituted. Firstly, he criticizes the instrumentalistic view on physical theories, maintaining that "like the Delphic oracle theoretical physics rests on three legs: experience, mathematics and a workable set of general ideas. Some would like to cut this third leg away. [...] of course they are quite wrong". It meant that refuting instrumentalism was a premise to deal with the controversy. Next, he defines the controversy: "[But] it will probably become quite clear, in a few days, that under a superficial agreement on how to use the rules we have learnt, we entertain real differences of opinion as to what these rules refer to". D'Espagnat proposed then an agreement that one could see as diplomatic rules to be followed in order to have a pacific and creative coexistence in situation of scientific controversies. Here is the suggested agreement:

1. we should not take as our goals the conversion of the herectic but rather a better understanding of his standpoint;
2. we should not suggest that we consider as a stupid fool anybody in the audience (lest the stupid fools should in the end appear clearly to be ourselves!);
3. we should try to cling to facts;
4. nevertheless, we should be prepared to hear without indignation very nonconformist views which have no immediate bearing on facts

It's noteworthy that in spite of the differences between the *Foundations of Physics* and the Varenna's course, both needed to face the same task in order to justify their existence: to argue against the intrumentalistic view of science. The decade after the creation of *Foundations of Physics* and the staging of the Varenna course have confirmed how opportune they were. Bernard d'Espagnat (1976) published his *Conceptual Foundations of Quantum Mechanics*, which have become a reference text, even for non physicists, to introduce the subject under dispute. In 1972, Clauser (2001) introduced Bell's inequalities in a typical 'normal' science meeting: the 3rd Conference on Coherence and Quantum Optics. Experiments on "Bell's Inequalities" mobilized physicists,[13] and Aspect's experiment, which was seen by most physicists as a conclusive test for quantum theories and against local hidden variable theories, had significant repercussions, even outside of physics.

5. Connections Between Science, Philosophy, and Politics

It is appropriate to mention some reasons which could have contributed to the history of the interpretative controversy. During the 1950s and 1960s, in parallel with, and in some cases independent of, the dispute between the partisans of complementarity and causal interpretations, some studies appeared concerning interpretation problems and, in a more general way, foundation problems in quantum physics. Almost all these works were developed by physicists who were not involved in the creation of this theory, who were not its founding-fathers, or involved in the causality versus complementarity dispute. These studies included Popper's (1957), Landé's (1953, 1965) and Ballentine's (1970) works, which represented alternative interpretations to both complementarity and causal interpretations.

In the same way one can list Bunge's work since 1964, who, while axiomatising quantum theory, understood that the chief issue was not the recovery of determinism – a task he was attached to, with Bohm in the fifties – but of ejecting the observer.[14] The doctoral thesis of Everett III, in 1957, is very interesting from this standpoint. The thesis created a great problem for complementarity. He tried to use quantum mechanics to describe the whole universe, which, as we know, could only be described via general relativity. So, Everett was pioneered the unification of quantum and gravitation theories. In doing so, Everett was led to give up the role attributed to the means of observation by complementarity supporters. In effect, Everett was building a quantum theory without reduction of the wave function. This result pointed to the existence of a domain to which complementarity could not even be applied, and caused a splitting of opinions among Copenhagenners. He was advised by J. A. Wheeler, who was close to Bohr, and his thesis was discussed by Bohr.[15] He was cautious about Everett's result, but his long-time assistant, Léon Rosenfeld, put more emphasis on his criticism of Everett's work.[16]

Analogous problems were created by physicists such as Wigner (1963), who developed a most sophisticated account of mind-body interactionism and who was fully committed to the inherent subjectivity of quantum measurement whereby the observer's impact cannot be eliminated; as Daneri, Loinger and Prosperi, who in 1962 thought of solving the measurement problem by the idea of thermodynamical amplification, an idea that was quickly criticized by several physicists.[17] In the early nineteen fifties, Gleason, and Jauch and Piron tried to change von Neumman's proof in order to liberate it from the criticism implied in the existence of a hidden variables theory, compatible with quantum theory, as that suggested by David Bohm.

The works of Wigner, on one side, and of Daneri, Loinger and Prosperi, and Rosenfeld (1965), on the other side, are of special relevance for our story, because they enhanced that clash existing between complementarity's supporters, born from the issue of how to know what is the quantum standard solution to the measurement problem. The consensus about how to interpret the quantum theory vanished. One can find evidence of physicists' recognition of the dispute in the words of Frisch (1971, p. 14), opening a colloquium in 1968: "I understand that at present there exists a controversy, roughly speaking, between a group of people which includes Wigner as the best known person and another group centred on Milan [Daneri, Loinger and Prosperi], and that these two have different views on how this reduction [of the wave function during a measurement] happens".[18]

The repercussion of those works among physicists deserves a careful study. I think that such a study would be noteworthy because it is meaningful to think of the appearance of Bell's Inequalities and the changes in the controversy as being a qualitative jump resulting from slow changes in the sensibility of physicists to problems in the foundations of quantum physics.

The existence of political and ideological trends in the controversy on quantum physics must first be seen as a consequence of the strong philosophical implications of quantum theory. Besides themes not so ideologically loaded as locality, we find others more explosive such as causality, determinism, realism, and objectivity. So, it is not strange to find, since the nineteen thirties, ideological repercussions of the theory. It was, however, after World War II, in the context of the Cold War, that these ideological and political implications grew quickly and strongly. It's against that political background that one must see the semi-official campaign, commenced in 1947, against complementarity in the former USSR. Several aspects of this problem have been studied from a historical point of view,[19] but we do not yet have a comprehensive study of it.

Second, we need to recognise that there is not a univocal relationship between epistemology and ideology and politics. In the nineteen fifties and sixties, there were Marxists who criticised complementarity, namely Bohm, Vigier, Blokintsev, and Terletski; Marxists who supported complementarity, namely Rosenfeld, Fock, and Haveman; critics of complementarity indifferent to Marxism, namely Einstein and de Broglie; supporters of complementarity indifferent to Marxism, namely Bohr; and critics of Marxism supporting complementarity, namely Born,[20] Heisenberg, and Jauch. The most probable reason for this heterogeneous response, is that suggested by Michel Paty: "une position épistémologique peut s'arranger de diverses idéologies, mais elle peut évidemment, chez des individus singuliers, se lier à l'une particulière pour des raisons qui sont plutôt idiosyncratiques mais non universelles".[21]

This theme deserves more careful research, and the existence of diverse relationships between ideology and epistemology in individuals can be compatible with a degree of relationship at a collective or group level – there could be trends with individual deviations – that would allow a positive answer to Jammer's problem[22] (quoted in Section 1 of this paper). In any way, it seems that the institutionalization of the controversy was useful to its development, because it permitted the active participation of all protagonists, independently of their political or ideological engagements.

The question suggested by Jammer could be enlarged to take into account the attitude towards the controversy taken by the new generations of physicists, specially in the 1960s, and the effect on these generations, and thus indirectly on the quantum mechanics controversy, of the cultural and political turmoil in the American and European universities at the end of the 1960s.[23] But this is a separate undertaking.

Notes

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1 Planck's views were expressed in *Determinimus oder Indeterminimus*, Leipzig, Barth, 1938.

2 Einstein's and Schrödinger's criticisms must not be identified with dissatisfaction with the collapse of classical determinism. See, respectively, Michel Paty (1995), and Michel Bitbol, in the French translation of Schrödinger (1935), *Physique quantique et représentation du monde*, Editions du Seuil, Paris, 1992, note 1, pp. 140–1.

3 See Kragh (1999, p. 207); and Freire Jr. (1999, pp. 73–82).

4 Kragh (1999, p. 214), remarked that "in spite of von Neumann's words of caution, his mathematical proof was accepted widely and sometimes taken to be a proof of the Copenhagen interpretation". Only in the sixties, with Bell's works, would the flaws in this 'proof' be understood.

5 C. Philippidis et al., *Nuovo Cimento* 52B (1979), 15–28.

6 I thank J. F. Clauser for sending me a copy of that talk.

7 Several studies have appeared concerning Bohm's proposal. See Cushing (1994), Olwell (1999), Freire Jr. (1999), Mullet (1999), and Freire, Paty and Barros (2002). They present different accounts of its first unfavorable receptions. Cushing argues with historical contingence, i.e., when Bohm's ideas were published physicists had already adhered to complementarity; Freire and colleagues emphasize the fact that Bohm and collaborators did not get any new results in order to contrast their proposal with complementarity; Olwell and Mullet emphasize, in their reports, Bohm's insulation from American physicists' community after he was prosecuted by McCarthyism and left USA for Brazil.

8 Bell (1982), wrote 30 years later: "In 1952 I saw the impossible done". We have, however, independent evidence of Bell's early interest in Bohm's work, because he wrote to Pauli at the end of 1952 or the beginnings of 1953, asking for Pauli's papers on Bohm's ideas. Pauli answered, on 23 January 1953, suggesting his paper on the subject in the anniversary volume for Louis de Broglie. See Letter number 1510 in Pauli (1999, p. 28).

9 Alain Aspect – Interview with the author, Orsay, 20, January, 1992.

[10] The conference was hosted by Antonio Zicchici as an International 'Ettore Majorana' Conference in 18–23 April 1976. Its title was *Thinkshop on Physics*, its scientific heads were B. D'Espagnat and J. S. Bell, and the invited speakers included A. Aspect, Eberhard, J. F. Clauser, Fry, Horne, Imbert, Lamor-Rechti, S. Notarrigo, Pipkin, A. Shimony, V. L. Telegodi and J. D. Ullman.

[11] See *Foundations of Physics*, **1**, 1970, Editorial Preface.

[12] It would be interesting to study the evolution of D'Espagnat ideas at that time and its repercussion. L. Rosenfeld, for instance, seemed to approve the book, but not the paper in *Nuovo Cimento*. In 26 February 1966, D'Espagnat wrote to him: "Je tiens à vous remercier pour [...] l'approbation que vous avez la gentilesse d'y exprimer à l'égard de mon livre". Four months later, Rosenfeld wrote to him, in 8 July 1966: "Votre dernier travail 'Two Remarks on the Theory of Measurement' semble indiquer que vous avez besoin de vous retremper dans l'air pur de Copenhague [D'Espagnat had been there in 1954 and had received, in January 1966, an invitation of Rosenfeld to return there]. Il n'y a rien de tel comme cure de cette wignérite dont vous paraissez subir une atteinte, que j'espère légère". Those letters are at *Rosenfeld Papers*, in Niels Bohr Archive, Copenhagen. The papers are not catalogued yet.

[13] For a critical account of these results and of the meaning of these experiments, in the middle of the 1970s, see Paty (1977).

[14] "Then, in 1964, when I started working on the axiomatization of NRQM for my Foundations of Quantum Physics (Springer, 1967), I realized that Bohm's was not a valuable addition to standard QM, [...] It was not a question of injecting causality, but of ejecting the observer." Letter of Mario Bunge to the author, Montreal 01 Nov 1996.

[15] Bohr wrote to Wheeler, in 12 April 1957: "It appears to us that the argumentation contains some confusion as regards the observational problem and one of these days Aage Petersen will write to Everett about our discussion". Bohr Scientific Correspondence, in *Archives for the History of Quantum Physics*.

[16] In 21 December 1959, Rosenfeld wrote to Saul M. Bergmann, who had asked for an opinion about Everett's point of view: "The fact, emphasized by Everett, that is actually possible to set up a wave-function for the experimental apparatus and a Hamiltonian for the interaction between system and apparatus is perfectly trivial, but also terribly treacherous". *Rosenfeld Papers*.

[17] To the history of measurement problem, see Pessoa Jr. (1992). To the originals of the referred works, see Wheeler and Zurek (1983).

[18] I thank Aurino Ribeiro for calling my attention to that colloquium's volume.

[19] See Graham (1972); Jammer (1974, pp. 247–251); Cross (1991), and Freire Jr. (1993, 1997, 1999).

[20] For this interesting, and not well known case of a political position contrary to Marxism and USSR reinforcing the defense of complementarity, see Freire Jr. (2001).

[21] Michel Paty, Letter to the author, Paris, 24 February 2001.

[22] I have studied (Freire Jr., 1997) the diversified incidence among Marxist physicists of the campaign launched in USSR against complementarity. I remarked that the praxis of the communist parties overall was that of silencing those who tried to link complementarity with dialectical materialism, and I have argued that this was very harmful to the Marxist movement itself. However, I also remarked that the quantum physics case was quite different, in its consequences, from 'Lysenkoism' and similar affairs. The difference lies in the possibility of answering positively to the quoted Jammer's question. It deserves a careful analysis, for instance, of the role played by Marxism in the creation, in the 1950s, of the group and network under the leadership of De Broglie, who was not a Marxist, and Vigier, who is a Marxist.

[23] John Clauser (2001), describing his own trajectory wrote: "In the late sixties I was a graduate student at Columbia University, and was struggling, trying to understand quantum mechanics. While I had difficulty understanding the Copenhagen interpretation, the arguments by its critics seemed far more reasonable to me at that time. [...] The Vietnam War dominated the political thoughts of my generation. Being a young student living in this era of revolutionary thinking, I naturally wanted to

'shake the world'. Since I already believed that hidden variables may indeed exist, I figured that this [Bell's tests] was obviously the crucial experiment for finally revealing their existence."

References

Aspect, A., Grangier, Ph. & Roger, G.: 1982, 'Experimental Realization of Einstein–Podolsky–Rosen–Bohm Gedanken Experiment: A New Violation of Bell's Inequalities', *Physical Review Letters* **49**, 91–94.

Aspect, A., Dalibard, J. & Roger, G.: 1982, 'Experimental Test of Bell's Inequalities Using Time-Varying Analyzers', *Physical Review Letters* **49**, 1804–1807.

Ballentine, L.: 1970, 'The Statistical Interpretation of Quantum Mechanics', *Reviews of Modern Physics* **42**, 358–381.

Bell, J. S.: 1964, 'On the Einstein Podolsky Rosen Paradox', *Physics* **1**, 195–200.

Bell, J. S.: 1982, 'On the Impossible Pilot Wave', *Foundations of Physics* **12**, 989–999.

Bohm, D.: 1952, 'A Suggested Interpretation of the Quantum Theory in Terms of "Hidden" Variables, I and II', *Physical Review* **85**, 166–179, 180–193.

Clauser, J., Horne, M., Shimony, A. & Holt, R.: 1969, 'Proposed Experiment to Test Local Hidden-Variable Theories', *Physical Review Letters* **23**, 880–884.

Clauser, J.: 1992, Early History of Bell's Theorem and Experiment, in T. D. Black et al. (eds.), *Foundations of Quantum Mechanics*, World Scientific, Singapore, pp. 168–174.

Clauser, J.: 2001, Early History of Bell's Theorem, Invited talk at the Eight Rochester Conference on Coherence and Quantum Optics, 13–16 June 2001, University of Rochester.

Cross, A.: 1991, 'The Crisis in Physics: Dialectical Materialism and Quantum Theory', *Social Studies of Science* **21**, 735–759.

Cushing, J.: 1994, *Quantum Mechanics – Historical Contingency and the Copenhagen Hegemony*, The University of Chicago Press, Chicago.

Daneri, A., Loinger, A. & Prosperi, G. M.: 1962, 'Quantum Theory of Measurement and Ergodicity Conditions', *Nuclear Physics* **33**, 297–319.

D'Espagnat, B.: 1965, *Conceptions de la Physique Contemporaine – Les Interprétations de la Mécanique Quantique et de la Mesure*, Hermann, Paris.

D'Espagnat, B.: 1966, 'Two remarks on the Theory of Measurement', *Suppl al Nuovo Cimento* **1**, 828–838.

D'Espagnat, B. (ed).: 1971, *Foundations of Quantum Mechanics*, Proceedings of the Int. School of Physics 'Enrico Fermi', Academic Press.

D'Espagnat, B.: 1976, *Conceptual Foundations of Quantum Mechanics*, 2nd ed., W. A. Benjamin Inc., Reading [1st ed. 1971].

Einstein, A., Podolsky, B., and Rosen, N.: 1935, 'Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?', *Physical Review* **47**, 777–870.

Everett III, H.: 1957, ' "Relative States" Formulation of Quantum Mechanics', *Reviews of Modern Physics* **29**, 454–462.

Freire Jr., O.: 1993, 'L'interprétation de la Mécanique Quantique Selon Paul Langevin', *La Pensée* **292**, 117–134.

Freire Jr., O.: 1997, 'Quantum Controversy and Marxism', *Historia Scientiarum* **7**, 137–152.

Freire Jr., O.: 1999, *David Bohm e a Controvérsia dos Quanta*, Coleção CLE, Vol. 27, Centro de Lógica Epistemologia e Histöria da Ciência, Campinas.

Freire Jr., O.: 2001, 'Science, Philosophy and Politics in the Fifties – On a Max Born's Unpublished Paper Entitled "Dialectical Materialism and Modern Physics" ', *Historia Scientiarum* **10**, 248–254.

Freire Jr., O., Paty, M. & Barros, A. R.: 2002, 'Physique Quantique et Causalité Selon Bohm – Analyse d'un cas d'Accueil Défavorable, in P. Helgh Kragh, P. Marage & G. Vanpaemel (eds.), *History of Modern Physics*, Turnhout, Brepols, 261–273.

Frisch, O. R.: 1971, 'The Conceptual Problem of Quantum Theory from the Experimentalist's Point of View', in T. Bastin (ed.), *Quantum Theory and Beyond – Essays and Discussions Arising From a Colloquium*, Cambridge University Press, London, pp. 13–21.

Graham, L.: 1972, *Science and Philosophy in the Soviet Union*, Knopf, New York.

Greca, I. M. & Moreira, M. A.: 2001, 'Uma Revisao da Literatura Sobre Estudos Relativos ao Ensino de Mecânica Quântica Introdutória', *Investigações em Ensino de Ciências* **6**(1), [Porto Alegre], Electronic edition.

Greca, I. M. & Freire Jr., O.: 2003, 'Does an Emphasis on The Concept of Quantum States Enhance Students' Understanding of Quantum Mechanics?', *Science & Education* **12**(5–6), 541–557 (this issue).

Greca, I. M. & Herscovitz, V. E.: 2002, 'Construyendo Significados en Mecánica Cuántica: Fundamentación y Resultados de Una Propuesta Innovadoa para su Introducción en el nivel universitario', *Enseñanza de las Ciencias*, Forthcoming.

Haroche, S., Raimond, J-M, Brune, M.: 1997, 'Le Chat de Schrödinger se Prête à l'Expèrience', *La Recherche* **301**, 50–55.

Jammer, M.: 1974, *The Philosophy of Quantum Mechanics – The Interpretations of Quantum Mechanics in Historical Perspective*, John Wiley & Sons, New York.

Kragh, H.: 1999, *Quantum Generations – A History of Physics in the Twentieth Century*, Princeton University Press.

Landé, A.: 1953, 'Probability in Classical and Quantum Theory', in *Scientific Papers Presented to Max Born*, Oliver & Boyd, Edinburgh, pp. 59–64.

Landé, A.: 1965, *New Foundations of Quantum Mechanics*, Cambridge University Press, London.

Mullet, S.: 1999, Political Science: The Red Scare as the Hidden Variable in the Bohmian Interpretation of Quantum Theory, Senior Thesis HIS679, University of Texas at Austin.

Olwell, R.: 1999, 'Physical Isolation and Marginalization in Physics – David Bohm's Cold War Exile, *ISIS* **90**, 738–756.

Paty, M.: 1977, 'The Recent Attempts to Verify Quantum Mechanics', in José Leite Lopes & Michel Paty, *Quantum Mechanics, a Half Century Later*, D. Reidel Publishers, Dordrecht, pp. 261–289.

Paty, M.: 1995, 'The Nature of Einstein's Objections to the Copenhagen Interpretation of Quantum Mechanics', *Foundations of Physics* **25**, 183–204.

Pauli, W.: 1999, in K. von Meyenn (ed.), *Scientific Correspondence, V. IV, Part II: 1953–1954*, Springer.

Pessoa Jr., O.: 1992, 'O Estudo da Medição em Mecânica Quântica; um Exame Atualizado', *Cadernos de História e Filosofia da Ciência*, série 3, **2**, 177–217.

Popper, K.: 1957, 'The Propensity Interpretation of the Calculus of Probability, and the Quantum Theory', in S. Körner (ed.), *Observation and Interpretation in the Philosophy of Physics – With Special Reference to Quantum Mechanics*, Dover Publishing, New York, pp. 65–70, 78–89.

Rosenfeld, L.: 1953, 'L'évidence de la Complementarité', in André George (ed.), *Louis de Broglie – Physicien et Penseur*, Editions Albin Michel, Paris, pp. 43–65.

Rosenfeld, L.: 1965, 'The Measuring Process in Quantum Mechanics', *Suppl Progress of Theoretical Physics*, Commemoration issue for the 30th Anniversary of the meson theory by Dr. H. Yukawa, pp. 222–235.

Rowe, M. A. et al.: 2001, 'Experimental Violation of a Bell's Inequality with Efficient Detection', *Nature* **409**, 791–794.

Schrödinger, E.: 1935, 'Die gegenwärtige Situation in der Quantenmechanik', *Naturwissenschaften* **23**, 807–812, 823–828, 844–849. Eng. tr. in Wheeler. & Zurek (1983, pp. 152–167).

von Neumann, J.: 1932, *Mathematische Grundlagen der Quantenmechanik*, Springer, Berlin.

Wheeler, J. A. & Zurek, W. H. (eds.): 1983, *Quantum Theory and Measurement*, Princeton University Press, Princeton.

Wigner, E.: 1963, 'The Problem of Measurement', *American Journal of Physics* **31**, 6–15.