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Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics

DAVID BOHM WAS among the most promising students of his generation when he took his degree in 1943. He got a position at Princeton, did research on plasma theory, and published a well received graduate textbook entitled *Quantum theory*. But in 1951, his life suffered two important changes. He became a victim of McCarthy's anticommunist hysteria and he changed his research focus to a causal interpretation of quantum mechanics. Those two changes were a turning point in his personal and scientific life. In the McCarthy period, Bohm could not survive in American academia. He obtained a position in Brazil, but did not enjoy it there. However, having had his passport confiscated by American officials, it was as a Brazilian citizen that he left Brazil in January 1955 to take a position at the Technion in Haifa. Two years later he went to England, where he finally found a convenient place to pursue his research for the rest of his prolonged exile. His main scientific interests remained in the unorthodox subject he began to work on in 1951, but from 1970 on he shifted his approach to what he called "implicit order."[1]

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The following abbreviations are used: AC, Arquivos do CNPq, Museu de Astronomia, Rio de Janeiro; BP, Bohm Papers, Birkbeck College, University of London; *PR*, *Physical review;* RP, Rosenfeld Papers, Niels Bohr Archive, Copenhagen; USP, University of São Paulo; and WP, Wheeler Papers, American Philosophical Society, Philadelphia.

1. John Wheeler, *Geons, black holes and quantum foam: A life in physics* (New York, 1998), 215-216; David Bohm, *Quantum theory* (New York, 1951); and "A suggested interpretation

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Physicists did not like Bohm's causal interpretation and some were downright hostile. Both the vicissitudes of Bohm's life and the adverse initial reception of his causal interpretation have attracted the attention of historians, philosophers, and physicists, and some of them have blamed the former for the latter. "The political atmosphere in the U.S. at that time did not help rational debate and in consequence there was little discussion and the interpretation was generally ignored for reasons that had more to do with politics than science," according to Bohm's assistant, Basil Hiley. F. David Peat, a science writer and former Bohm collaborator, also found the political explanation for Bohm's unfavorable reception appealing, but limited its force to the Princeton physics community. The historians Russel Olwell and Shawn Mullet blamed Bohm's Brazilian exile for the poor reception of his causal interpretation.[2] The case has attracted some interest, probably because beginning in the early 1970s the interpretation and foundations of quantum mechanics became a field of intensive research. "Bohmian mechanics" attracted some of the researchers in the 1990s; and by the end of the century, Bohm was considered one of the most gifted protagonists in the field of research he had helped to create. A sign of his new prestige can be found in the volume honoring the centenary of the *Physical review,* which includes commentaries on and reprints from the most important papers ever published in the leading journal of American physics. One chapter is dedicated to papers, including Bohm on the causal interpretation, that concern foundations of quantum mechanics. A photo of Bohm opens the chapter.[3]

In order to explain the poor initial reception of Bohm's physical ideas, and his Brazilian exile, it is evidently necessary to look not only to his political problems, but also to the status of research into the foundations of quantum mechanics. By

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of the quantum theory in terms of 'hidden' variables," *PR, 85* (1952), 166-193.

2. Basil Hiley, "David Joseph Bohm," Royal Society London, *Biographical memoirs*, *43* (1997), 106-131, on 113; F. David Peat, *Infinite potential: The life and times of David Bohm* (Reading, 1997), 133; Russell Olwell, "Physical isolation and marginalization in physics—David Bohm's Cold War exile," *Isis, 90* (1999), 738-56, on 750; and Shawn Mullet, "Political science: The red scare as the hidden variable in the Bohmian interpretation of quantum theory" (Senior thesis HIS679, University of Texas at Austin, unpub. paper, 1999). Mullet, after contact with sources about Bohm's stay in Brazil, has changed his views; cf. Shawn Mullet, "Creativity and the mainstream: David Bohm's migration to Brazil and the hidden variables interpretation," unpublished paper, Workshop on "Migrant scientists in the twentieth century" (Milan, 2003).

3. Olival Freire Jr., "The historical roots of 'foundations of physics' as field of research (1950-1970)," *Foundations of physics*, *34* (2004), 1741-1760. See the evolution of the number of citations of Bohm's original paper on causal interpretation, a graphics based on Fabio Freitas and Olival Freire Jr., "Sobre o uso da web of science como fonte para a história da ciência," *Revista da SBHC*, *1* (2003), 129-147. For a discussion of "Bohmian mechanics," James Cushing, Arthur Fine, and Sheldon Goldstein, eds., *Bohmian mechanics and quantum theory: An appraisal* (Dordrecht, 1996). Henry Stroke, ed., *The* Physical review*: The first hundred years—a selection of seminal papers and commentaries* (New York, 1995).

status I mean both the role played by the standard interpretation (complementarity), and the place of foundations of quantum mechanics in research agendas and teaching duties. In putting scientific ideas in their cultural contexts I neither juxtapose them nor find strong causal links between them; as remarked by Peter Galison and Andrew Warwick, "understanding science as a cultural activity…means learning to identify and to interpret the complicated and particular collection of shared actions, values, signs, beliefs and practices by which groups of scientists make sense of their daily lives and work." They also noted that, "this kind of approach has already been widely applied to the history of the experimental sciences, but the literature on the theoretical side is much less developed." No connections between theories and experiments related to the foundations of quantum mechanics had been made in the early 1950s; we need therefore to understand the reception of a theoretical approach in a context lacking experimental links. The suggested approach of science as a cultural activity calls attention to the role of pedagogy in the production of science.[4]

I begin by showing that Bohm found support in Brazil for his research program. The evidence adduced also contributes to filling a gap pointed out by Alexis De Greiff and David Kaiser; the construction of knowledge outside the leading centers of calculation and, consequently, of the globalization of knowledge, remains woefully understudied."[5] Bohm continued to work consistently on the causal interpretation, kept in contact with colleagues abroad, discussed his proposal with visitors from Europe and the United States, and profitted from collaboration with Brazilian physicists in achieving some of the published results on the causal interpretation. Bohm's activities in Brazil did not match the views he expressed in the letters he wrote at the time, which reflected his personality and personnel problems. Bohm would have faced elsewhere many of the obstacles that he faced in Brazil while working on a causal interpretation. The second part of the paper focuses on the dispute between supporters of causal and complementarity interpretations. Bohm's proposal represented the first alternative to complementarity, and for this reason was seen as a major challenge. Supporters of complementarity succeeded in characterizing the dispute as philosophical, that is, not suitable for professional physicists. The fact that Bohm and his collaborators did not get any new results both influenced, and was influenced by, the characterization of foundations as philosophy.

David Bohm and his supporters challenged what Max Jammer called the "almost unchallenged monocracy of the Copenhagen school in the philosophy of

4. Peter Galison and Andrew Warwick, "Introduction: Cultures of theory," *Studies in history and philosophy of modern physics, 29B* (1998), 287-294. For the role of pedagogy, David Kaiser, "Cold War requisitions, scientific manpower, and the production of American physicists after World War II," *HSPS, 33:1* (2002), 131-159; *Drawing theories apart: The dispersion of Feynman diagrams in postwar physics* (Chicago, 2005); David Kaiser, ed., *Pedagogy and the practice of science: Historical and contemporary perspectives* (Cambridge, 2005); and their bibliographical references.

5. Alexis De Greiff and David Kaiser, "Foreword," *HSPS, 33:1* (2002), 1-2.

quantum mechanics."[6] I will discuss the possible meanings and implications of Jammer's characterization. Additionally, I will sketch two comparative essays, one as description of the role of the McCarthyist climate in different contexts, the other a comparision of the reception of Bohm's causal interpretation with that of the interpretation suggested by Hugh Everett in the late 1950s.

1. BRAZILIAN EXILE

Settling in

In 1951 the House Un-American Activities Committee (HUAC) subpoenaed Bohm to talk about his activities and links with the Communist Party during the war at the Radiation Laboratory at Berkeley. He claimed the Fifth Amendment protection against self-incrimination. He was indicted by a jury for contempt of Congress, arrested, freed on bail, and eventually acquitted on May 31, 1951. In this acquittal, Bohm profitted from a decision of the Supreme Court reasserting the Fifth Amendment for people testifying before congressional committees. However, his personal damages were irreversible. At the beginning of the trial, Princeton University placed him on paid leave and did not reappoint him. Princeton's decision still stirs up controversy. John Archibald Wheeler wrote in his autobiography, after mentioning that he had invited Bohm to Princeton: "since the Bohm affair—which understandably polarized the campus—occurred while I was away, I played no part in it. Had I been there, I'm not sure I would have been outspoken in Bohm's defense….The university was gauche in its manner of dealing with Bohm, yet I could sympathize with its goal, to preserve its reputation as a center of unbiased scholarly inquiry, not the home of blind loyalty to one ideology or another."[7]

Bohm began to look for a position abroad. He tried in Manchester,[8] with the support of Albert Einstein, to whom he had become close while owing to their discussions of the interpretation of quantum mechanics. Manchester did not hire him. Brazil then entered the picture. Princeton had graduated a small group of Brazilian physicists and had become a meeting place for them (figure 1). Jayme Tiomno had graduated under John Wheeler and Eugene Wigner in 1950, José Leite Lopes had

6. Max Jammer, *The philosophy of quantum mechanics: The interpretations of quantum mechanics in historical perspective* (New York, 1974), 250.

7. Those events are described in Olwell, Mullet (thesis), Peat, 90-103, and Hiley, 113-114, all cited in (ref. 2). Wheeler (ref. 1), 215. The most accurate description of Bohm's links with the Communist Party of the United States is Alexei Kojevnikov, "David Bohm and collective movements," *HSPS, 33:1* (2002), 161-192. Oppenheimer's behavior in Bohm's case is discussed in David Cassidy, *J. Robert Oppenheimer and the American century* (New York, 2005), 282. For McCarthyism and universities, Ellen Schrecker, *No ivory tower: McCarthyism and the universities* (New York, 1986); and Jessica Wang, *American science in an age of anxiety: Scientists, anticommunism and the cold wa*r (Chapel Hill, 1999).

8. Albert Einstein to Patrick Blackett, 17 Apr 1951, Albert Einstein Archives. I thank Michel Paty for sending me copies of Einstein's and Bohm's letters re Bohm's case.

FIG 1 Standing: C. Lattes, H. Yukawa, W. Schützer. Sitting: H. Carvalho, J. Leite Lopes and J. Tiomno, Princeton, May, 1949. *Source*: Leite Lopes personal archive.

studied under Wolfgang Pauli and Josef Jauch in 1946 and was named a Guggenheim Fellow in 1949, and Walter Schützer had completed a Master degree in 1949. Bohm was one of the readers of Tiomno's doctoral dissertation and served as the chairman of his dissertation committee when Wigner was away.[9] Tiomno invited Bohm to the University of São Paulo. The appointment had the recommendation of Einstein and Oppenheimer and the support of Abrahão de Moraes, then the head of the Physics Department, and Aroldo de Azevedo, an influential geographer.[10] Later, to keep Bohm in his Brazilian position, de Moraes asked Einstein to send letters for eventual promotion addressed to the highest administrative levels, including President Getúlio Vargas.[11]

Bohm went to Brazil an innocent and, as soon as he arrived, he wrote optimistically to Einstein, "The university is rather disorganized, but this will cause no trouble in the study of theoretical physics. There are several good students here, with whom it will be good to work." Later, however, he expressed considerable disatisfaction: "The country here is very poor and not as advanced technically as the U.S., nor is it as clean." "I am afraid that Brazil and I can never agree."[12]

9. Jayme Tiomno, interviewed by the author, 4 Aug 2003.

10. Record number 816/51 [microfilm], Archives of the Faculdade de Filosofia, Ciências e Letras, USP.

11. Abrahão de Moraes did not need to use them. The letter to President Vargas is published in *Estudos avançados*, [São Paulo] *21* (1994).

12. David Bohm to Albert Einstein, Nov 1951, BP (C.10-11), emphasis added. David Bohm to Hanna Loewy, 6 Oct 1953, BP (C.39). As he briefly put it: "Brazil is an extremely

Bohm arrived in Brazil on October 10, 1951. One month later American officials confiscated his passport and told him that he could only retrieve it to return to his native country.[13] Bohm wrote Einstein, "Now what alarms me about this is that I do not know what it means. The best possible interpretation is that they simply do not want me to leave Brazil, and the worst is that they are planning to carry me back because perhaps they are reopening this whole dirty business again. The uncertainty is certainly very disturbing, as it makes planning for the next few years very difficult."[14] Bohm's stay in Brazil, without a passport, changed his mood; he wrote to Melba Phillips: "Ever since I lost the passport, I have been depressed and uneasy, particularly since I was counting very much on [a] trip to Europe as an antidote to all the problems that I have mentioned."[15] Bohm's response to the confiscation of his passport was to seek Brazilian citizenship.[16]

The University of São Paulo (USP), founded in 1934, was a testament to the power of the regional elite, who sought cultural hegemony after the defeat of the state of São Paulo in the 1932 rebellion.[17] When Bohm emigrated, Brazil was experiencing a new democracy that arose from the anti-dictatorial struggle of the Brazilian people and from their participation in World War II on the Allied side.[18] Bohm's double identity as Marxist and Jew was not unfavorable in Brazil; on the contrary, it probably garnered him support. The fragile Brazilian democracy initially legalized the Communist Party, but later banned it. Communists continued to play a role in Brazilian life, for example, the writers Jorge Amado and Graciliano Ramos, the painter Cândido Portinari, the historian Caio Prado Jr., the physicist Mário Schönberg, and the architect Oscar Niemeyer.[19]

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backward and primitive country." David Bohm to Albert Einstein, 3 Feb 1954. Albert Einstein Archives.

13. One should remark that David Fox, a colleague of Bohm's at Berkeley, faced a similar constraint in Israel but refused to deliver his passport, and did not lose his citizenship. Stirling Colgate to George Owen (Deputy Director, Visa Office, U.S. State Dept), 4 Nov 1966, BP (C.8).

14. David Bohm to Albert Einstein, Dec 1951, BP (C.10-11).

15. David Bohm to Melba Phillips, n.d., BP (C.46 - C.48).

16. Brazilian laws did not allow double citizenship. After Bohm became a Brazilian citizen, the U.S. State Department decreed that Bohm had lost his American citizenship. He only recovered it in 1986, after a lawsuit.

17. "It [creation of the USP] meant a political choice of São Paulo, after its defeat in the Constitutionalist Revolution of 1932, betting on science and culture as sources of its political redemption." Shozo Motoyama, "Os principais marcos históricos da ciência e tecnologia no Brasil," *Revista da Sociedade Brasileira de História da Ciência, 1* (1985), 41-49, on 44.

18. Thomas Skidmore, *Politics in Brazil, 1930-1964; An experiment in democracy* (New York, 1967); and Antonio M. de Almeida Jr., "Do declínio do Estado Novo ao suicídio de Getúlio Vargas," Boris Fausto, *História geral da civilização brasileira, tomo III, vol. 3: O Brasil republicano – sociedade e política (1930-1964)* (Rio de Janeiro, 1996), 225-255.

19. Leôncio M. Rodrigues, "O PCB: Os dirigentes e a organização," Fausto (ref. 18), 361-443, on 412.

Brazil had been a *terre d'accueil* for Jews since the beginning of the 20th century. An important literary recognition of this tolerance came from the Austrian Jew Stefan Zweig, who suggested in 1941 that Brazil could be a land of the future to Jews persecuted in Europe. The social and economic conditions associated with the exceptional phase of development in the metropolitan area of São Paulo were favorable to the integration of the Jewish community before and after World War II. The picture was not entirely rosy. Anti-Semitic features existed in the nationalist politics of the dictatorial regime of the "Estado Novo" (1930-1945); and the weakness of anti-Semitic feelings in Brazil did not extend to racial democracy for Brazilians of African descent.[20]

One of the most gifted Brazilian physicists, Mario Schönberg, a member of the Physics Department that hired Bohm, was a Jew who had represented the Communist Party in the state parliament after World War II. Schönberg had not had plain sailing. He was arrested in 1948 for his Communist affiliation. Nonetheless, despite the arrests of political activists and the closing of unions and the Communist Party, Communist intellectuals, like Schönberg, kept their positions at the universities. This could not have happened 20 years later, during the military dictatorship (1964-1985), when Schönberg was arrested and obliged to retire by presidential decree. Forced retirement also ended, sometimes only temporarily, the careers of physicists Tiomno, and Leite Lopes, the sociologist Fernando Henrique Cardoso (who would become President of Brazil), and the economist Celso Furtado.[21]

Bohm's interpretation of quantum mechanics

Bohm depicted quantum systems, such as electrons, as particles with well defined positions, and associated them with a function of the form $\psi = R \exp(iS/\hbar)$. Applying the Schrödinger equation to this function and exploiting analogies between the resulting equations and the Hamilton-Jacobi equation of classical mechanics, he showed that the electrons could have a well defined momentum $\mathbf{p} = \nabla S(\mathbf{x})$. In addition, the electrons suffer the action of a "quantum potential" $U(\mathbf{x}) = -\hbar^2\nabla^2 R/2mR$ in addition to the potentials known from classical physics. In this model, $P = |\psi(\mathbf{x})|^2$ gives the probability density of a statistical ensemble of particle positions. Bohm's electrons have well defined positions as well as momenta; thus, they have continuous and well defined trajectories. These p's and x's are the hidden variables in Bohm's models. He developed them by ascribing well defined posi-

20. Stefan Zweig, *Brazil: A land of the future* (Riverside, 2000) [1st edn. 1941]; Henrique Rattner, *Tradição e mudança (a comunidade judaica em São Paulo)* (São Paulo, 1977); Maria L.T. Carneiro, *O anti-semitismo na era Vargas (1930-1945)* (São Paulo); Thomas Skidmore, *Black into white; race and nationality in Brazilian thought* (Durham, 1993). I am grateful to Marcos Chor and Augusto Videira for some remarks about the literature on this subject.

21. Olwell (ref. 2), 750; Rodrigues (ref. 19); for the last period, see Thomas Skidmore, *The politics of military rule in Brazil: 1964-85* (New York, 1988), esp. chapt. 4.

tions and momenta to measurement devices. From the Hamiltonian of the coupling between such devices and the micro systems, observable results could be predicted. Bohm applied these ideas to detailed calculations of stationary states, transitions between stationary states (including scattering problems), and the Einstein-Podolsky-Rosen *gedankenexperiment*. To achieve results compatible with those from quantum mechanics, Bohm modeled light as electromagnetic waves.

Bohm's results agreed with the usual quantum mechanics for non-relativistic situations. He departed from complementarity, or the "usual interpretation" as he called it, in its essential assumption, "that the most complete possible specification of an individual system is in terms of a wave function that determines only probable results of actual measurement processes." In addition, he expected that some assumptions of his models could be relaxed, permitting predictions different from quantum mechanics in domains in which it was facing difficulties, such as the myriad new "fundamental" particles and the infinities in quantum electrodynamics. According to Bohm, "the usual mathematical formulation seems to lead to insoluble difficulties when it is extrapolated into the domain of distances of the order of 10^{-13} cm or less. It is therefore entirely possible that the interpretation suggested here may be needed for the resolution of these difficulties." Bohm knew that his quantum potential exhibited strange features, such as the instantaneous propagation of interactions in systems with many bodies. However, he hoped to remove the blemish in a future relativistic generalization of his models.[22]

Before Bohm's papers appeared in print, Einstein and Pauli informed him that Louis de Broglie had suggested a similar approach in 1927, which Bohm had not known. Pauli had criticized de Broglie's approach when first proposed; under his and other criticism, de Broglie had given up his idea; and now Bohm had to face the same objections. Pauli had argued that de Broglie's proposal fitted Max Born's probabilistic interpretation of the ψ function only for elastic collisions. In the case of inelastic scattering of particles by a rotator, a problem Enrico Fermi had solved in 1926, de Broglie's idea was incompatible with assigning the rotator stationary states before and after the scattering. Pauli had considered this failure intrinsic to de Broglie's picture of particles with definite trajectories in space-time.[23]

Pauli addressed his criticisms to a draft version of Bohm corrected in consequence. This draft has not survived, but an indication of the corrections has. In response to Pauli's criticisms Bohm wrote: "I hope that this new copy will answer some of the objections to my previous manuscript….To sum up my answer to your

22. Bohm (ref. 1).

23. Einstein's remark is in Michel Paty, "Sur les 'variables caches de la mécanique quantique – Albert Einstein, David Bohm et Louis de Broglie," *La pensée*, *292* (1993), 93-116. Bohm to Pauli, [Jul 1951], Wolfgang Pauli, *Scientific correspondence*. Vol. IV, Part I, ed. Karl von Meyenn (Berlin, 1996), 343-345. Most of Pauli's letters to Bohm did not survive; we infer their contents from Bohm's replies. Bohm to Karl von Meyenn, 2 Dec 1983, ibid, on 345. Broglie's pilot wave and Pauli's criticisms are in *Électrons et photons—rapports et discussions du cinquième conseil de physique* (Paris, 1928), 105-141, and 280-282.

criticisms…I believe that they were based on the excessively abstract assumptions of a plane wave of infinite extent for the electrons' Ψ function. As I point out in section 7 of paper I, if you had chosen an incident wave *packet* instead, then after the collision is over, the electron ends up in one of the outgoing wave packets, so that a stationary state is once more obtained." Pauli did not read the second manuscript as he considered it too long. Bohm: "If I write a paper so 'short' that you will read it, then I cannot answer all of your objections. If I answer all of your objections, then the paper will be too 'long' for you to read. I really think that it is your duty to read these papers carefully."

As a precaution, he summarized his views and the improvements in letters:[24]

> In the second version of the paper, these objections are *all* answered in detail. The second version differs considerably from the first version. In particular, in the second version, I do not need to use "molecular chaos." You refer to this interpretation as de Broglie's. It is true that he suggested it first, but he gave it up because he came to the erroneous conclusion that it does not work. The essential new point that I have added is to show in detail (especially by working out the theory of measurement in paper II) that his interpretation leads to all of the results of the usual interpretation. Section 7 of paper I is also new [transitions between stationary states – the Franck-Hertz experiment], and gives a similar treatment to the more restricted problem of the interaction of two particles, showing that after the interaction is over, the hydrogen atom is left in a definite "quantum state" while the outgoing scattered particle has a corresponding definite value for its energy.

Eventually, Pauli studied Bohm's papers as well as the letters. Pauli conceded that Bohm's model was logically consistent: "I do not see any longer the possibility of any logical contradiction as long as your results agree completely with those of the usual wave mechanics and as long as no means is given to measure the values of your hidden parameters both in the measuring apparatus and in the observed system." Pauli ended with a challenge: "as far as the whole matter stands now, your 'extra wave-mechanical predictions' are still a check, which cannot be cashed." Pauli never ceased to oppose the hidden variable interpretation. For Bohm, however, Pauli's challenge was less pressing than de Broglie's.[25]

Louis de Broglie had had the idea of a "double solution," in which the waves of the Schrödinger equation pilot the particles, which are singularities of the waves. Just before the meeting of the Solvay council in October 24-29, 1927 he gave up this idea because of its mathematical difficulties and presented his report to the meeting with just the "pilot wave." The particles were reduced to objects external to the theory. After the 1927 meeting he adhered to the complementarity interpretation. Bohm was right in remarking that de Broglie had not carried his ideas to a logical conclusion, but de Broglie surely had a share in the idea of

24. Bohm to Pauli, Jul 1951, Summer 1951, Oct 1951, 20 Nov 1951; Pauli (ref. 23), on 343-346, 389-394, and 429-462.
25. Pauli to Bohm, 3 Dec 1951, plus an appendix, Pauli (ref. 23), 436-441.

hidden variables in quantum mechanics. Bohm resisted accepting it. To Pauli he suggested this interesting analogy: "If one man finds a diamond and then throws it away because he falsely concludes that it is a valueless stone, and if this stone is later found by another man who recognize its true value, would you not say that the stone belongs to the second man? I think the same applies to this interpretation of the quantum theory.[26]

Eventually Bohm adopted a diplomatic way, suggested by Pauli, to recognize de Broglie's priority while maintaining the superiority of his own work: "I have changed the introduction of my paper so as to give due credit to de Broglie, and have stated that he gave up the theory too soon (as suggested in your letter)." In addition to changing the introduction, he added "a discussion of interpretations of the quantum theory proposed by de Broglie and Rosen" and rebutted Pauli's criticisms. By the time Bohm's papers appeared in print, de Broglie was returning to his old approach together with his assistant Jean-Pierre Vigier. They would become the most important of Bohm's allies in the hidden-variable campaign.[27]

Reception of the interpretation

Historians have not rated Bohm's work in Brazil very highly. Jessica Wang wrote that McCarthyism forced him to give up research for several years. Later, she slightly modified her views: "Unhappy with the quality of intellectual life at the University of São Paolo [sic] and beset with physical ailments, Bohm searched for a way out." Russell Olwell recognized that "Bohm continued to work on questions of theoretical physics," but added incorrectly "in isolation." Correctly taking into account the level of experimental physics in Brazil, Olwell wrote that "the Brazilian physics community lacked the kind of tools Bohm had used as a graduate student in experimental physics." But he did not consider that, since before leaving the United States, Bohm had been dedicated to the problem of the foundations of quantum mechanics, a field of theoretical physics with no contact with experiments in the 1950s. Experiments in this field came out later.[28] In fact Bohm developed an intense and large scientific activity in Brazil. He discussed his proposal with visitors like Richard Feynman, Isidor Rabi, Léon Rosenfeld, Mario Bunge, Carl Friedrich von Weizsäcker, Herbert Anderson, Donald Kerst, Marcos Moshinsky, Alejandro Medina, and Guido Beck, and Brazilian physicists Schönberg, Jean Meyer, and Leite Lopes. Bohm's work in Brazil gave rise to several publicatons and also collaborations. These involved Vigier, who went to Brazil for three months especially to work with Bohm; the American Ralph Schiller, who had been a student of the

26. For the evolution of de Broglie's ideas, Louis de Broglie, *Nouvelles perspectives en microphysique* (Paris, 1952), 115-143. Bohm to Pauli, Oct 1951, Pauli (ref. 23).
27. Bohm to Pauli, 20 Nov 1951, Pauli (ref. 23). Bohm (ref. 1), 191-193.
28. Jessica Wang, "Science, security and the Cold War: The case of E.U. Condon," *Isis, 83* (1992), 238-289, on 267; Wang (ref. 7), on 278; Olwell (ref. 2), on 750.

cosmologist Peter Bergmann at Syracuse University and stayed in Brazil for two years as Bohm's assistant; and the Brazilians Tiomno and Walther Schützer.

Bohm's main hope for an ally among foreign visitors was Richard Feynman, who spent his sabbatical year in 1951 at the Centro Brasileiro de Pesquisas Fisicas (CBPF) in Rio de Janeiro.[29] Bohm liked Feynman's initial reaction: "At the scientific conference at Belo Horizonte, I gave a talk on the quantum theory, which was well received. Feynman was convinced that it is a logical possibility, and that it may lead to something new." Thus to Hanna Loewy:[30]

> Right now, I am in Rio giving a talk on the quantum theory. About the only person here who really understands is Feynman, and I am gradually winning him over. He already concedes that it is a logical possibility. Also, I am trying to get him out of his depressing trap down long and dreary calculations on a theory [procedures of renormalization in Quantum Field Theory] that is known to be of no use. Instead maybe he can be gotten interested in speculation about new ideas, as he used to do, before Bethe and the rest of the calculations got hold of him.

Bohm's hopes were unfounded, since "in his physics Feynman always stayed close to experiments and showed little interest in theories that could not be tested experimentally." The only reference Feynman made to hidden variables as a result of his Brazilian sabbatical was a mention, as a possible avenue for the development of theoretical physics, in a general paper published in a Brazilian science journal. That could scarcely nourish Bohm's hopes.[31] More promising support came from Guido Beck, one-time assistant to Heisenberg who had fled to Brazil from the Nazis. Beck did not share a belief in the causal interpretation, but defended Bohm against the criticisms of Léon Rosenfeld and insisted Bohm should be encouraged to show what his approach could attain. Beck helped Bohm obtain funding from Brazil's national science foundation, the CNPq.[32] The Argentine Mario Bunge, who had

29. José Leite Lopes, "Richard Feynman in Brazil: Personal recollections," *Quipu, 7* (1990), 383-397; and Jagdish Mehra, *The beat of a different drum* (New York, 1994), 333-342.
30. David Bohm to Hanna Loewy, [w/d], 4 Dec 1951, BP (C.38).
31. Silvan S. Schweber, "Feynman, Richard," J.L Heilbron, ed., *The Oxford guide to the history of physics and astronomy* (New York, 2005), 118-20; Richard Feynman, "The present situation in fundamental theoretical physics," *Anais da Academia Brasileira de Ciências*, *26:1* (1954), 51-60. For the role played by Feynman, Bethe, and the renormalization calculations in physics at that time, see Sam Schweber, *QED and the men who made it: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton, 1994).
32. Guido Beck to Léon Rosenfeld, 1 May 1952, RP. Rosenfeld to Beck, 9 Feb 1953; Bohm to Beck, 16 Sep 1952; 31 Dec 1952; 13 Apr 1953; 5 May 1953; 26 May 1953; Guido Beck Papers, Centro Brasileiro de Pesquisas Físicas, Rio de Janeiro. Rosenfeld was sensitive to Beck's remarks. In the English translation of the original French paper Rosenfeld deleted the comparison which had been criticized by Beck. The original expression is: "on comprend que le pionnier s'avançant dans un territoire inconnu ne trouve pas d'emblée la bonne route; on comprend moins qu'un touriste s'égare encore après que ce territoire a été levé

been Beck's doctoral student spent a year working with Bohm, but nothing came of it. Bunge attacked the difficult problem of the "Bohmization" of relativistic quantum mechanics and the elimination of infinities in quantum electrodynamics. In the middle 1960s, disenchanted with the hidden variable interpretation, he gave it up.[33]

Feynman, Beck, Bunge, Vigier, and Schiller were one thing; Rabi, Rosenfeld, and von Weizsäcker quite another:[34]

> We had an international Congress of Physics....8 physicists from the States (including Wigner, Rabi, Herb, Kerst, and others), 10 from Mexico, Argentina, and Bolivia, aside [a] few from Europe, were brought here by the UNESCO and by the Brazilian National Res. Council….The Americans are clearly very competent in their own fields, but very naïve and reactionary in other fields….I gave a talk on my hidden variables, but ran into much opposition, especially from Rabi. Most of it made no real sense.

Bohm formulated Rabi's view thus: "As yet, your theory is just based on hopes, so why bother us with it until it produces results. The hidden variables are at present analogous to the 'angels' which people introduced in the Middle Ages to explain things."

Here is Rabi's statement of his opinion:

> I do not see how the causal interpretation gives us any line to work on other than the use of the concepts of quantum theory. Every time a concept of quantum theory comes along, you can say yes, it would do the same thing as this in the causal interpretation. But I would like to see a situation where the thing turns around, when you predict something and we say, yes, the quantum theory can do it too.

Bohm's main answer was a comparison with the debates on atomism in the 19th century: "[E]xactly the same criticism that you are making was made against the atomic theory—that nobody had seen the atoms, nobody knew what they were like, and the deduction about them was gotten from the perfect gas law, which was already known." But Bohm faced tougher questions than his analogy suggested. How would the model be made relativistic? Anderson wanted to know how Bohm could recover the quantum feature of indiscernibility of particles, i.e., the exclusion principle; Medina asked if Bohm's approach could "predict the existence of a spin

et cartographié au vingt-millième." Léon Rosenfeld, "L'évidence de la complementarité," André George, ed., *Louis de Broglie—physicien et penseur* (Paris, 1953), 43-65, on 56; idem, "Strife about complementarity," *Science progress, 163* (1953), 393-410, reprinted in Robert Cohen and John Stachel, eds., *Selected papers of Léon Rosenfeld* (Dordrecht, 1979).

33. Mario Bunge to the author, 1 Nov 1996, and 12 Feb 1997.

34. David Bohm to Miriam Yevick, [received 20 Aug 1952]; Bohm to Melba Phillips, n.d., BP. I merged the two letters in my narrative. I am grateful to Shawn Mullet for the courtesy of a CD with copies of Miriam Yevick's letters. They are now also available at BP.

of a particle as in field theory;" Leite Lopes and Kerst called for experiments that could decide between the interpretations; and Moshinsky, asked whether there is a "reaction of the motion of the particle on the wave field." Bohm's answer to Anderson is interesting. He said that the causal interpretation only needed to reproduce the experimental predictions of quantum theory, not each one of its concepts. "All I wish to do is to obtain the same experimental results from this theory as are obtained from the usual theories, that is, it is not necessary for me to reproduce every statement of the usual interpretation….You may take the exclusion principle as a principle to explain these experiments [levels of energy]. But another principle would also explain them."[35]

Rosenfeld was a doctrinaire supporter of complementarity. He wrote, "I certainly shall not enter into any controversy with you or anybody else on the subject of complementarity, for the simple reason that there is not the slightest controversial point about it." Rosenfeld had gone to Brazil to discuss the epistemological problems of quantum mechanics. He offered a course on classical statistical mechanics in Rio de Janeiro, published papers in Portuguese on the epistemological lessons of quantum mechanics, and gave a talk in São Paulo on the non-controversial issue of complementarity. Bohm reported his exchange with Rosenfeld to Aage Bohr: "Prof. Rosenfeld visited Brazil recently, and we had a rather hot and extended discussion in São Paulo, following a seminar that he gave on the foundations of the quantum theory. However, I think that we both learned something from the seminar. Rosenfeld admitted to me afterwards that he could at least see that my point of view was a possible one, although he personally did not like it."[36]

Von Weizsäcker recognized in 1971 that debates with Bohm on hidden variables had motivated him to work on what he called "complementarity logic," a many-valued logic. During his visit to Brazil, von Weizsäcker had allied himself with a group of physicists with whom Bohm was in dispute about funding. He saw von Weizsäcker's activities as a plot. The business got nasty. Bohm advised the physicist Philip Morrison and the mathematician Miriam Yevick that "Nazis [are] taking over Brazilian physics….[T]ry to see what you can do about lining up publicity against Weissacre [sic], *but don't do a thing till I say 'go'*." To Guido Beck, he identified the group involved: "I am writing you to let you know that Marcello and Stammreich, *apparently* acting on behalf of the Weissacker—Leal [sic] group are doing their best to annoy me."

Marcello is the Brazilian physicist Marcello Damy de Souza Santos. He worked with cosmic rays and built USP's betatron, the first accelerator to be used in Latin America, in 1950. The German spectroscopist Hans Stammreich, who had migrated

35. Ibid. *New research techniques in physics*, *Proceedings*, Rio de Janeiro and São Paulo, July 15-29, 1952, Rio de Janeiro, 1954, pp. 187-198.

36. Léon Rosenfeld to David Bohm, 30 May 1952, RP; Bohm to Aage Bohr, 13 Oct 1953, Aage Bohr Papers, Niels Bohr Archive, Copenhagen; Léon Rosenfeld, "A filosofia da física atômica," *Ciência e cultura*, *6:2* (1954), 67-72; ibid., "Classical statistical mechanics" (Rio de Janeiro, 1953), published in 2005 by CBPF.

to Brazil in the 1940s, was a professor of physics at USP. "Leal" refers to the brothers Jorge and Paulo Leal Ferreira, Brazilian physicists who eventually founded the Instituto de Física Teórica in São Paulo. According to Bohm, Damy's arguments involved ideological considerations, since he had "been telling everyone here that (a) I am Communist, (b) My theory is Marxist….[B]oth statements are, of course, nonsensical." As the dispute concerned the hiring of Bohm's assistants, he wrote that "Stammreich [had] accused [him] of filling the place with North Americans….I was warmly defended by several Brazilians, however, and my proposal was passed by a large majority."[37]

Bohm considered the papers he wrote with Tiomno and Schiller and with Vigier to be main achievements of the causal program in the early 1950s.[38] With Vigier, Bohm met Pauli's objection that Bohm had included an arbitrary element in the causal interpretation, by using a ψ function that satisfied Schrödinger's equation.[39] Bohm had tried to solve the question by himself, without success.[40] De Broglie and Vigier were cognizant of the problem in 1952.[41] In 1954, Bohm and Vigier were able

37. "In 1953, while still a member of the Max-Planck-Institute in Göttingen, von Weizsäcker visited, in an administrative capacity, Brazil where he met with David Bohm in São Paulo and discussed with him the problem of hidden variables. After his return to Göttingen von Weizsäcke, anxious to work out some ideas raised in his discussion with Bohm, decided to conduct a seminar, together with Georg Süssmann, with the objective of studying alternative formulations of quantum mechanics. It was in the course of this seminar, which was also attended by Heisenberg, that von Weizsäcker worked out his "complementarity logic." Jammer (ref. 6), 376. Bohm to Melba Phillips (w/d), Bohm to Miriam Yevick (w/d), BP. Bohm to Guido Beck (w/d), Guido Beck Papers.

38. David Bohm, *Causality and chance in modern physics* (London, 1984), 114, 118, notes 11, 12; and David Bohm and Basil Hiley, *The undivided universe* (London, 1993), 205. Bohm's papers written in collaboration with other physicists while he was in Brazil were David Bohm and Jean-Pierre Vigier, "Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations," *PR, 96* (1954), 208-216; David Bohm, Ralph Schiller, and Jayme Tiomno, "A causal interpretation of the Pauli equation (A)." *Nuovo cimento, suppl*. Vol. 1 (1955), 48-66; David Bohm and Ralph Schiller, "A causal interpretation of the Pauli equation (B)." *Nuovo cimento, suppl*. Vol. 1 (1955), 67-91; David Bohm and Walter Schützer, "The general statistical problem in physics and the theory of probability," *Nuovo cimento, suppl*. Vol. 2 (1955), 1004-1047. Besides, Bohm published five articles and letters alone. For an analysis of the ensemble of these papers, See Olival Freire Jr., *David Bohm e a controvérsia dos quanta* (Campinas, 1999).

39. Wolfgang Pauli, "Remarques sur le problème des paramètres cachés dans la mécanique quantique et sur la théorie de l'onde pilote," André George, ed., *Louis de Broglie – physicien et penseur* (Paris, 1953), 33-42, 38.

40. David Bohm, "Proof that probability density approaches $|\psi|^2$ in causal interpretation of the quantum theory," *PR, 89* (1953), 458-466. A simplified and shortened version of this paper was presented at the above mentioned international scientific meeting held in Brazil, (ref. 35), 187-198.

41. "C'était aussi un des problèmes décisifs que Bohm n'avait pas traité dans ses papiers de 1952." Jean Pierre Vigier, interviewed by the author, 27 Jan1992.

to prove that under certain general conditions any function could become a solution of the Schrödinger equation. To get that result, they used an analogy between Bohm's approach and the hydrodynamic model suggested by Erwin Madelung in 1926, which embedded microscopic quantum particles in a subquantum medium with random fluctuations.[42] Thus, the "molecular chaos" that Bohm had abandoned after his discussions with Pauli came back in his work with Vigier. With Tiomno and Schiller, Bohm included spin in his model, although via analogy with Pauli's equation and not relativity.[43] Bohm and Vigier modeled elementary particles as extended bodies in space-time, associating their degrees of freedom with quantum numbers in an effort to classify for the myriad newly discovered particles. Their paper, published some years later,[44] began a lasting collaboration among Bohm, Vigier, de Broglie, and their associates.[45] With Walter Schützer, Bohm worked on a study of the role of probability in physical theories.[46] He discussed the same issue with Jean Meyer. In *Causality and chance in modern physics,* written during Bohm's stay in Brazil but only published in 1957, he conceived of causal and probabilistic descriptions as possibilities with the same philosophical rank. This revaluation moved him far from his initial fixation on causal description, an intellectual shift that would appear more clearly in his ideas of the 1970s.

The collaboration between Bohm and Vigier was abetted by an irony typical of the Cold War. Had Bohm remained in the U.S., Vigier might not have been able to visit him. Vigier, too was a communist. Before becoming one of the most active spokesmen for the causal program, had made a name in the Communist Party in France. As Jessica Wang has pointed out in writing about the "age of anxiety" in American history, "in addition to refusing passports to American scientists, the State Department also restricted the entry of foreign scientists with left-wing political ties into the United States….Scientists from France, where the Left was particularly strong, he had an especially hard time. As much as 70 to 80 percent of visa requests from French scientists were unduly delayed or refused."[47] A main fruit of the collaboration of the two communists was to return the stanchly conservative de Broglie to his search for a deterministic quantum mechanics. As a Nobel Prize winner and one of the founding fathers of wave

42. Bohm and Vigier (ref. 38).

43. Bohm, Schiller, and Tiomno; Bohm and Schiller (ref. 38).

44. David Bohm and Jean-Pierre Vigier, "Relativistic hydrodynamics of rotating fluid masses," *PR, 109* (1958), 1882-91. "Alors [en Brésil] Bohm et moi on a fait deux papiers, un qui a été fait de suite, qui est sorti en 1954, sur la statistique, et un deuxième qui est sorti plus tard." Vigier (ref. 41).

45. The main achievements of this approach were presented in Louis de Broglie et al, "Rotator model of elementary particles considered as relativistic extended structures in Minkowski space," *PR, 129* (1963), 438-450.

46. Bohm and Schützer (ref. 38).

47. Wang (ref. 7), 278.

mechanics, de Broglie brought clout and recognition as well as ideas.[48] Vigier did not neglect the left, and enlarged the French group with several young Marxists. The Institut Henri Poincaré, under the leadership of de Broglie and Vigier became the main institutional base for supporters of the causal interpretation.[49]

Bohm made much of the French work, no doubt in part because of Vigier's Marxist engagement: "I have heard from someone that in a debate on causality given in Paris, when our friend Vigier got up to defend causality, he was strongly cheered by the audience (which contained a great many students). I would guess that many of the younger people in Europe recognize that the question of causality has important implications in politics, economy, sociology, etc."[50] The connection appeared so obvious to Bohm that he complained when fellow travelers like Philip Morrison did not support him. And he wondered why the causal interpretation had appeared in the West and not in the USSR and why Soviet physicists did not join him.[51]

Reception of the interpreter

Bohm arrived in Brazil at a propitious time for Brazilian physics. Cesare Lattes had participated in the discovery, in 1947, of cosmic-ray pions, and, in 1948 the detection of artificially produced pions. These achievements resonated in Brazil. An alliance among scientists, the military, businessmen, and politicians developed that aimed to strengthen physics in Brazil. This alliance led to the creation of the Centro Brasileiro de Pesquisas Físicas [CBPF] and, in the same year that Bohm arrived in Brazil, to the creation of the first federal agency exclusively dedicated to funding scientific research, the CNPq.[52] From that CNPq, Bohm received several grants to

48. For the evolution of de Broglie's thoughts on these issues, see Louis de Broglie, "La physique quantique restera-t-elle indéterministe?" *Bulletin de la Société française de philosophie, XLVI* (1953), 135-173.

49. Cross saw Bohm's work just as a reflection of the ideological Marxist climate of the time; thus he missed the fact that the quantum controversy continued even when that climate faded. Andrew Cross, "The crisis in physics: Dialectical materialism and quantum theory," *Social studies of science, 21* (1991), 735-759. A lacuna in the history of physics in the 20th century is the analysis of the activities of the de Broglie—Vigier group.

50. David Bohm to Miriam Yevick, 5 Nov 1954, BP.

51. "This type of inconsistency in Phil [Morrison] disturbs me. He should be helping, instead of raising irrelevant obstacles;" David Bohm to Melba Phillips, n.d. BP. "Then the orientation is determined strongly by the older men, such as Fock and Landau….It is disappointing that a society that is oriented in a new direction is still unable to have any great influence on the way in which people think and work;" Bohm, 18 Mar 1955, BP. "I ask myself the question 'Why in 25 years didn't someone in USSR find a materialistic interpretation of quantum theory?'….But bad as conditions are in U.S., etc, the only people who have thus far had the idea are myself in U.S., and Vigier in France." David Bohm to Miriam Yevick, 7 Jan 1952, BP.

52. Ana M.R. Andrade, *Físicos, mésons e política: a dinâmica da ciência na sociedade*

develop the causal interpretation—around Cr $155,000 for the years 1952/3. Those funds permitted Bunge to stay in São Paulo for one year and Schiller to have his wages supplemented for two years.[53] Besides, Bohm received Cr$18,000 for the travel expenses of Schiller and his wife from the U.S.;[54] Cr$37,200 for of Vigier's stay of three months;[55] Cr$100,000 for research on cosmic rays by Kurt Sitte as well as an air ticket for him and his family plus Cr$180,000 to augment Sitte's stipend from USP.[56] Bohm also won grants for his students Abrahão Zimmerman, Ruth Pereira da Silva, Paulo Roberto de Paula e Silva, and Klaus Tausk.[57] CNPq also supported the visits of Rosenfeld, Rabi, and von Weizsäcker.[58] Most of the money Bohm received went to research on cosmic rays, a field under Bohm's responsibility at USP. Nevertheless, the board of the CNPq explicitly supported the development of the causal interpretation. An indication of the interest of CNPq in the research appears from the report of Joquim Costa Ribeiro, the Scientific Director of the agency on Bohm's application for funds for Vigier:[59]

> I call the attention of the Board to the interest of this subject. Prof. Bohm is today on the agenda of theoretical physics at an international level owing to his theory, which is a little revolutionary because it intends to restore to quantum mechanics the principle of determinism, which seems, in a certain way, to have been shaken by Heisenberg's principle. Prof. Bohm seems to have found one solution to this difficulty of modern physics, trying to reconcile quantum mechanics with the

(São Paulo, 1999); Andrade, "The discovery of the π-meson, " in Helge Kragh et al, eds., *History of modern physics* (Turnhout, 2002), 313-21. Personal reminiscences from this period are in José Leite Lopes, "Cinquenta e cinco anos de física no Brasil: evocações," unpubl. paper (Rio de Janeiro, 1998), available at http://www4.prossiga.br/Lopes/. Impressions on Brazilian physics, by a contemporary visitor, are in Gordon L. Brownell, "Physics in South America," *Physics today, 5* (Jul 1952), 5-12, on 11-12. It is true that, at the beginning of the 1950s, the main activities in Brazilian theoretical physics had shifted from São Paulo to Rio de Janeiro. Nevertheless, there were close relations between the two centers, and Bohm commuted between São Paulo and Rio de Janeiro.

53. AC [Processo 578/51]. I am grateful to Ana M.R. Andrade, and her assistants, Tatiane dos Santos and Vanessa Albuquerque, for their help in unearthing those documents.

54. AC [Processo 572/52].

55. AC [Processo 242/53]. It was a partial funding, insofar as Vigier also had support from France.

56. AC [Processo 243/53]. Sitte came from Syracuse University to Brazil, after receiving an invitation from Bohm, to work on cosmic rays. Hiring Sitte was the subject of the dispute between Bohm and other members of the Physics Department, related in this paper while commenting on Bohm's discussion with von Weizsäcker. Ana M.R. Andrade, "Os raios cósmicos entre a ciência e as relações internacionais," Marcos C. Maio, ed., *Ciência, política e relações internacionais: Ensaios sobre Paulo Carneiro* (Rio de Janeiro, 2004), 215-242.

57. AC [Processos 567/51 and 578/51].

58. AC [Processos 1704/53, 504/53, 249/52, resp.].

59. AC, Records of the Conselho Diretor, 139th meeting, 25 Feb 1953.

> rigid determinism of classical physics. I am not speaking in detailed technical terms, but summarizing the issue. Bohm's theory has given rise to a great debate in Europe and United States, and Prof. Vigier has expressed his willingness to come to Brazil, mainly to meet the team of theoretical physicists and discuss the problem here. This seems to me to be a very prestigious thing for Brazil and our scientific community.

Since Bohm had not published his causal interpretation it did not figure in his candidacy for his position at USP. Once when he came to Brazil, however, support from the CNPq came primarily for his work on it.

Bohm's uneasiness in Brazil

Bohm's correspondence with Einstein, Pauli, and Phillips, debates in scientific journals with Takabayasi, Keller, Epstein, Halpern, and Freistadt, papers by Rosenfeld, Pauli, Born, and Heisenberg, laudatory essays by Schatzman and Freistadt in cultural magazines, and the news Bohm had from Bohr and von Neumann, show that the causal interpretation did not pass unnoticed.[60] Most notices were unfavorable reception. Bohm did not understand this skepticism. Bohm's hopes were not modest, "if I can succeed in my general plan, physics can be put back on a basis much nearer to common sense than it has been for a long time." His mood oscillated depending on the reception of his ideas and on the work he had done on them. Thus he could write, "I gave two talks on the subject here, and aroused considerable enthusiasm among people like Tiomno, Schützer, and Leal-Ferreira, who are assistants….Tiomno has been trying to extend the results to the Dirac equation, and has shown some analogy with Einstein's field equations." And again, "I am becoming discouraged also because I lack contact with other people, and feel

60. For an analysis of Einstein's reaction, see Michel Paty, "The nature of Einstein's objections to the Copenhagen interpretation of quantum mechanics," *Foundations of physics*, *25* (1995), 183-204; Paty (ref. 23). Takehiko Takabayasi, "On the formulation of quantum mechanics associated with classical pictures," *Progress of theoretical physics, 8* (1952), 143-182; Takabayasi, "Remarks on the formulation of quantum mechanics with classical pictures and on relations between linear scalar-fields and hydrodynamical fields," *Progress of theoretical physics, 9* (1953), 187-222; Joseph Keller, "Bohm's interpretation of the quantum theory in terms of 'hidden' variables," *PR, 89* (1953), 1040-41; Hans Freistadt, "The crisis in physics," *Science and society, 17* (1953, 211-237; Freistadt, "The causal formulation of quantum mechanics of particles: The theory of de Broglie, Bohm and Takabayasi," *Nuovo cimento, suppl. V* (1957), 1-70; Saul Epstein, "The causal interpretation of quantum mechanics," *PR, 89* (1953), 319; Otto Halpern, "A proposed re-interpretation of quantum mechanics," *PR, 87* (1952), 389; Evry Schatzman, "Physique quantique et realité," *La pensée, 42-43* (1952), 107-22. For the view that the causal interpretation passed unnoticed, see James Cushing, *Quantum mechanics: Historical contingency and the Copenhagen hegemony* (Chicago, 1994); and Hiley (ref. 2), 113.

that there is a general lack of interest in new ideas among physicists throughout the world."[61]

Only by taking into account Bohm's attitude towards those who did not share his opinion about the causal interpretation can one understand his relations with Brazilian physicists. Schönberg, Leite Lopes, and even Tiomno did not support Bohm's research program understood as the recovering of determinism. Tiomno collaborated with Bohm to see what physics could be developed by using Bohm's model. Leite Lopes, a former student of Pauli's, was skeptical about the causal interpretation. Schönberg worked on the mathematical foundations of quantum theory and on the hydrodynamic model of quantum mechanics, a model close to that of Bohm and Vigier, but he opposed seeking a causal description in atomic phenomena.[62]

> Schönberg is 100 percent against the causal interpretation, especially against the idea of trying to form a conceptual image of what is happening. He believes that the true dialectical method is to seek a new form of mathematics, the more 'subtle' the better, and try to solve the crisis in physics in this way. As for explaining chance in terms of causality, he believes this to be "reactionary" and "undialectical." He believes instead that the dialectical approach is to assume "pure chance" which may propagate from level to level, but which is never explained in any way, except in terms of itself.

The attitude of theoretical physicists in Brazil towards Bohm's approach resembled that of physicists elsewhere. They all held the Copenhagen interpretation to be the only viable approach to quantum mechanics.

2. THE RECEPTION OF THE CAUSAL INTERPRETATION

Reaction of the old guard

Mara Beller described "the Copenhagen dogma" as "the rhetoric of finality and inevitability." As she wrote, "the founders and followers of the Copenhagen interpretation advocated their philosophy of physics not as a possible interpretation but as the only feasible one."[63]

61. David Bohm to Melba Phillips, 28 June 1952; ibid., [w.d.], BP (C.46 – C.48). David Bohm to Hanna Loewy, 6 Oct 1953, BP (C.39).
62. For Tiomno's stance, Freire Jr. (ref. 38), 95. David Bohm to Miriam Yevick, 24 Oct 1953, BP. For Schönberg's work on quantum mechanics and geometry, see Mario Schönberg, "Quantum theory and geometry," *Max Planck Festschrift* (Berlin, 1958), 321.
63. Mara Beller, *Quantum dialogue: The making of a revolution* (Chicago, 1999), on 191-210; Beller described Bohm, based on a late work by Bohm and Peat, as pleading for "tolerance, for creative plurality, for peaceful theoretical coexistence…friendly and open-minded, and joyful scientific cooperation and communication," Beller, 210. However, this does not match with Bohm's uneasiness towards everybody who did not agree with the causal interpretation. Biographical studies on Bohm did not yet come to grips with his personal evolution.

Pauli and Rosenfeld were the first to react; Pauli concentrated on the physical and epistemological aspects, Rosenfeld on the philosophical and ideological ones. As Rosenfeld explained his strategy to Pauli, "My own contribution to the anniversary volume [for de Broglie] has a different character. I deliberately put the discussion on the philosophical ground, because it seems to me that the root of evil is there rather than in physics." After Bohm's papers appeared in print, Pauli advanced new criticisms, which surprised Bohm: "I am surprised that Pauli has had the nerve to publicly come out in favor of such nonsense….I certainly hope that he publishes his stuff, as it is so full of inconsistencies and errors that I can attack him from several different directions at once." Pauli had criticized the causal interpretation for not preserving the symmetry between position and momentum representations, expressed in the standard formalism by the theory of unitary transformations. And, as we know, he had objected that Bohm had borrowed the meaning of Ψ from the quantum theory. In a letter to Markus Fierz, Pauli raised the stakes. He observed that Catholics and Communists depended on determinism to buttress their eschatological faiths, the former in the heaven to come, the latter in the terrestrial paradise. Pauli also warned his old friend. Giuseppe Occhialini, who had worked at USP during the 1930s and continued scientific collaboration there after the war, against "Bohm in São Paulo and his 'causal' quantum theory."[64]

For Rosenfeld, complementarity was both a direct result of experience and an essential part of quantum theory.[65] Since complementarity implied the abandonment of determinism, Rosenfeld saw the causal interpretation as a metaphysical mark, "Determinism has not escaped this fate [becoming an obstacle to progress]; the physicist who still clings to it, who shuts his eyes to the evidence of complementarity, exchanges (whether he likes it or not) the rational attitude of the scientist for that of the metaphysician." Every good Marxist should understand that. "The latter, as Engels aptly describes him, considers things 'in isolation, the one after the other and the one without the other,' as if they were 'fixed, rigid, given once for all.'"[66] Most of Rosenfield's work as Bohr's assistant was related to epistemological

64. Léon Rosenfeld to Wolfgang Pauli, 20 Mar 1952; Pauli (ref. 23), on 587-588. Bohm to Beck [w/d], Guido Beck Papers. Beck had reported to Bohm's the content of Pauli's seminar in Paris, in 1952. The criticisms were published in Pauli's contribution to the Louis de Broglie Festschrift, see Pauli (ref. 39). Pauli to Markus Fierz, 6 Jan 1952, Pauli (ref. 23), 499-502; Pauli to Giuseppe Occhialini, [1951-1952]. Archivio Occhialini 5.1.14, Università degli studi, Milan. I thank Leonardo Gariboldi for calling my attention to this document.

65. In the French version of the paper, Rosenfeld emphasized the idea of complementarity resulting from experience, but in the English version, reacting to criticisms from Born, he attenuated his stand, changing "La relation de complémentarité comme donné de l'expérience" to "Complementarity and experience." Rosenfeld (ref. 32); Olival Freire Jr., "Science, philosophy and politics in the fifties: On the Max Born's unpublished paper entitled 'dialectical materialism and modern physics,'" *Historia scientiarum, 10* (2001), 248-254.

66. Rosenfeld (ref. 32).

matters and Rosenfeld's brand of Marxism the Western Marxism rather than the Soviet variety, to use the terms introduced by Perry Anderson.[67] Rosenfeld believed that complementarity was a dialectical achievement that had to be defended not only against Bohm's criticisms but also against Soviet critics who blamed it for introducing idealism in physics.[68] Rosenfeld was orthodox in quantum mechanics and heterodox in Marxism.

Rosenfeld mobilized colleagues wherever he could to take up the fight. He pushed Frédéric Joliot-Curie—a Nobel prize winner and member of the French Communist Party—to oppose French Marxist critics of complementarity;[69] advised Pauline Yates—Secretary of the "Society for cultural relations between the peoples of the British Commonwealth and the USSR"—to withdraw from *Nature* her translation of a paper by Yakov Ilich Frenkel critical of complementarity;[70] asked *Nature* not to publish a paper by Bohm entitled "A causal and continuous interpretation of the quantum theory;"[71] and advised publishers not to translate into English one of de Broglie's books dedicated to the causal interpretation.[72] Rosenfeld's correspondence shows that his campaign had wide support.[73] Denis Gabor wrote, "I was much amused by the onslaught on David Bohm, with whom I had a long discussion on this subject in New York, in Sept. 51. Half a dozen of the most eminent scientists have got their knife into him. Great honour for somebody

67. Perry Anderson, *Considerations on Western Marxism* (London, 1979).

68. "But in any case the relation of complementarity is the first example of a precise dialectical scheme, whose formal structure has been accurately analysed by the logicians." Rosenfeld (ref. 32).

69. "Je crois mon devoir de vous signaler une situation que je considère comme très sérieuse et qui vous touche de près. Il s'agit de vos 'poulains' Vigier, Schatzman, Vassails e tutti quanti, tous jeunes gens intelligents et pleins du désir de bien faire. Malheureusement, pour le moment, ils sont bien malades. Ils se sont mis en tête qu'il fallait mordicus abattre la complémentarité et sauver le déterminisme." He did not succeed; Joliot diplomatically kept his distance from the battle. "Autant je suis d'accord avec leurs préoccpations concernant les grands principes de la physique moderne, autant je suis d'accord avec vous sur la nécessité d'en comprendre le sens exact et profond avant de se lancer dans des discussions avec des citations qui ne sont que des planages trahissant parfois leurs auteurs." Léon Rosenfeld to Frédéric Joliot-Curie, 6 Apr 1952; Joliot to Rosenfeld, 21 Apr 1952. RP. See also Michel Pinault, *Frédéric Joliot-Curie* (Paris, 2000), 508.

70. Pauline Yates to Léon Rosenfeld, 7 Feb 1952, 19 Feb 1952, RP.

71. Rosenfeld succeeded, "the editors stopped work on this article." The paper had been submitted to *Nature* by Harrie S.W. Massey. *Nature*'s editors to Léon Rosenfeld, 11 Mar 1952, RP. "Also I sent a brief article to Massey with the suggestion that he publish it in *Nature*." David Bohm to Miriam Yevick, n.d., BP. Bohm did not keep a copy of the unpublished paper, but there is a copy of it in Louis de Broglie Papers, Archives de l'Académie des sciences, Paris.

72. Léon Rosenfeld, "Report on L. de Broglie, La théorie de la mesure en mécanique ondulatoire." *n.d.* RP. This book had been published in 1957, Paris: Gauthier-Villars.

73. Rosenfeld (ref. 32). The Japanese translation was published in *Kagaku, 25* (1955).

so young."[74] Less equivocal positive letters came from Abraham Pais,[75] Guido Beck, Robert Cohen,[76] Eric Burhop, Vladmir Fock,[77] Jean-Louis Destouches,[78] Robert Haveman,[79] and Adolf Grünbaum.[80] Burhop and Beck took issue with Rosenfeld's rhetoric, however,[81] and Lancelot L. Whyte challenged him publicly over his review of Bohm's "Causality and chance in modern physics."[82]

74. Denis Gabor to Léon Rosenfeld, 7 Jan 1953. RP.
75. "I find your piece about complementarity interesting and good….I could not get very excited about Bohm. Of course it doesn't do any good, but (with the exception of Parisian reactions) it also doesn't do any harm. I find that Bohm wastes his energy and that it will harm him personally a lot because he is moving into the wrong direction—but he needs to realize this himself, he is a difficult person." Abraham Pais to Léon Rosenfeld, 15 May [1952], RP. I thank Katrien Straeten for the translation from the Dutch.
76. "I turn to you because my own reaction to the Bohm thing and to the pilot wave revival has been quite negative, while yet I share Professor Einstein and others' uneasiness at the orthodox situation." Robert Cohen to Léon Rosenfeld, 31 Jul 1953, RP.
77. "Je voudrais aussi discuter avec vous les questions d'interprétation de la mécanique quantique et surtout les causes et les effects de la 'maladie Bohm-Vigier', assez repandue, hélas." Vladmir Fock to Léon Rosenfeld, 7 Apr 1956. RP. For Fock's criticism of Bohm's views, see Vladmir Fock, "On the interpretation of quantum theory," *Czechoslovakian journal of physics, 7* (1957), 643-656.
78. Jean-Louis Destouches to Léon Rosenfeld, 19 Dec 1951, RP. This letter is a fair description of de Broglie's hesitations before his conversion to causal interpretation. It also describes the French philosophical context in which the causal interpretation was well received: "L'indéterminisme quantique et les conceptions de Bohr et Heisenberg n'ont jamais été admises en France sauf par M. Louis de Broglie et ses élèves….Les jeunes gens ont accueilli avec enthousiasme le travail de Bohm qui correspond à toutes les tendances philosophiques qui les animent: réalisme thomiste, déterminisme marxiste, rationalisme cartésien. Je suis donc maintenant à peu près le seul ici à soutenir encore l'interprétation quantique de Bohr."
79. "I read with great interest your paper and I am glad seeing that our ideas are, in their essential aspects, in agreement." Léon Rosenfeld to Robert Haveman, 7 Oct 1957; Haveman to Rosenfeld, 13 Sep 1957. RP.
80. Adolf Grünbaum to Léon Rosenfeld, 1 Feb 1956, 20 Apr 1957, 3 Oct 1957; Rosenfeld to Grünbaum, 14 Feb 1956, 21 May 1957, 11 Dec 1957, RP.
81. "Incidentally the only other comment I would offer on your article was I thought perhaps you were a little cruel to Bohm. Do you think you could spare the time to write to him? He is a young Marxist…being victimized for his political views in the U.S." Burhop was organizing a meeting among Rosenfeld, John Bernal, Maurice Levy, Maurice Cornforth, and Cecil Powell, to discuss Rosenfeld's article. Eric Burhop to Léon Rosenfeld, 5 May 1952, RP. I discussed in Part I Beck's stands.
82. Lancelot Whyte to Léon Rosenfeld, 8 Apr 1958; 14 Mar 1958; 22 Mar 1958; 27 June 1958; Rosenfeld to Whyte, 17 Mar 1958, RP. Rosenfeld to Whyte, 28 May 1958, is in L.L. Whyte Papers, Boston University, Department of Special Collections. Léon Rosenfeld, "Physics and metaphysics," *Nature*, *181* (1958), 658; Lancelot Whyte, "The scope of quantum mechanics," *British journal for the philosophy of science*, *9* (1958), 133-134.

Rosenfeld, Pauli, Heisenberg, and Born built a common front against the causal interpretation, but disagreed, usually in private, over tactics. Rosenfeld criticized Heisenberg probably of leaning towards idealism. Pauli and Born privately criticized Rosenfeld's mixture of Marxism with complementarity; as part of their debate, Max Born sent Rosenfeld a ten-page typed text arguing that dialectical materialism could not be corroborated by reference to just one achievement of contemporary science. Born abandoned the idea of publishing the text in the atmosphere of détente between West and East in the late 1950s. Acting as editor of a volume in honor of Bohr, Pauli prevented Rosenfeld, whom he labeled "√BohrxTrotzky," from adorning his paper with banalities on Materialism.[83]

The label of "philosophical controversy"

Pauli's substantive attack on Bohm's approach rested on several assertions: Since it does not have "any effects on observable phenomena, neither directly nor indirectly…the artificial asymmetry introduced in the treatment of the two variables of a canonically conjugated pair characterizes this form of theory as *artificial metaphysics*." [If the] "new parameters could give rise to empirically visible effects…. they will be in disagreement with the general character of our experiences, [and] in this case this type of theory loses its physical sense."[84] Rosenfeld minus rhetoric came to this: "I intentionally confine the debate to the field of epistemology, for the crucial issue is one of logic, not of physics….Bohm's argument is very cleverly contrived. One would look in vain for any weakness in its formal construction."[85] Heisenberg condemned causal interpretations as "ideological."[86] Bohr and Born always emphasized the epistemological nature of the choices related to interpreting the quantum formalism.

Consequently, physicists in the early 1950s saw the controversy as a strictly philosophical dispute concerning ontology (the constitution of the microsystem as waves or/and particles) and epistemology (the status of determinism in physical theories, the completeness of theories, the role of the space-time description). They often used "metaphysical" to characterize disputes without implications for the development of physics. Even physicists who tried to present the controversy impartially shared this view. For example, Albert Messiah's influential textbook, first published in 1958, taught that "the controversy has finally reached a point

83. Léon Rosenfeld, "Heisenberg, physics and philosophy," *Nature 186* (1960), 1960, 830-831; ibid., "Berkeley *redivivus*," *Nature, 228* (1970), 479. Olival Freire Jr. (ref. 65). Pauli to Heisenberg, 13 May 1954; Pauli to Rosenfeld, 28 Sep 1954, *Wolfgang Pauli – Scientific correspondence,* Vol. IV, Part II (1953-1954), [ed. by Karl von Meyenn] (Berlin, 1999), 620-621, 769.
84. Pauli (ref. 39). Emphasis added.
85. Rosenfeld (ref. 31).
86. Werner Heisenberg, *Physics and philosophy* (New York, 1958), 133.

where it can no longer be decided by any further experimental observations; it henceforth belongs to the philosophy of science rather than to the domain of physical science proper."[87] Similarly Fritz Bopp spoke for many in characterizing a conference dedicated in 1957 to foundational problems in quantum mechanics: "what we have done today was predict the possible development of physics—we were not doing physics but metaphysics."[88]

The main result Bohm and his collaborators obtained was the empirical equivalence with nonrelativistic quantum mechanics. They searched in vain for predictions not foreseen by the usual quantum mechanics and also failed to find a satisfactory relativistic generalization of their approach.[89] The absence of new results reinforced the derogatory label of "philosophical" stuck on them by their opponents. This label was damaging as it put off young physicists. A career in physics is not a career in philosophy. The label Vigier chose—"illustrative of dialectical materialism"[90]—appealed to Marxist physicists, but effective as it was in the 1950s, could not sustain a research program.

The working of a monocracy—quantum mechanics training and research agenda

Max Jammer referred to the "almost unchallenged monocracy of the Copenhagen school in the philosophy of quantum mechanics." John Heilbron, discussing the first missionaries of the Copenhagen, showed that beyond Bohr's close circle (Heisenberg, Pauli, Jordan, Born, Rosenfeld) and their brilliant opponents (Einstein, Schrödinger), physicists did not consciously adhere to complementarity or criticize

87. Albert Messiah, *Quantum mechanics* (Amsterdam, 1964), Vol. 1, 48. However, he did not please the hard core of the supporters of the Copenhagen interpretation. Rosenfeld wrote to him praising the book, but in disagreement with his diagnosis of the controversy. For Rosenfeld, "Ce n'est pas en effet d'expérience, mais bien de simple logique qu'il s'agit ici." Léon Rosenfeld to Albert Messiah, 16 Jan 1959, RP.

88. Bopp, in Stephan Körner, ed., *Observation and interpretation in the philosophy of physics, with special reference to quantum mechanics* (New York, 1957), 51. By the way, Bopp was working on another alternative interpretation, the so-called "stochastic interpretation."

89. My previous writings on Bohm's case overestimated these aspects, since they were not put in the broader context that I am discussing in this paper. Two examples of casual interpretation supporters disenchanted with such results are Mario Bunge and Philippe Leruste. "However, as time went by and no new predictions came out of the new formulation, I started to have doubts. Then, in 1964, when I started working on the axiomatization of NRQM for my *Foundations of physics* (Springer, 1967), I realized that Bohm's was not a valuable addition to standard QM and that the solution to his (and de Broglie's and Einstein's) problems lay elsewhere, namely in a realistic reinterpretation of standard QM." Mario Bunge to the author, 1 Nov 1996 and 12 Feb 1997. Ph. Leruste to the author, 27 Jan 1992.

90. Jean-Pierre Vigier, "Quelques problèmes physiques posés par les theses de Lénine," *La pensée*, *57* (1954), 60-66.

it, but rather used the quantum machinery to scrutinize the microscopic world. Heilbron also suggested that the philosophical flavor of Bohr's views on the interpretation of quantum mechanics contributed to American and British indifference to complementarity.[91] Sam Schweber added two American peculiarities, both of them hostile to philosophizing about quantum mechanics: the placing of theoretical and experimental physicists in the same departments, and American trends toward pragmatism.[92] Analyzing how American physicists reacted in the 1920s to the philosophical problems of quantum theory, Nancy Cartwright observed: "Americans in general had little anxiety about the metaphysical implications of the quantum theory; and their attitude was entirely rational given the operationalist-pragmatist-style philosophy that a good many of them shared."[93] These characteristics intensified after World War II. According to David Kaiser, "the pedagogical requirements entailed by the sudden exponential growth in graduate student numbers during the cold war reinforced a particular instrumentalist approach to physics." In this context, Kaiser continues, "epistemological musings or the striving for ultimate theoretical foundations—never a strong interest among American physicists even before the war—fell beyond the pale for the postwar generation and their advisors."[94]

The textbooks from which physicists learned quantum mechanics until the 1950s did not "reflect much concern at all about the interpretation of the theory."[95] According to Helge Kragh, "most textbook authors, even if sympathetic to Bohr's ideas, found it difficult to include and justify a section on complementarity. Among forty-three textbooks on quantum mechanics published between 1928 and 1937, forty included a treatment of the uncertainty principle; only eight of them mentioned the complementarity principle."[96] Bohr's epistemological writings were circulated in

91. J.L. Heilbron, "The earliest missionaries of the Copenhagen spirit," Peter Galison et al, eds., *Science and society: The history of modern physical science in the twentieth century,* (New York, 2001), *4*, 295-330.

92. Sam Schweber, "The empiricist temper regnant: Theoretical physics in the United States 1920-1950," *HSPS*, *17:1* (1986), 55-98.

93. Nancy Cartwright, "Philosophical problems of quantum theory: The response of American physicists," in Lorenz Krüger et al, eds., *The probabilistic revolution* (Cambridge, 1987), *2*, 407-435. For an analysis along the same lines, see Katherine R. Sopka, *Quantum physics in America 1920-1935* (New York, 1980), 3.67-3.69. Assmus suggests a more prosaic explanation for this little anxiety, since her thesis is: "aspiring quantum scientists chose the field of molecular structure in which to make their mark, avoiding the competitive field of atomic physics, which by the 1920s has become the cutting-edge of European physics." Philosophical problems arose mainly in atomic physics, once they were just implicit in molecular physics. Alexi J. Assmus, "The Americanization of molecular physics," *HSPS, 23:1* (1992), 1-34. I thank David Kaiser for calling my attention to this paper.

94. Kaiser, "Cold war" (ref. 4), 154-156.

95. Jagdish Mehra and Helmut Rechenberg, *The historical development of quantum theory* (New York, 2001), *6:2*, 1194.

96. Helge Kragh, *Quantum generations: A history of physics in the twentieth century* (Princeton, 1999), 211.

papers presented in scientific meetings and printed in anthologies, not textbooks.[97] That worried Rosenfeld. "There is not a single textbook of quantum mechanics in any language in which the principles of this fundamental discipline are adequately treated, with proper consideration of the role of measurements to define the use of classical concepts in the quantal description….there is thus most obviously an urgent need for a good elementary treatise….But it will be extremely difficult to find an author for such a book: those who have the competence to write it are too busy with other problems."[98] He urged Bohr to do it, "There is great interest in the topic among chemists and biologists, but there is no book that one can refer them to and that could protect them from the confusion created by Bohm, Landé, and other dilettantes. I will now do my bit here in Manchester by giving a lecture for chemists and biologists; but nothing can replace the book that *you* must write."[99]

The absence of foundational issues from the textbooks, ultimately ran against complementarity. As the number of people interested in such issues grew, especially in the 1960s, ignorance of complementarity eased the way for its critics.[100]

A "field of struggles to conserve or transform this field of forces"[101]

In a well argued paper of 1977, Trevor Pinch used Pierre Bourdieu's sociological concept of "scientific field" to argue that Bohm had successfully followed a "succession strategy" before 1952 (accumulating symbolic capital), and had then switched to a "subversion strategy" with the publication of his heterodox paper on "hidden variables." Pinch doubted whether Bohm's strategy of publishing an "interpretation" had been advisable; he might have foreseen the "conservationist strategy" of the "elite" of quantum physics. As he wrote, [102]

97. Thomas Kuhn, *The structure of the scientific revolutions* (Chicago, 1970).
98. Léon Rosenfeld, "Report on L. de Broglie" (ref. 72). The report follows: "The nearest to a really good treatment is found in Landau and Lifschitz's outstanding treatise: but it is too short and not explicit enough to be a real help to the student. The only books which are purposely devoted to an exposition of the principles are v. Neumann's aforementioned treatise and a little book by Heisenberg: the first is (as stated above) misleading in several respects, the second is too sketchy and on the subject of measurements it even contains serious errors (however surprising this may appear, the author being one of the founders of the theory). As to Bohr's authoritative article, it is in fact only accessible to fully trained specialists and too difficult to serve as an introduction into this question."
99. Léon Rosenfeld to Niels Bohr, 14 Jan 1957. Bohr scientific correspondence (31), Archives for the History of Quantum Physics, American Philosophical Society.
100. Freire (ref. 3).
101. For Bourdieu, "the scientific field, like other fields, is a structured field of forces, and also a field of struggles to conserve or transform this field of forces." Pierre Bourdieu, *Science of science and reflexivity* (Cambridge, 2004), 33, transl. R. Nice. For a critical review of Bourdieu's stance on the new sociology of science, Hélène Mialet, "The 'righteous wrath' of Pierre Bourdieu," *Social studies of science*, *33:4* (2003), 613-621.
102. Trevor Pinch, "What does a proof do if it does not prove? A study of the social

> The attacks on Bohm by the quantum elite can be regarded as part of what Bourdieu calls the conservationist strategy to be followed by the elite to ensure continual return on their investments. Bohm, by advocating a heterodox interpretation, was challenging the elite's authority by questioning the legitimacy of their previous investments in the interpretation of quantum theory. The official-history mode articulation of von Neumann's proof can be regarded then as an attempt to maintain a particular authority structure.

Although Pinch directed a few words to Pauli and Rosenfeld, stating that their criticisms are "along the metaphysical dimension of scientific activity and do not involve the construction of a specific cognitive object onto which the dispute could crystallize," most of his analysis concerned the symbolic role of von Neumann's proof. But that focus is too narrow to catch all the monopolies Bohm was challenging with his "subversion strategy."[103] Von Neumann's proof did not play a role in the arguments by Rosenfeld, Pauli, and Heisenberg, nor in the skepticism of physicists like Feynman, Beck, and Leite Lopes. The investment in the idea that complementarity had solved the foundational problems of quantum mechanics was larger than the investment in von Neumann's proof, and defended with more determination.

Von Neumann did not publish any paper criticizing the causal interpretation. Bohm reported that "von Neumann thinks my work correct, and even 'elegant,' but he expects difficulties in extending it to spin."[104] Von Neumann probably interested himself in Bohm's work in the 1950s while revising the English translation of his *Mathematische Grundlagen der Quantenmechanik* (1932), in which his famous proof appeared. To his publisher, he explained the difficulties, "the text had to be extensively rewritten, because a literal translation from German to English is entirely out of question in the field of this book. The subject-matter is partly physical-mathematical, partly, however, a very involved conceptual critique of the logical foundations of various disciplines….This philosophical-epistemological discussion has to be continuously tied in and quite critically synchronized with the parallel mathematical-physical discussion." Michael Stöltzner has recently given a plausible account of von Neumann's criteria for the success of physical theory that suggests how he might have evaluated Bohm's work:[105]

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conditions and metaphysical divisions leading to David Bohm and John von Neumann failing to communicate in quantum physics," Everett Mendelsohn et al, eds., *The social production of scientific knowledge* (Dordrecht, 1977), 171-216, on 206. I thank Joan Bromberg for calling my attention to this paper.

103. Pinch, ibid., 183.

104. By contrast, in the same letter, Bohm says that "the elder Bohr [Niels Bohr] didn't say much to Art[hur] Wightman, but told him he thought it 'very foolish.'" David Bohm to Melba Phillips, n.d., BP (C.46 – C.48). The same comment on von Neumann's reaction can be found in David Bohm to Wolfgang Pauli, [Oct 1951], in Pauli (ref. 23), 389-394.

105. John von Neumann to H. Cirker, [President of Dover Pub], 3 Oct 1949. John von

> [These criteria were] empirical adequacy in the narrow sense…simplicity of the description scheme, heterogeneity of the material described by it, and fertility for further developments; as to these aesthetic criteria, the Bohm program performs rather poorly…von Neumann could accept Bohm's proposal as an interesting model, but not as a promising interpretation.

Pauli and Stöltzner judged that von Neumann was the most conciliatory reaction to [Bohm's interpretation] among the fathers of the 'Copenhagen interpretation.'" Had Bohm remained in Princeton, he and von Neumann might have had productive discussions.

Catherine Chevalley convincingly argued that one cannot understand Bohr's reflections independent of their context which related, on one hand, to the "history of atomic physics," and, on the other, to the "history of a philosophical tradition widely different in content from either logical positivism or *Lebensphilosophie*." Chevalley remarked that "the term 'Copenhagen interpretation' appear[ed] only in the mid-1950's in the context of hidden-variables and Marxist materialism." This led her to conjecture that Bohr's thoughts were distorted and assimilated to a term whose exact content has been the object of wide disagreement. Chevalley also remarked that the first to use the term "Copenhagen school" was, among critics, the Soviet physicist Blokhinzev and, among supporters, Heisenberg.[106] Rosenfeld criticized Heisenberg for the usage: the label might induce people to admit the existence of other interpretations. Heisenberg conceded the point: [107]

> I avow that the term "Copenhagen interpretation" is not happy since it could suggest that there are other interpretations, like Bohm assumes. We agree, of course, that the other interpretations are nonsense, and I believe that this is clear in my book [*Physics and Philosophy*], and in previous papers. Anyway, I cannot now, unfortunately, change the book since the printing began enough time ago.

The term "Copenhagen school" first became current in the 1950s. The battles of that decade affected the causal interpretation, isolating it among physicists, and also the standard interpretation, distorting the views of the founding fathers.

Neumann Papers [Box 27, Folder 8], Library of Congress, Washington. John von Neumann, *Mathematical foundations of quantum mechanics* (Princeton, 1955). Michael Stöltzner, 1999. "What John von Neumann thought of the Bohm interpretation," Daniel Greenberger et al, eds., *Epistemological and experimental perspectives on quantum physics* (Dordrecht, 1999), 257-262, 260. On Bohm's program he wrote: "First, one is faced with two equations instead of one, without gaining new empirical predictions….Due to its inherent non-locality, the Bohm interpretation is hardly fertile in the view of the successes of local quantum field theories in elementary particle physics….Bohm's interpretation does not contain any new constant that would represent some new subquantum physics." On von Neumann and foundations of quantum mechanics, see Miklós Rédei and Michael Stöltzner, *John von Neumann and the foundations of quantum mechanics* (Dordrecht, 2001).

106. Catherine Chevalley, "Why do we find Bohr obscure?", Greenberger (ref. 105), 59-73.

107. Werner Heisenberg to Léon Rosenfeld, 16 Apr 1958, RP.

Some comparative perspectives

The main actors in the battle over the causal interpretation lived in Europe: Pauli, Rosenfeld, Born, Heisenberg, Fock, de Broglie, and Vigier. In America, the main reaction came from Einstein, who criticized complementarity, the causal interpretation, and McCarthyism.[108] Thus the fate of the causal interpretation in the 1950s was decided by actors not influenced by McCarthyism. Nor did Bohm's status as a Communist and a victim of McCarthyism work against him or his proposal in Brazil.

Nonetheless, Bohm's persona as a Communist victim of McCarthyism and the support of his program by young Marxist physicists in France did influence the reception of the causal interpretation. Marxist criticism against the complementarity interpretation in the USSR and in the West was more influential than previously recognized.[109]

Here the saga of Hugh Everett III provides a useful comparison. Everett wrote a thesis at Princeton and, also like Bohm, produced an alternative interpretation of quantum mechanics that was equally poorly received in its first ten years and later revived. Differently from Bohm, however, Everett did not have to face McCarthyism or exile.[110]

Everett argued the need for a new interpretation for quantum theory in order to meet the challenge of quantizing general relativity. He remarked that quantum mechanics is incomparable with the idea of a closed universe, a concept essential for cosmologists, since "the whole interpretive scheme of [its[formalism rests upon the notion of external observation." Von Neumann's presentation of quantum theory rests on the axiomatic distinction between two processes of evolution of the quantum states: one is discontinuous and not ruled by the Schrödinger equation, and happens during observations; the other is the deterministic change of an isolated system, governed by the Schrödinger equation, which takes place in the absence of measurements. Everett dispensed with the first of von Neumann's processes and pushed to its ultimate consequences a quantum treatment based exclusively on the second process. Everett considered the measuring device as a subsystem subject to quantum mechanics. This argument was in line with von Neumann's mathematical approach but far from Bohr's insistence that laboratory apparatus should be treated according to classical physics. Everett's *tour de force* was to at-

108. Abraham Pais, *Einstein lived here* (New York, 1994); Fred Jerome, *The Einstein file* (New York, 2002).

109. Loren Graham, *Science and philosophy in the Soviet Union* (New York, 1972), 19; Olival Freire Jr., "Marxism and quantum controversy: Responding to Max Jammer's question," unpub. paper, conference on "Intelligentsia: Russian and Soviet science," University of Georgia, Oct 2004.

110. I have presented elsewhere what I called "the many lives of Everett's interpretation," contrasting the obstacles faced by Everett while doing his dissertation with the renaissance of his ideas ten years later; Freire (ref. 3).

tribute physical reality to an ever branching universe, each branch being the state of a subsystem plus the related state of the whole system, at a moment immediately after each physical interaction. Even though this scheme is far from intuitive, it is not logically inconsistent, "since all the separate elements of a superposition individually obey the wave equation with complete indifference to the presence or absence ("actually" or not) of any other elements." Our common-sense intuition is preserved because "this total lack of effect of one branch on another also implies that no observer will ever be aware of any 'splitting' process."[111]

Everett recommended his approach as "a *metatheory* for the standard theory," and described its advantages in dealing with "imperfect observations and approximate measurement" and approaching "quantization of general relativity." Nonetheless, heresy is heresy and Everett emphasized his distance from Bohr's epistemological considerations: "The particular difficulties with quantum mechanics that are discussed in my paper have mostly to do with the more common (at least in this country) form of quantum theory, as expressed, for example, by von Neumann, and not so much with the Bohr (Copenhagen) interpretation. The Bohr interpretation is to me even more unsatisfactory, and on quite different grounds." Everett in effect combined Bohm's realism and von Neumann's quantum treatment of measuring devices. Everett's dissertation put Wheeler in a quandary. He early approved the physico-mathematical scheme, "the correlation [paper] seems to me practically ready to publish," but he disliked Everett's epistemological considerations, which included a section on the different interpretations of quantum mechanics. "I am frankly bashful about showing it to Bohr in its present form, valuable and important as I consider it to be; because of parts subject to mystical misinterpretations by too many unskilled readers." As a follower of Bohr, Wheeler could not accept Everett's rejection of complementarity.[112]

Wheeler had the idea of convincing Bohr of the value of Everett's approach and persuading Everett to remove the offensive epistemological considerations from his dissertation. He hoped thereby to arrange for the publication of Everett's dissertation in full by the Danish Academy of Sciences which would legitimize it among the supporters of complementarity.[113] In 1956, with a draft of the dissertation in his luggage, Wheeler went to Copenhagen to review it with Bohr. Aage Petersen and Alexander Stern could not, and did not, accept Everett's ideas about observation in quantum mechanics. Stern described Everett's point of view as theology, Wheeler

111. Hugh Everett III, " 'Relative state' formulation of quantum mechanics," *Reviews of modern physics*, *29* (1957), 454-462.

112. Hugh Everett to Aage Petersen, 31 May 1957. WP, Series I, Box Di—Fermi Award #1, Folder Everett. For the first version of the dissertation, Hugh Everett, "The theory of the universal wave function," Bryce DeWitt and Neill Graham, eds., *The many-worlds interpretation of quantum mechanics* (Princeton, 1973), 3-140; John Wheeler to Everett, 21 Sep 1955, Everett Papers, Series I-5, American Institute of Physics, College Park, MD.

113. John Wheeler to Allen Shenstone, 28 May 28 1956, WP Series I, Box Di – Fermi Award #1, Folder Everett.

rejoined: "If it is a theological statement to postulate the 'universal wave function,' it is also a theological statement to refuse to entertain the postulate." Rosenfeld set forth the position of Bohr's camp:[114]

> [Everett's] work suffers from the fundamental misunderstanding which affects all 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 the 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.

Wheeler did not surrender. He recommended approval of the dissertation and a stay for Everett in Copenhagen. The dissertation was duly approved in 1957 and published abridged in a special issue of *Reviews of modern physics*, along with the proceedings of a conference that Everett had not attended and a note by Wheeler about the possible convergence between Everett's ideas and complementarity. Everett went to Copenhagen in 1959, but the discussions with Bohr bore no fruit. Disillusioned with the whole affair and satisfied with his work on game theory and computers for the Pentagon, Everett abandoned physics. He never again wrote on the interpretation of quantum mechanics, even when his ideas were revived by others ten years. During its first decade in print, his paper received no more than 20 citations.[115]

Blocked by the Copenhagen monocracy Everett's ideas had a fate similar to Bohm. But Everett did not face the political harassment that Bohm endured. Rather than suffer, Everett had a good job at the Pentagon when his "relative states" interpretation of quantum mechanics suffered unfavorable reception. Once again McCarthyism does not appear as the major obstacle to the acceptance of Bohm's physics.

114. John Wheeler to Alexander Stern, 25 May 1956. WP, Series 5—Relativity notebook 4, p. 92. The sentence is handwritten on the typed letter. It is also written "CWM", which suggests Charles W. Misner was its author. Léon Rosenfeld to Saul M. Bergmann, 21 Dec 1959, RP.

115. Hugh Everett (ref. 112); John Wheeler, "Assessment of Everett's 'relative state' formulation of quantum theory," *Reviews of modern physics, 29* (1957), 463-465; Hugh Everett, interviewed by C.W. Misner, May 1977. Tape transcribed by E. Shikhotsev. Everett Papers, series I-3, American Institute of Physics, College Park, MD; Freitas and Freire (ref. 3).

3. THE RUSE OF HISTORY

The monocratic Copenhagen school divided during the 1960s over a dispute between Wigner and Rosenfeld about the measurement problem in quantum mechanics. One fraction remains with Bohr; the other, the Princeton school, roughly centered on von Neumann and Wigner. Research on the foundations of quantum mechanics flourished in the 1970s and the 1980s, especially over issues related to John Bell's theorem and the measurement problem. It is an historical irony that the main scientific contribution from this field of research—the Bell theorem and experimental tests confirming quantum mechanical predictions and refuting locality—was motivated by Bohm's insistence on hidden variables. Bell owed more to Bohm than is usually recognized.[116]

"Smitten by Bohm's paper," Bell attempted to determine what was wrong with von Neumann's proof, since it did not allow for hidden variables in quantum mechanics. Bell knew the proof only indirectly, from his reading of Max Born's *Natural philosophy of cause and chance*; he could not read von Neumann's book himself. Since he could not read German and the English edition had not yet appeared. He appealed to Franz Mandl, his colleague at Harwell. "Frank was of German origin, so he told me something of what von Neumann was saying. I already felt that I saw what von Neumann's unreasonable axiom was." He wrote to Pauli asking for reprints of his papers on Bohm's proposal. Bell went to study in Birmingham in 1953. Asked by Rudolf Peierls, who would become his adviser, to give a talk about what he was working on, "Bell gave Peierls a choice of two topics: the foundations of quantum theory or accelerators." Peierls chose the latter, which was the end of the first stage of Bell's involvement with hidden variables.

Bell resumed this work at Stanford during a leave of absence from CERN. In the first of two articles on the foundations of quantum mechanics that he published while in the U.S., he acknowledged for "intensive discussion" with Mandl in 1952 and, subsequently, with Professor J.M. Jauch. In a later interview Bohm discussed more fully the origins of the Stanford paper, [117]

> I had once again begun considering the foundations of quantum mechanics, stimulated by some discussions with one of my colleagues, Josef Jauch. He, it turned out, was actually trying to strengthen von Neumann's infamous theorem. For me, that was like a red light to a bull. So I wanted to show that Jauch was wrong. We had gotten into some quite intense discussions. I thought I had located the unreasonable assumption in Jauch's work.

116. Freire (ref. 3); Olival Freire Jr., "Orthodoxy and heterodoxy in the research on the foundations of quantum physics: E.P. Wigner's case," Boaventura S. Santos, ed., *Cognitive justice in a global world: Prudent knowledge for a decent life* (forthcoming).
117. Jeremy Bernstein, *Quantum profiles* (Princeton, 1991), 65-68. Wolfgang Pauli to John Bell, 23 Jan 1953, Pauli (ref. 83), on 28. John Bell, "On the problem of hidden variables in quantum mechanics," *Reviews of modern physics, 38* (1966), 447-452.

A few words from the paper published by Jauch and Piron will explain what was at stake:[118]

> There are several reasons why we propose to re-examine here von Neumann's proof again. First of all there seems to be a renewed interest in a critique of the foundations of quantum mechanics and some of the recent attempts in this direction have not always done full justice to von Neumann….Bohm in his book [*Causality and chance in modern physics*, 1957] even goes so far as to accuse von Neumann of circular reasoning. If this were true, this "proof" would mean, of course, exactly nothing and would leave all doors open for speculations on a "sub-quantum mechanical level" and a "deeper reality" so dear to the above-mentioned authors.

Bell accordingly addressed his first paper to those who believe that the "the question concerning the existence of such hidden variables received an early and rather decisive answer in the form of von Neumann's proof on the mathematical impossibility of such variables in quantum theory." Bell's work therefore falls in the tradition of reinforcing proofs against hidden variables, a tradition that had been challenged by Bohm, de Broglie, and their collaborators. If the possibility of introducing hidden variables in quantum mechanics was Bell's motive, his approach differed much from Bohm's. He was not interested in building viable models mimicking quantum mechanics; instead, he subjected von Neumann's proofs and, later on, also the assumptions behind the Einstein-Podolsky-Rosen *gedankexperiment*, with intense scrutiny. Bell's theorem contrasted quantum mechanical predictions with a family of hidden variables that fulfilled the criterion of locality. This criterion, relevant to Einstein's reasoning, requires that measurement of a property of one of two particles that once interacted does not affect any property of the other after their separation. The Bell theorem is that no local hidden variable theory can recover all quantum mechanical predictions, and the quantitative measurements of this shortfall are the Bell inequalities. These inequalities have motivated a cornucopia of experiments. By arriving at this theorem, Bell had shown both the restrictive assumption in von Neumann's proof (the additivity of the expectation values) and why Bohm's hidden variables were possible (they were as nonlocal as quantum theory).[119]

Independent of its intrinsic merits, which still awake passions, Bohm's hidden variables gave him a role in the history of physics comparable to Kepler, who contributed to the creation of modern science while looking for celestial music in the

118. J.M. Jauch and C. Piron, "Can hidden variables be excluded in quantum mechanics?" *Helvetia physica acta, 36* (1963), 827-837, on 827. Emphasis in the original.
119. Bell (ref. 117); ibid., "On the Einstein Podolsky Rosen Paradox," *Physics, 1* (1964), 195-200. Michael Stöltzner, "Bell, Bohm, and von Neumann: Some philosophical inequalities concerning no-go theorems and the axiomatic method," Tomasz Placek and Jeremy Butterfield, eds. *Non-locality and modality* (Dordrecht, 2002), 37-58.

planetary system. In a rough analogy, Newton depended on Kepler as Bell depended on Bohm. The comparison is not mine. In 1958, Lancelot Whyte, an engineer and philosopher of science, defending Bohm against Rosenfeld's attacks, wrote to Rosenfeld, "Naturally you are fully aware…that valuable results may spring from mistaken motives and reasoning. Kepler is a good example. But this awareness is not evident in your review." Bohm would have enjoyed this comparison, if he had known of it.[120]

120. Lancelot Whyte to Léon Rosenfeld, 8 Apr 1958, RP.

OLIVAL FREIRE JR.*

Science and exile: David Bohm, the Cold War, and a new interpretation of quantum mechanics

ABSTRACT

In the early 1950s the American physicist David Bohm (1917-1992) produced a new interpretation of quantum mechanics and had to flee from McCarthyism. Rejected at Princeton, he moved to São Paulo. This article focuses on the reception of his early papers on the causal interpretation, his Brazilian exile, and the culture of physics surrounding the foundations of quantum mechanics. It weighs the strength of the Copenhagen interpretation, discusses the presentation of the foundations of quantum mechanics in the training of physicists, describes the results Bohm and his collaborators achieved. It also compares the reception of Bohm's ideas with that of Hugh Everett's interpretation. The cultural context of physics had a more significant influence on the reception of Bohm's ideas than the McCarthyist climate.