Chapter 2
Challenging the Monocracy of the Copenhagen School

Abstract Quantum mechanics—the physical theory for atoms, radiation and their interaction—was developed in the first quarter of the twentieth century. This was accompanied by a quarrel, with philosophical overtones, on its interpretation. Bohr called it complementarity and later it was labeled the Copenhagen interpretation. Complementarity spurred a debate among giants such as Bohr and Einstein. In 1952 David Bohm made the boldest challenge to this interpretation suggesting instead a causal interpretation. The proposal was harshly criticized by most commentators and supported by just a few. Bohm had joined the Communist Party in 1943 while at Berkeley and was caught in the witch-hunt of the McCarthyism era. He opted for a life of exile in Brazil, then Israel, and eventually England. His passport was confiscated by U.S. officials and his citizenship was revoked. While some Soviet scholars criticized the complementarity interpretation as idealistic, and thus bourgeois, they did not endorse Bohm's endeavor to recover determinism, which frustrated Bohm. At the forefront of this battle it was two giants who quarreled: Bohm and Rosenfeld. Both gifted physicists and dedicated Marxists, they nonetheless disagreed about how to interpret physics and its philosophical lessons. Bohm promised to generalize his approach for the relativistic domain, but this was not fulfilled. In the late 1950s, Bohm experienced a major intellectual change. He broke with Marxism, abandoned the causal interpretation, moved towards Eastern thinkers and began a long-standing project of reforming physics along the themes of order and wholeness.

2.1 Interpretation of Quantum Theory Before David Bohm[1]

The inception of quantum physics, between 1925 and 1927, and its early origins dating back to 1900, along with the debates about its interpretation, are some of the topics better exploited in the literature concerning the history of physics of the

[1] In this chapter I draw from some of my previous works, in particular (Freire Jr. 1999, 2005, 2011a, b).

O. Freire Junior, *The Quantum Dissidents*, DOI 10.1007/978-3-662-44662-1_2

twentieth century. As it is impossible to give a fair account of that history in a few introductory lines, I will only refer to some milestones of the debates before Bohm entered the scene in the early 1950s. Quantum physics began with old phenomena concerning electromagnetic radiation and its interaction with matter, and a few new ideas, such as the quantum of action and the granularity of light. Its development was marked by a close relationship between novel ideas and precision measurements of new and old phenomena, which led to a completely new theoretical landscape between 1925 and 1927. This landscape needed to be interpreted in terms of physics in order to make sense of its abstract mathematical formalism. This was divisive for physicists, creating what the philosopher Karl Popper (Popper and Bartley 1982) would later call "the schism in Physics," a controversy now more than 8 decades old and only comparable to the one that pitted Newtonians against Cartesians at the dawn of modern physics.

One of the poles of the controversy was the line of interpretation developed by the Danish physicist Niels Bohr, which he christened "the complementarity view." In general, he considered complementarity's main features to be the following: Max Born's quantum probabilistic descriptions are non-reducible to deterministic descriptions; quantum jumps are intrinsic to quantum descriptions; the means of observation play a prominent role, i.e. quantum phenomena should consider both the system and the observation devices; classic concepts, such as wave and particles, are used in a complementary manner, i.e. not jointly in the same experiments but in mutually exclusive ways; discreteness of the physical magnitude action is a fundamental feature of nature; and quantum theory is considered a complete theory, i.e. not superseded by other theories dealing with phenomena concerning radiation and its interaction with matter.

Bohr's thoughts were presented embedded in philosophical considerations concerning the role of ordinary language in guaranteeing objectivity to research in physics. First considered "obscure" by many, Bohr's philosophy has been scrutinized by philosophers in recent decades. Shoulder-to-shoulder with Bohr were some of the creators of quantum theory, such as Werner Heisenberg, Max Born, Pascual Jordan, and Wolfgang Pauli, each with subtle differences in their interpretations. However, not all the founding fathers of quantum physics aligned themselves with Bohr's complementarity. Albert Einstein was initially skeptical about the consistency of the theory, and later about its completeness, producing several *Gedankenexperiments* to reveal shortcomings in the theory; Louis de Broglie tried to maintain determinism and the images of wave and particles, suggesting a model of particles being piloted by waves; and Erwin Schrödinger did not accept quantum jumps and pleaded for a wave representation of quantum phenomena.

In parallel to more philosophical and conceptual debates, John von Neumann looked for a rigorous presentation of the quantum mathematical formalism in an attempt to replace the coexisting, equivalent presentations of the quantum theory in terms of matrixes, wave functions, and algebras. The Hungarian-born mathematician described all these previous presentations as special cases of a more general mathematical framework, namely that of Hilbert's space vectors. Von Neumann's presentation would leave a lasting imprint on the debates on the foundations of quantum physics as many of these debates took von Neumann's as the orthodox presentation of quantum theory. In particular, he formalized what quantum physicists call "the reduction of the wave packet" to describe measurement processes as an independent axiom in quantum theory (technically he described it as the workings of a projection operator). It meant that measurements were not ruled by the Schrödinger equation which now describes only the evolution of quantum states before measurement processes. Later, this formulation would be the standard introduction to the intractable measurement problem, the problem of the process through which superposition of quantum states, the most fundamental quantum feature, disappears during measurements. In addition, he provided a theorem prohibiting the enlargement of quantum theory with additional or "hidden" variables. As we will see, one of David Bohm's first achievements with his causal interpretation was to create a practical counterexample to this rule.

The controversy on the interpretation and foundations of quantum physics, "quantum controversy" as shorthand, was a heated topic among physicists in 1927, particularly at the Fifth Solvay International Conference on Electrons and Photons held in Brussels in October of that year (Institut International de Physique Solvay 1928; Bacciagaluppi and Valentini 2009). In the following years the debates between Einstein and Bohr on this subject attracted the attention of physicists and images and text featuring the two giants quarreling are now iconic in the culture of physics. However, as time went by, physicists tended to attribute a less important role to the controversy as more mundane subjects occupied their agenda. Applications and extensions of quantum physics and mainly nuclear physics dominated the scene in the 1930s.

Picture 2.1 Bohr and Einstein's debate portrayed in the artistic imagination—Sculpture in Park Muzeon, Moscow. Photo by Climério P. da Silva Neto

During the war more practical efforts, mainly related to the building of the atomic bomb and radar, absorbed physicists' energies, at least in the US. After the war, old themes such as the discovery of new particles, fixing the machinery of quantum electrodynamics, and a renewed approach to solid state caught the attention of the physicists. Indeed, since the middle of the 1930s, the typical physicist either considered foundational issues to be off the main agenda of physics or thought that they had already been solved by Niels Bohr and his close companions. As reminded by the Danish physicist Christian Møller, assistant of Niels Bohr in Copenhagen, "although we listened to hundreds and hundreds of talks about these things, and we were interested in it, I don't think, except Rosenfeld perhaps, that any of us were spending so much time with this thing . . . When you are young it is more interesting to attack definite problems. I mean this was so general, nearly philosophical."[2] Those were the times the historian of physics Max Jammer

[2] C. Møller, interviewed by T.S Kuhn, 29 July 1963, Archives for the History of Quantum Physics (hereafter AHQP), American Philosophical Society, Philadelphia, PA, cited in Jacobsen (2012, p. 55).

referred to as the "almost unchallenged monocracy of the Copenhagen school in the philosophy of quantum mechanics." Although research on the foundations of quantum theory was not a priority in the early 1950s, interest in the subject was far from dead. Indeed the first salvos in the new battles were fired by Niels Bohr reviewing their disagreements in the paper ("Discussion with Einstein on Epistemological Problems in Atomic Physics") for the volume of "The Library of Living Philosophers" edited by P. A. Schilpp to honor Einstein (Bohr 1949; Einstein 1949; Schilpp and Einstein 1949). Bohr's paper and Einstein's reply played a role in arousing the dormant debate. Einstein himself continued to voice his discomfort with quantum physics and new critics of the complementarity view were appearing among Soviet physicists and philosophers.[3] All these criticisms influenced David Bohm, as we will see.

2.2 Bohm's Causal Interpretation of Quantum Mechanics

In early July 1951, the American physicist David Bohm, from Princeton University, submitted a lengthy paper entitled "A suggested interpretation of the quantum theory in terms of 'hidden' variables" to the prestigious journal *Physical Review*. The paper was organized in two parts, both published in early 1952, and the technical title hid its far-reaching philosophical implications (Bohm 1952b). Soon both David Bohm and his critics were using "causal interpretation" to label his approach to quantum theory, clarifying Bohm's ambition to restore a kind of determinism analogous to that of classical mechanics (Bohm 1952a, 1953a; Bohm and Vigier 1954). Unlike the early critics of quantum mechanics, Bohm did not just express hopes of going back to a causal description for atomic phenomena. In fact, he built a model for his approach which assumed that an object like an electron is a particle with a well defined path, which means it is simultaneously well defined in both position and momentum. It is noteworthy that in quantum theory it is precisely the impossibility of such a simultaneous determination which breaks with the classical determinism, while in classical mechanics the possibility of that simultaneous definition assures the classical deterministic description. Bohm's work had philosophical implications as a consequence of its physical assumptions. According to him, this interpretation "provides a broader conceptual framework than the usual interpretation, because it makes possible a precise and continuous description of all processes, even at the atomic level." More explicitly, he stated that,

> This alternative interpretation permits us to conceive of each individual system as being in a precisely definable state, whose changes with time are determined by definite laws, analogous to (but not identical with) the classical equations of motion. Quantum-

[3] For the debates before 1950 see Jammer (1974), for Jammer's quotation, see his p. 250. For the Soviet critics see Graham (1987).

> mechanical probabilities are regarded (like their counterparts in classical statistical mechanics) as only a practical necessity and not as a manifestation of an inherent lack of complete determination in the properties of matter at the quantum level. (Bohm 1952b, p. 166)

Fully aware of the philosophical implications of his proposal, Bohm concluded the paper by criticizing the usual interpretation of quantum mechanics on philosophical grounds. He accused "the development of the usual interpretation" of quantum theory of being "guided to a considerable extent by the principle of not postulating the possible existence of entities which cannot now be observed," and remarked that "the history of scientific research is full of examples in which it was very fruitful indeed to assume that certain objects or elements might be real, long before any procedures were known which would permit them to be observed directly," the case of the atomistic hypothesis being the best historical example. Bohm also noted that this principle derived from the "general philosophical point of view known during the nineteenth century as 'positivism' or 'empiricism.'" Then he explained to his readers that "a leading nineteenth-century exponent of the positivist view was Mach." While conceding that "modern positivists appear to have retreated from this extreme position," he stated that this position was still reflected "in the philosophical point of view adopted by a large number of modern theoretical physicists." Apart from this philosophical digression, the philosophical implications of Bohm's proposal concerned not only the recovery of determinism as a mode of description of physical phenomena, but also the adoption of a realist point of view toward physical theories, both discarded by the complementarity view.[4]

Later in his career, Bohm (1987, p. 33) emphasized that recovering determinism was not his main motivation and that his major dissatisfaction was that "the theory could not go beyond the phenomena or appearances." Building an ontology to explain phenomena would become a permanent goal in Bohm's research with determinism pushed down on his agenda. However, in the 1950s Bohm and the debate triggered by his proposal did indeed promote the recovery of determinism.

To illustrate the strength of the attachment of Bohm and his collaborators to the philosophical priority of causality, we can make reference to the work he and Jean-Pierre Vigier did in 1954, changing Bohm's original model slightly. In this work, they embedded the electron in a fluid undergoing "very irregular and effectively random fluctuation" in its motion (Bohm and Vigier 1954). While these fluctuations could be explained by either a deterministic or a stochastic description, Bohm and Vigier framed them into the causal interpretation approach, giving their paper the title "Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations."

---

[4] Bohm (Bohm 1952b, pp. 188–189). Bohm's reference to Ernst Mach, criticizing the positivist view, is a shibboleth of his Marxist background, a feature we will return to later, as this reference gained currency among Marxists in the first half of the twentieth century following the diffusion of *Materialism and Empirio-criticism* (Lenin 1947).

Bohm not only suggested a new conceptual and philosophical framework. He also raised the stakes by suggesting his approach could be fruitful in new domains of physics, promising that "modifications can quite easily be formulated in such a way that their effects are insignificant in the atomic domain [...] but of crucial importance in the domain of dimensions of the order of 10^{-13} cm." Bohm was indeed referring to intra-nuclear distances, an area in which there was a proliferation of discoveries of new particles requiring the development of new methods in quantum field theories. Bohm's promises, however, were as appealing as vague, saying that "it is thus entirely possible that some of the modifications describable in terms of our suggested alternative interpretation, but not in terms of the usual interpretation, may be needed for a more thorough understanding of phenomena associated with very small distances." The promise of fulfillment of such an expectation was then postponed: "we shall not, however, actually develop such modifications in any detail in these papers" (Bohm 1952b, p. 166).

In Bohm's original model, electrons suffer physical influences both from potentials, such as electromagnetic potentials, and from a new potential resulting from mathematical manipulations of the Schrödinger equation, which Bohm labeled the "quantum potential." Technically this new potential arises when one exploits analogies between the Schrödinger equation of quantum theory and the Hamilton-Jacobi equation of classical mechanics. To make a clear comparison, let us take an electron with well defined positions described by a function of the form $\psi = R$ exp $(iS/\hbar)$, which must satisfy the Schrödinger equation, and let us call $R(\mathbf{x})^2 = P(\mathbf{x})$. After some mathematical manipulations we get Eqs. (2.1) and (2.2) resulting from Bohm's approach.

$$\partial P/\partial t + \nabla(P\nabla S/m) = 0 \tag{2.1}$$

$$\partial S/\partial t + (\nabla S)^2/2m + V + U = 0 \tag{2.2}$$

where

$$U = -\left(\hbar^2/2\mathrm{m}\right)\left(\nabla^2 R/R\right)$$

Bohm then further exploited these analogies by suggesting that electrons have a well defined momentum $\mathbf{p} = \nabla S(\mathbf{x})$. The same analogies suggest that the "extra" term U in Eq. (2.2) may be interpreted as the action of a "quantum potential" on electrons, in addition to the potentials known from classical physics, such as electromagnetic potentials. In addition, according to this model, Eq. (2.1) is a continuity equation, and Bohm suggests that we take $P = |\psi(x)|^2$, where ψ is the solution of the Schrödinger equation, to assure the conservation of the probability density of an ensemble of particle positions. As remarked by Max Jammer (1988, p. 693), "Bohm interprets [P] as the probability of the particle's *being* at the position defined by the argument x of $\psi(x)$ and not, as Born conceived it, as the probability of *finding* the particle at that position if performing a suitable measurement." Bohm's model of electrons has 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. They are "hidden" when compared to standard quantum mechanics as Heisenberg's uncertainty relations forbid the simultaneous precise definitions of positions and momenta. Later on, however, the physicist John Bell, a supporter of Bohm's proposal, would consider Bohm had been unhappy choosing the term "hidden variables." Bell would remark that complementarity is the interpretation which hides either of the complementary variables as they could not be considered images of the phenomena (Bell 2004, p. 201).[5] In order to get models which were able to produce the same results as quantum mechanics, Bohm needed to ascribe well defined positions and momenta to the measurement devices too. Thus, from the Hamiltonian (kinetic plus potential energies) of the coupling between such devices and the micro systems, observable results could be predicted. Bohm used these models to carry out detailed calculations of a number of different problems, for instance, stationary states, transitions between stationary states (including scattering problems), the Einstein-Podolsky-Rosen *Gedankenexperiment*, and photoelectric and Compton effects. To achieve results compatible with those from quantum mechanics, Bohm modeled light as electromagnetic waves. In all these problems he found the results predicted by the usual mathematical formalism of quantum theory (Jammer 1988; Bohm 1952b, p. 183).

Bohm's achievement was not a minor one. He was able to build an approach to quantum theory leading to the same predictions as usual quantum mechanics and develop the first alternative interpretation to the dominance of the complementarity view among physicists. This empirical equivalence was dependent on adopting hidden variables in the system and in the measurement device, which was an improvement on his initial approach. This was done in reacting to criticisms made by Wolfgang Pauli. In fact, with this improvement Bohm's approach became superior to an earlier and analogous approach that had been suggested by Louis de Broglie in 1927, then entitled the "pilot-wave approach" (Bohm 1952b, pp. 191–193). This earlier approach was unknown to Bohm until he received Pauli's criticisms, as we will see later. To be more precise, Bohm's approach was equivalent to non-relativistic quantum mechanics as his electron model, for instance, did not have "spin."

That Bohm's approach was unable to deal with relativistic systems is clear from the equation of the quantum potential. Indeed, it is enough to take a system with two electrons to see that the quantum potential tells us that an interaction could propagate from one electron to the other instantaneously without any time dependency. This would not have been considered a major flaw when Bohm published his papers if one recalls that in the historical process of the creation of quantum physics, non-relativistic equations came first and relativistic generalizations a little

[5] According Bell's words, "absurdly, such theories are known as 'hidden variable' theories. Absurdly, for there is not in the wavefunction that one finds an image of the visible world, and the results of experiments, but in the complementary 'hidden'(!) variables." I am thankful to Michael Kiessling for calling my attention to Bell's remarks.

later. At any rate, critics would ask Bohm for these generalizations and Bohm would promise that they were under way.

Bohm's papers also raised other philosophical and technical issues. Empirically equivalent to the standard quantum mechanics, Bohm's would be a nice example of what philosophers call the underdetermination of theories by empirical data. According to the philosopher Paul Feyerabend, after Bohm's work, "it follows that neither experience nor mathematics can help if a decision is to be made between wave mechanics and an alternative theory which agrees with it in all those points where the latter has been found to be empirically successful" (Feyerabend 1960, p. 325). This philosophical thesis, also called the Duhem-Quine thesis, a reference to the scientist and philosopher Pierre Duhem and the philosopher Willard Van Orman Quine, was well set in logical terms but it was, and it is, at least unpleasant for physicists to realize that some of their best theories are not the only possible description of phenomena.[6] Finally, Bohm's approach was a practical example showing that something was wrong with von Neumann's mathematical proof against the possibility of introducing hidden variables in quantum mechanics. Bohm was fully aware of this in his approach, making it explicit in his papers, and he would attentively follow von Neumann's reactions to his proposals (Bohm 1952b, pp. 187–188). Finally, as we have already noted, Bohm did not refuse the philosophical debate implied by his proposals as he not only defended his approach with both technical and conceptual arguments, but also accused supporters of the standard interpretation of being the twentieth-century equivalents of the anti-atomists in the nineteenth century.

2.3 Backgrounds of Bohm's Causal Interpretation

Before analyzing the reception of Bohm's proposal by his fellow physicists, let us go back to see how Bohm evolved towards this interpretation of quantum mechanics. In addition, let us flesh out our history by considering the life and environment of the person involved. Bohm's proposal of a causal interpretation for quantum physics was a surprising move both on the physics scene at the time and in his own professional career. He had been awarded his PhD during World War II at Berkeley under the supervision of Robert Oppenheimer, who was then already involved in the Manhattan Project. His dissertation was dedicated to the subject of scattering in proton deuteron collisions, which was a sensitive subject for the Manhattan Project; thus it was immediately classified. As Bohm had no clearance to present his dissertation and did not work on the atomic project due to his union activities and links with the Communist Party he could not defend his PhD dissertation. Bohm was then involved with the American Communist Party and the union activities of

[6] On the Duhem-Quine thesis, see (Harding 1976). On quantum mechanics as an illustration of this thesis, see Cushing (1994).

technical and scientific workers. It was the eve of the war and Bohm was attracted to the Communist ranks as it seemed to him that the USSR could be an essential force against the Nazis. In this move he was joined by a few of Oppenheimer's students, all of whom paid a high price for it after the war. Subsequently Bohm broke his organizational ties with the Communist Party while keeping the same ideological inclinations until the late 1950s. According to historian Alexei Kojevnikov (2002, p. 166), "Bohm severed his ties with the organized communist movement while remaining a convinced Marxist with a special interest in the philosophy of dialectical materialism."

The problem was solved with Oppenheimer testifying to the quality of Bohm's dissertation, which led Berkeley to grant him his doctoral degree. Still during the war, as a research fellow at Berkeley, he began to work with the Australian Harrie S. W. Massey on the problem of electrical currents passing through a gas in magnetic fields, an issue considered relevant for the enrichment of uranium and thus part of the Manhattan Project. The problem led them to study plasmas, but the process of enrichment did not prove useful for the war effort. After the war, hired by Princeton University, Bohm and his graduate student Eugene Gross resumed work on plasmas, developing the approach called "collective variables in classical plasmas." Then, with the graduate student David Pines, Bohm moved to study current in metals, elaborating a quantum approach to the phenomenon using the same collective variable resource he had successfully applied to the classical treatment of plasmas. His jointly-authored papers with Pines and Gross would become landmarks in this field.[7]

Bohm was then considered by elder fellow physicists to be one of the most promising American theoretical physicists of his generation—"probably Oppenheimer's best student at Berkeley" according to historian Sam Schweber—and it was in this capacity that he was one of the few to be invited to the 1947 Shelter Island conference, the first of a series of conferences held in the US dealing with topics such as high energy nuclear physics, new nuclear particles, and anomalies and procedures for fixing quantum electrodynamics.[8] The list of topics on which Bohm had worked until the late 1940s did not presage his move towards working on the foundations of quantum physics.

Clues that might shed light on the inception of his alternative interpretation of quantum theory come from his teaching duties at Princeton. Having been educated at Berkeley, where "Bohr was God and Oppie [Oppenheimer] was his prophet," according to Weinberg, one of Oppenheimer's students at Berkeley, Bohm's classes on quantum mechanics naturally reflected Bohr's views on this theory. From these classes his textbook *Quantum Theory* (Bohm 1951) emerged. A close inspection of

[7] On Bohm's biography, see Peat (1997) and Mullet (2008b). For an analysis of Bohm's works, see Kojevnikov (2002). Bohm and Gross (1949a, b), Bohm and Pines (1951, 1953), Pines and Bohm (1952). The fourth paper in the series was authored only by Pines (1953).

[8] Sam Schweber, "Bohm Memorial," Folder A.M., David Bohm Papers, Birkbeck College, University of London (hereafter BP), cited in Mullet (2008a, p. 40) and Mehra (1994, pp. 217–218).

this book, however, reveals how far Bohm was from being truly Bohrian. Indeed, Bohm's *Quantum Theory* is remarkable for its attempt to combine Niels Bohr's complementarity with Bohm's own kind of realism. While the former denied quantum theory the ambition of describing a world independent of measurements, the latter included an ontological description of the quantum world, referred to by Bohm as "an attempt to build a physical picture of the quantum nature of matter." The book is also noteworthy for his conceptual clarity and a few innovations, such as the reformulation of the EPR thought experiment using spin instead of position and momentum, which later became the standard formulation for EPR theory and experiments due to its mathematical simplicity. Bohm also included a treatment of the measurement process using random phases, which he would use later in his work on the causal interpretation.[9]

When the book came out, Bohm was already moving towards the elaboration of his causal interpretation. Later he would acknowledge at least two influences on his move: a discussion with Albert Einstein at the Institute of Advanced Studies in Princeton after the book was published and the reading of a paper by a Soviet physicist criticizing the complementarity view for its idealistic and subjectivist inclinations. As told by historian Max Jammer,

> Stimulated by his discussion with Einstein and influenced by an essay which, as he told the present author, was "written in English" and "probably by Blokhintsev or some other Russian theorist like Terletzkii," and which criticized Bohr's approach, Bohm began to study the possibility of introducing hidden variables. (Jammer 1974, p. 279)

Later Jammer (1988, p. 692) reiterated this story in a kind of Festschrift for Bohm's 70th birthday. Bohm never contested it. This information, however, raised a doubt, as Jammer himself noted. "Bohm [had] forgotten the exact title and author of this paper" and there was no paper either by Blokhintsev or by Terletzkii published in English before Bohm's shift to the causal interpretation (Jammer 1974, p. 279 footnote 63). Indeed, the papers by the Soviets criticizing complementarity published in Western languages appeared in French in 1952, while Bohm's shift to the causal interpretation occurred in 1951.[10] The riddle may be explained by free translations from the Soviet papers which may have circulated among Marxist intellectual circles in the West before their publication. Plausible as this explanation is, unfortunately, we do not have documentary evidence to support it. Furthermore, the statement on the influence of the Soviet views in the inception of the causal interpretation is crucial information given Bohm's Marxist beliefs and the role played by the criticisms from Soviet philosophers and physicists against the complementarity interpretation in the quantum controversy (Graham 1972, 1987). However, and again unfortunately, archival documents unearthed since then have not been able to reinforce Jammer's interesting clue. Indeed, most of Bohm's personal papers did not survive and he did not keep copies of his correspondence. Later on, a few letters from him to some friends and fellow physicists surfaced but

---

[9] For Weinberg's statement, see Mullet (2008a, p. 39).

[10] Blokhintsev (1952) and Terletsky (1952).

they are not enough to document the personal and intellectual environment at the time of his move.[11] Einstein's conversation with Bohm after the publication of *Quantum Theory* was the start of a relationship which would last until Einstein's death in 1955. Ironically, as it may seem, Einstein would support Bohm on several grounds except in defense of Bohm's approach to quantum mechanics. We will see some of their exchanges throughout this chapter.

2.3.1 Trapped in the Cold War Storm

During the 1950s David Bohm would fight his most important intellectual battle while pushing for the causal interpretation of quantum theory. That battle happened in extreme personal circumstances as he was trapped in a Cold War storm that made his story almost Kafkaesque. Back in 1949 he had been subpoenaed to appear before the HUAC (House Committee on Un-American Activities) where he was asked about his connections with the Communist Party. Bohm took the Fifth Amendment of the US Constitution (the right to refuse to answer a question because the response could be self-incriminating). In the anti-communist hysteria typical of Cold War times in the US, a period later called McCarthyism, he was indicted for contempt of Congress, arrested and then released on bail. In the following months the court would find him not guilty. Like Kafka's character in *The Trial*, Bohm never knew exactly what he was accused of. As the historian David Kaiser remarked, being a theoretical physicist with leftist inclinations in Cold War America was enough to mark anybody as a highly probable target of anticommunist hysteria. For the American laymen, the atomic bomb could be reduced to a single equation which could be passed on to the USSR. This would mean that the enemy would immediately possess the same weapons America had developed during World War II.[12] Meanwhile, Princeton University suspended his contract,

[11] The David Bohm Papers, deposited at Birkbeck College, University of London, reveal few documents from the period prior to his departure to Brazil at the end of 1951, when the papers on the causal interpretation had already been submitted for publication. After leaving the U.S., there is a meaningful correspondence with Einstein; Melba Philips, an American physicist and friend of Bohm; Hanna Loewy and Miriam Yevick, his friends. Most of the correspondence with Wolfgang Pauli, relevant for the period prior to his departure from the U.S. and after the completion of his paper in the causal interpretation, was recovered and published by Karl von Meyenn in the collection dedicated to Pauli's correspondence (Pauli and Meyenn 1996, 1999). More recently, a batch of letters between Bohm and the French astrophysicist Evry Schatzman was unearthed by Virgile Besson at Schatzman's papers, Observatoire de Paris. These letters corroborate the main points of our work. Furthermore, they weaken the possibility of Bohm's reading of Soviet papers while moving to build the causal interpretation. Indeed, he did not mention this in his letters to Schatzman while describing his work to obtain this interpretation.

[12] "The early years of the Cold War were not a pleasant time to be an intellectual in the United States, especially if he or she happened to have a past or present interest in the political left. [. . .]

prevented him from attending classes and using the university libraries, and in June 1951 did not renew his contract.[13]

Picture 2.2 David Bohm reading a newspaper; after refusing to testify whether or not he was a member of the Communist Party before the House Un-American Activities Committee. Library of Congress, New York World—Telegram and Sun Collection, courtesy AIP Emilio Segre Visual Archives

theoretical physicists emerged as the most consistently named whipping-boys of McCarthyism" (Kaiser 2005, p. 28).

[13] Historians have already set the record of most of this history. The cases of persecution towards Bohm and his colleagues at Berkeley, Bernard Peters, Joseph Weinberg, and Giovanni Rossi Lomanitz have been well charted by Shawn Mullet (2008a); Princeton's attitudes towards him were analyzed by Russell Olwell (1999); the anti-communist hysteria in American academia was studied by Ellen Schrecker (1986), Jessica Wang (1999), and David Kaiser (2005). Bohm's imprisonment and bail is also in Kojevnikov (2002, p. 181).

The intersection of Bohm's political persecution and his move towards a new interpretation of quantum theory has attracted the attention of historians. Christian Forstner has suggested that isolation from Princeton and the American community of physicists was influential in Bohm's abandoning the standard interpretation of quantum physics and adopting a heterodox interpretation. Unfortunately, as we have previously discussed, historians have to deal with the scant documentary evidence around the circumstances of these events in the crucial months in Bohm's life between his appearance at the HUAC in 1949, the publication of his book *Quantum Theory*, and the completion of his causal interpretation in the middle of 1951. As such, we may only deal with plausible conjectures in mapping the influences and motivations of his move toward the causal interpretation.[14]

2.3.2 Bohm, de Broglie, and Pauli: Conceptual Issues and Disputes About Priorities

Bohm was unaware of previous work by Louis de Broglie along analogous lines. What we may reconstruct about how Bohm reacted when informed of de Broglie's works sheds light on the kind of technical problems he needed to solve in order to make his proposal consistent. It is also illuminating regarding the disputes and alliances in the controversy over the foundations of quantum physics. Last but not least, as Wolfgang Pauli was one of the people to warn Bohm about de Broglie's works and as their exchange is one of the most relevant for the early debate on the causal interpretation, it is interesting to see their discussion in some detail. Before Bohm's papers appeared in print, Einstein and Pauli informed him that de Broglie had suggested a similar approach at the 1927 Solvay conference, which Bohm had not known about. Pauli had criticized de Broglie's approach when first proposed and de Broglie had reacted by giving up his idea. 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 stationary states to a rotator, before and after the scattering. Pauli had considered this failure intrinsic to de Broglie's picture of particles with definite trajectories in space-time, an approach de Broglie had called the "pilot wave", which means particles with well defined paths ruled by waves coming from the Schrödinger equation.[15]

[14] On the influences on Bohm's shift towards the causal interpretation, see Jammer (1974, 1988) and Forstner (2008).

[15] Einstein's remark is in Paty (1993). Bohm to Pauli, [Jul 1951], in Pauli and Meyenn (1996, pp. 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 (Institut International de Physique Solvay 1928, pp. 105–141 and 280–282). See also Bacciagaluppi and Valentini (2009).

Pauli addressed his criticisms toward a draft version, which 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 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." Initially Pauli did not read the second manuscript as he considered it too long, which angered Bohm. He rebuked Pauli: "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[16]:

> 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 analyzed Bohm's papers as well as the letters. Pauli conceded that Bohm's model was logically consistent, which was recognition of Bohm's work: "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, related to Bohm's promise of applying his approach to new domains such as high energy physics: "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 and would formulate new objections, as we will see later. For Bohm, however, Pauli's challenge now was less pressing than de Broglie's.[17]

Before 1927, Louis de Broglie had had the idea of a "double solution," in which the waves of Schrödinger's equation pilot the particles, which are singularities of the waves. Just before the meeting of the Solvay council on October 24–29, 1927 he gave up this idea because of its mathematical difficulties and presented his report to

---

[16] Bohm to Pauli, July 1951, Summer 1951, Oct 1951, 20 Nov 1951 (Pauli and Meyenn 1996, pp. 343–346, 389–394, and 429–462).

[17] Pauli to Bohm, 3 Dec 1951, plus an appendix, (Pauli and Meyenn 1996, pp. 436–441).

the meeting with just the "pilot wave" proposal. 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 their logical conclusion, but de Broglie surely had a share in the idea of hidden variables in quantum mechanics. Bohm resisted accepting this. He suggested the following interesting analogy which expresses the silent dispute about priorities between the two physicists, the young American and the elder Frenchman: "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."[18]

In the end Bohm adopted a diplomatic way, suggested by Pauli, to recognize de Broglie's contribution 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 had returned to his old causal approach reviving the idea of "double solution" with his assistant Jean-Pierre Vigier. They would become the most important of Bohm's allies in the hidden-variable campaign.[19]

2.3.3 Exile in Brazil

Let us now return to the outcomes of the troubles Bohm was facing in the era of McCarthyism. After Princeton refused to renew his contract, Bohm realized that it was highly unlikely that he would get another job in American academia as the witch-hunt was growing in America. He therefore looked for opportunities abroad and left the US exiling himself for the rest of his life. He left the US for Brazil, Brazil for Israel, and Israel for the UK, where he finally settled. It was after unsuccessfully attempting to work in the UK, in particular Manchester, that Bohm considered the possibility of exile in Brazil. A small group of Brazilian physicists had graduated from Princeton, among them Jayme Tiomno, who had graduated under John Wheeler and Eugene Wigner in 1950; José Leite Lopes, who had studied under Wolfgang Pauli and Josef Jauch in 1946 and was named a Guggenheim Fellow in 1949; and Walter Schutzer who had completed a Master's degree in 1949. Bohm was one of the readers of Tiomno's doctoral dissertation and

[18] For the evolution of de Broglie's ideas, see Broglie (1956, pp. 115–143). Bohm to Pauli, Oct 1951, *op. cit*.

[19] Bohm to Pauli, 20 Nov 1951, *op. cit*. (Bohm 1952b, pp. 191–193).

served as the chairman of his dissertation committee when Wigner was away. Tiomno invited Bohm to the University of São Paulo. The appointment had the recommendation of Einstein and Oppenheimer and the support at the University of São Paulo of Abrahão de Moraes, then the head of the Physics Department, and Aroldo de Azevedo, an influential geographer. Later, to keep Bohm in his Brazilian position, de Moraes asked Einstein to send letters for an eventual promotion addressed to the highest administrative levels, including President Getúlio Vargas. Bohm arrived in Brazil on October 10, 1951 and would leave for Israel in early 1955.[20]

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 dissatisfaction: "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." "Brazil is an extremely backward and primitive country." One month after his arrival American officials confiscated his passport and told him that he could only retrieve it to return to his native country. This profoundly changed Bohm's fate and morale. He wrote to 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." 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." Bohm's mood oscillated also depending on the reception of his ideas and the work he had done on them. In addition, his 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." Once he wrote, "I gave two talks on the subject here, and aroused considerable enthusiasm among people like Tiomno, Schutzer, 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 then, "I am becoming discouraged also because I lack contact with other people, and feel that there is a general lack of interest in new ideas among physicists throughout the world."[21]

---

[20] Albert Einstein to Patrick Blackett, 17 Apr 1951, Albert Einstein Archives. Jayme Tiomno, interviewed by the author, 4 Aug 2003. Record number 816/51 [microfilm], Archives of the Faculdade de Filosofia, Ciências e Letras, USP. Abrahão de Moraes did not need to use the letter to President Vargas, it is published in *Estudos avançados*, [São Paulo] 21 (1994).

[21] David Bohm to Albert Einstein, Nov 1951, BP (C.10–11). David Bohm to Hanna Loewy, 6 Oct 1953, BP (C.39). David Bohm to Albert Einstein, Dec 1951, BP (C.10–11). David Bohm to Albert Einstein, 3 Feb 1954. Albert Einstein Archives. David Bohm to Melba Phillips, n.d., BP (C.46–C.48). 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).

As we will see, in Brazil 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, profited from collaboration with Brazilian physicists, and published results on the causal interpretation. Thus Bohm's activities in Brazil did not reflect the pessimistic views he expressed in some of the letters he wrote at the time. They tell us more about his personality and the context. That context was conditioned by political insecurity and by the adverse reception of his proposal among his fellow physicists, a subject we discuss in the following section. Bohm would have faced many of the obstacles that he faced in Brazil elsewhere in working on a causal interpretation. Furthermore, Bohm's double identity as a Marxist and a Jew was not a liability in Brazil; on the contrary, it probably garnered him support. Brazil had been a *terre d'accueil* for Jews since the beginning of the twentieth century and following the participation of the country in World War II with the Allies, the dictatorship called *Estado Novo* (1937–1939) ceded room to a democratic regime. While political liberties were limited from 1945 to 1964, they were enough for Communists to continue to play a role in Brazilian life. Examples are 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.[22]

Moreover, Bohm arrived in Brazil at a propitious time for Brazilian physics. Cesare Lattes had participated in the discovery of cosmic-ray pions in 1947 in the UK, and in 1948 in the detection of artificially produced pions at Berkeley. These achievements resonated in Brazil, especially after the role of science in the war and the production of the first atomic bomb. An alliance among scientists, the military, businessmen, and politicians was developed so as to strengthen physics in Brazil. This led to the creation of the Centro Brasileiro de Pesquisas Físicas [CBPF] in Rio de Janeiro and, in the same year that Bohm arrived in Brazil, to the creation of the first federal agency exclusively dedicated to funding scientific research, CNPq. Bohm received several grants from CNPq to develop the causal interpretation. Visits to Brazil by Ralph Schiller and Mario Bunge, both his invitees, and visits by Jean-Pierre Vigier and Léon Rosenfeld were afforded by this agency. Most of the money Bohm received went to research on cosmic rays, a field under Bohm's responsibility at USP. Nevertheless, the board of CNPq explicitly supported the development of the causal interpretation. An indication of the interest of CNPq in the research appears in the report of Joquim Costa Ribeiro, physicist and the Scientific Director of the agency, on Bohm's application for funds for Vigier[23]:

---

[22] For more details on Bohm's stay in Brazil, see (Freire Jr. 2005, pp. 4–7 and 10–19). On Jews in Brazil, see Rattner (1977); on Brazilian communist intellectuals, see Rodrigues (1996, p. 412). During the 1930s, however there were some obstacles to Jews in Brazil, see Saidel and Plonski (1994).

[23] On Lattes's cosmic ray work, see Vieira and Videira (2014). On Brazilian physics in the early 1950s, see Andrade (1999), Brownell (1952). Costa Ribeiro's report is in Arquivos do CNPq (Records of the Conselho Diretor, 139th meeting, 25 Feb 1953), Museu de Astronomia, Rio de Janeiro.

> 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 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.

2.4 Critics and Supporters of the Causal Interpretation

Bohm's approach to quantum mechanics did not pass unnoticed, as revealed by research into archives containing correspondence and papers from the early 1950s. As a matter of fact, most of the physicists who reacted to the causal interpretation were downright hostile to it, while a few of them became strong supporters, and a number of others had mixed reactions. Let us try to chart the initial reception of the causal interpretation, as it is illuminating of the dominant climate at that time towards research on the foundations of quantum physics. Wolfgang Pauli and Léon Rosenfeld were the first to react, Pauli even while the papers were in draft, as we have seen. 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." Let us first examine Pauli's reaction.[24]

After Bohm's papers appeared in print, Pauli advanced new criticisms, which Bohm knew of before their publication. Bohm was astonished: "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 possibility of changing basis in the vector space through unitary transformations. He had also complained that Bohm's approach had borrowed the meaning of Ψ from quantum theory. In a letter to Markus Fierz, Pauli raised the stakes on the philosophical grounds. He observed that Catholics and Communists depended on determinism to buttress their eschatological faiths, the former in the heaven to come, the latter in paradise on earth. These references were implicitly directed to Louis de Broglie on the one hand, and to Bohm and Vigier on the other. Pauli also

---

[24] Léon Rosenfeld to Wolfgang Pauli, 20 Mar 1952, in Pauli and Meyenn (1996).

warned his old friend Giuseppe Occhialini about "Bohm in São Paulo and his 'causal' quantum theory." Occhialini had worked in Brazil at USP during the 1930s and continued scientific collaboration there after the war. Pauli's substantive and persistent attack on Bohm's approach was based on two issues: 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." And yet, "[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." Apparently, this criticism of Pauli echoed well among physicists. "Incidentally, Pauli has come up with an idea (in the presentation volume for de Broglie's 50th birthday) which slays Bohm not only philosophically but physically as well," wrote Max Born to Einstein.[25]

Among the physicists who supported the complementarity view Rosenfeld played a singular role as a vocal and harsh critic of the causal interpretation. This role should be framed, however, by considering the following issues. While he had been Niels Bohr's closest assistant for epistemological matters, as an adept of Marxism he saw the battle against the causal interpretation as part of the defense of what he considered to be the right relationship between Marxism and science. Indeed Rosenfeld was sensitive to criticisms against complementarity coming from the Marxist camp even before the appearance of the causal interpretation. It was the beginning of what the historian of science Loren Graham called "the age of the banishment of complementarity" in the USSR, as part of the *Zhdanovshchina*, "the most intense ideological campaign in the history of Soviet scholarship." As early as 1949, following criticisms appearing in the USSR, he wrote to Bohr, "[I am] just writing an article on 'Komplementaritet og modern Rationalisme' in order to clear up the various misunderstandings which arise when one tries to mix complementarity in all possible sorts of mysticism, whatever it is a question of idealism as with Eddington and others, or about the Russian Pseudo-Marxism. These many 'ismes' are surely too tedious to [you] but [I] feel that one cannot any longer content oneself by ignoring that nonsense."[26]

Rosenfeld went so far as to deny the very existence of a controversy on the interpretation of quantum physics, writing to Bohm, "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."

[25] Bohm to Beck [w/d], Guido Beck Papers, Centro Brasileiro de Pesquisas Físicas, Rio de Janeiro. Beck had reported to Bohm 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 (1953). Pauli to Markus Fierz, 6 Jan 1952, in Pauli and Meyenn (1996, pp. 499–502); Pauli to Giuseppe Occhialini, [1951–1952]. Archivio Occhialini 5.1.14, Università degli studi, Milan. Max Born to Einstein, 26 Nov 1953, in (Einstein et al. 1971).

[26] For Rosenfeld's biography, see Jacobsen (2012). On the debates on the quantum theory in the former USSR, see Graham (1987, p. 325 and 328). Rosenfeld to Bohr, 31 May 1949, Bohr Scient. Corr, AHQP.

For Rosenfeld, complementarity was both a direct result of experience and an essential part of quantum theory. Since complementarity implied the abandonment of determinism, as it precludes the simultaneous definition of position and momentum, which is the basis of mechanical determinism, Rosenfeld saw the causal interpretation as a metaphysical regression, writing, "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 [metaphysician], 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.'" 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 into physics. Rosenfeld's brand of Marxism was Western Marxism rather than the Soviet variety, to use the terms used by Perry Anderson. Thus Rosenfeld was orthodox in quantum mechanics and heterodox in Marxism.[27]

Rosenfeld mobilized colleagues wherever he could to take up the fight against the causal interpretation. He appealed to his professional connections as well as companions sharing ideological ties with Marxism. He pushed Frédéric Joliot-Curie—a Nobel prize winner and member of the French Communist Party—to oppose French Marxist critics of complementarity[28]; advised Pauline Yates—Secretary of the "Society for cultural relations between the peoples of the British Commonwealth and the USSR"—to withdraw her translation of a paper by Yakov Ilich Frenkel critical of complementarity from *Nature*; asked *Nature* not to publish a paper by Bohm entitled "A causal and continuous interpretation of the quantum

[27] Léon Rosenfeld to David Bohm, 30 May 1952, Léon Rosenfeld Papers, Niels Bohr Archive, Copenhagen (hereafter RP). In the French version of the paper, Rosenfeld (1953) emphasized the idea of complementarity resulting from experience, but in the English version, reacting to criticisms from Max Born, he attenuated his stand, changing "La relation de complémentarité comme donné de l'expérience" to "Complementarity and experience." On Born's criticism, see Freire Jr. and Lehner (2010). "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 1953). For Western Marxism, see Anderson (1976).

[28] "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 préoccupations 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 Pinault (2000, p. 508).

theory;" and advised publishers not to translate one of de Broglie's books dedicated to the causal interpretation into English.[29]

Rosenfeld's correspondence shows that his campaign had wide support, as testified by Denis Gabor, "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 so young." Positive letters came from Abraham Pais, Robert Cohen, Vladimir Fock, Jean-Louis Destouches, Robert Havemann, and Adolf Grünbaum. Pais, who had been a student of Rosenfeld in Utrecht, wrote, "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." Cohen, a young Marxist physicist, wrote, "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." Fock, who was the most influential and vocal supporter of complementarity in the USSR, wrote complaining that "Bohm-Vigier illness" was so widespread. Havemann, a German Communist physical chemist, sent him a paper on quantum complementarity, and Rosenfeld replied, "I read with great interest your paper and I am glad to see that our ideas are, in their essential aspects, in agreement."[30]

Guido Beck and Eric Burhop took issue with Rosenfeld's rhetoric, however, and Lancelot L. Whyte challenged him publicly over his review of Bohm's later book *Causality and chance in modern physics*. Guido Beck, one-time assistant to Heisenberg who had fled to Brazil from the Nazis, did not share a belief in the causal interpretation, but defended Bohm against Léon Rosenfeld's criticisms and insisted Bohm should be encouraged to show what his approach could achieve. Rosenfeld was sensitive to Beck's remarks. In the English translation of the original French

[29] Pauline Yates to Léon Rosenfeld, 7 Feb 1952, 19 Feb 1952, RP. Rosenfeld succeeded, "the editors stopped work on this article." The paper had been submitted to *Nature* by Harrie S.W. Massey [with whom Bohm had worked in the Manhattan Project at Berkeley]. *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. Léon Rosenfeld, "Report on L. de Broglie, La théorie de la mesure en mécanique ondulatoire." n.d. RP. The book Rosenfeld advised against translating was Broglie (1957).

[30] Denis Gabor to Léon Rosenfeld, 7 Jan 1953; Abraham Pais to Léon Rosenfeld, 15 May [1952]; Robert Cohen to Léon Rosenfeld, 31 Jul 1953; Vladmir Fock to Léon Rosenfeld, 7 Apr 1956; all papers at *RP*. For Fock's criticism of Bohm's views, see Fock (1957). Jean-Louis Destouches to Léon Rosenfeld, 19 Dec 1951; Léon Rosenfeld to Robert Haveman, 7 Oct 1957; Haveman to Rosenfeld, 13 Sep 1957; 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. All letters are at RP. On Havemann, see Hoffmann (1999).

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 et cartographié au vingt-millième." Burhop, who was at that time organizing a meeting among Rosenfeld and Marxist or left-wing physicists, such as John Bernal, Maurice Levy, Maurice Cornforth, and Cecil Powell to discuss Rosenfeld's article, also wrote: "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."[31]

Rosenfeld went 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 complementarity. Bohm met him and reported on their exchange 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." Bohm and Rosenfeld would meet each other again at the conference held in Bristol in 1957 and dedicated to foundational issues in quantum mechanics.[32]

Werner Heisenberg criticized Bohm's approach as "ideological" while Max Born initially was not impressed.[33] It was Rosenfeld who brought to his attention this interpretation, which led Born to criticize it. "I have already written my Guthrie Lecture in rough draft and have done there just what you suggest, namely, I have included the other party who prefer particles, like Bohm and the Russians which you quote (I cannot read Russian and I take it from your article.)" The common front against the causal interpretation hid disagreements, usually in private, over tactics. Rosenfeld publicly criticized Heisenberg 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

---

[31] 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 (1953). Eric Burhop to Léon Rosenfeld, 5 May 1952, RP. 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 Lancelot L. Whyte Papers, Department of Special Collections, Boston University. The disputed papers were Rosenfeld (1958) and Whyte (1958).

[32] Bohm to Aage Bohr, 13 Oct 1953, ABP; Rosenfeld (1954) and Rosenfeld (2005). For the Bristol conference's proceedings, see Körner (1957).

[33] Heisenberg's criticism was published in the widely read and translated *Physics and Philosophy* (Heisenberg 1958). However, Heisenberg did not pursue the combat. In the late 1950s, "[he] had written more than enough on the subject and had, he said, 'nothing new to say'" (Carson 2010, p. 92).

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.[34]

While Rosenfeld, Pauli, and Heisenberg were the most active critics among the old guard who had created quantum physics (some mixed reactions will be analyzed later), among the younger generation criticism also predominated, but sometimes using different arguments. Bohm presented his approach at an international meeting held in Brazil and met open opposition to his ideas. As he wrote to a colleague in the US:

> 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.[35]

Isidor Isaac Rabi was an American physicist (born in Galicia) from Columbia University who had won the 1944 Physics Nobel Prize for his work using resonance for recording magnetic properties of nuclei. 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."[36] Rabi's own statement of his criticism was,

> 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.[37]

Bohm answered making a comparison with the debates on atomism in the nineteenth century, an analogy he had already used in his papers: "[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

---

[34] Born to Rosenfeld, 28 Jan 1953, RP (Rosenfeld 1960, 1970; Freire Jr. and Lehner 2010). Pauli to Heisenberg, 13 May 1954; Pauli to Rosenfeld, 28 Sep 1954, in Pauli and Meyenn (1999, pp. 620–621 and 769).

[35] David Bohm to Miriam Yevick, [received 20 Aug 1952]; Bohm to Melba Phillips, n.d., BP. I merged the two letters in my narrative.

[36] Ibid.

[37] New research techniques in physics (1954, pp. 187–198).

quantum feature of indiscernibility of particles, i.e., the exclusion principle; Medina asked if Bohm's approach could "predict the existence of a spin 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 was 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."[38]

Among other criticisms of the causal interpretation, it is interesting to note the case of Mario Schönberg as it illustrates the complexity of the quantum controversy for the case when physicists shared the same background and Marxist ideological beliefs.[39] Bohm and Schönberg were both Jews and Communists but they failed to agree on one issue, the interpretation of quantum physics. Schönberg, a theoretical physicist, was working on the mathematical foundations of quantum theory and on the hydrodynamic model of quantum mechanics, a model close to that developed by Bohm and Vigier, as we will see later, but he opposed seeking a causal description in atomic phenomena. However, Schönberg exploited the physical implications of the quantum potential through hydrodynamic models. For instance, in Schönberg (1954), he showed that "the trajectories of the de Broglie-Bohm theory appear as trajectories of the mean motion of the turbulent medium." Despite their deep divergences, one of Schönberg's remarks was taken seriously by Bohm. Indeed, it was Schönberg who "first pointed Bohm in the direction of the philosopher G.W.F. Hegel, saying that Lenin had suggested that all good Communists read the German philosopher." This was an influence which would appear in Bohm's *Causality and Chance*, published in 1957. Unfortunately, Schönberg did not publish his views at the time, but from Bohm's reaction to them one can infer how close to Rosenfeld he was on the subject at stake[40]:

> 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

[38] All quotations are from New research techniques in physics (1954, ibid.).

[39] Other criticisms include Takabayasi (1952), Takabayasi (1953), Halpern (1952), Keller (1953), and Epstein (1953a, b).

[40] For discussions between Bohm and Schönberg, see Peat (1997, pp. 155–157). David Bohm to Miriam Yevick, 24 Oct 1953, BP. For Schönberg's work on quantum mechanics and geometry, see Schönberg (1959). Schönberg's scientific works are collected and reprinted in Schönberg and Hamburger (2009, 2013).

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.

2.4.1 Supporters

If the critics set the tone in the reception of Bohm's proposal, supporters were no less active, and included attempts to further develop the initial papers. The most important adherents came from France with Louis de Broglie, who reconverted to his early ideas of a deterministic description of quantum systems, and Jean-Pierre Vigier, his young assistant. The importance of de Broglie's support may be inferred from the fact that Rosenfeld and Pauli chose to criticize Bohm's approach in their contributions to the de Broglie *Festschrift*, while the French Nobel Prize laureate was cogitating about the implications of Bohm's papers. Eventually de Broglie abandoned the complementarity view in the quest for a causal interpretation of quantum physics.

The influence of de Broglie's reconversion to his earlier ideas can be seen in terms of the weight Rosenfeld attributed to it in a later letter to Niels Bohr: "This comedy of errors [the attempt to develop a 'theory of measurement' based on the 'causal interpretation' of quantum mechanics] would have passed unnoticed, as the minor incident in the course of scientific progress which it actually is, if it had not found powerful support in the person of L. de Broglie, who is now backing it with all his authority." In fact, de Broglie did not directly support Bohm's proposal, instead he pleaded for what he called the "double solution," which would remain as a mathematical suggestion and not a physical model for a causal interpretation. From 1953, through Vigier's visit to Bohm in Brazil, when their collaboration was already underway, de Broglie reminded Bohm of their differences: "You know our viewpoints are not entirely the same because I do not believe in the physical existence of the Ψ wave, which seems only to be the representation—rather subjective—of probabilities. By the way, when we have more than just one particle the Ψ wave must be represented in the configuration space with more than three dimensions and its non physical character appears then absolutely evident."[41]

Vigier brought momentum to the causal interpretation. He was influential among the French communists and in the Cold War times of the early 1950s he mobilized young Marxist physicists to work on the causal interpretation. With de Broglie and Vigier, the Institut Henri Poincaré became the world headquarters of the causal interpretation. A testimony from Jean-Louis Destouches reveals the isolation of complementarity in the French milieu: "The young people received with enthusiasm Bohm's work, which corresponds to the philosophical trends supporting their positions: Thomistic realism, Marxist determinism, Cartesian rationalism. I am

[41] Rosenfeld to Bohr, 21 Oct 1957, BSC, reel 31, AHQP, reel 31, cited in (Osnaghi et al. 2009, p. 101). Louis de Broglie to Bohm, 29 March 1953, Louis de Broglie Papers, Box 7, Archives de l'Académie des sciences, Paris.

almost the only one here to support Bohr's quantum interpretation."[42] Bohm also gathered support from the US, Argentina, and Brazil, through Hans Freistadt, Ralph Schiller, Mario Bunge, and Jayme Tiomno.[43]

Bohm considered the papers he wrote with Tiomno and Schiller and with Vigier to be the main achievements of the causal program in the early 1950s. With Vigier, Bohm answered Pauli's objection that he had included an arbitrary element in the causal interpretation, by using a ψ function that satisfied Schrödinger's equation. Bohm had tried to solve the issue by himself without success, while De Broglie and Vigier were cognizant of the problem in 1952. In 1954, Bohm and Vigier were able to prove that under certain general conditions any function could become a solution of the Schrödinger equation. To achieve this, 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. Thus, the "molecular chaos", an idea Bohm had abandoned after his discussions with Pauli, came back into his work with Vigier.[44]

Jayme Tiomno had met Bohm at Princeton while he was doing his PhD under John Wheeler on weak interactions. Ralph Schiller had worked on gravitation in his PhD under the supervision of Peter Bergmann at Syracuse University and had gone to Brazil to be Bohm's research assistant. With Tiomno and Schiller, Bohm enlarged the scope of his model to include spin, although via analogy with Pauli's equation and not through a relativistic treatment of electrons. Tiomno, however, was not an adherent of the causal interpretation. He worked with Bohm looking for the consequences of extending Bohm's model to other fields of physics, but did not share its philosophical assumptions concerning causality. The Argentinian Mario Bunge, who had been supervised by Guido Beck at La Plata University, spent a year working with Bohm in Brazil, 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. Bunge had studied physics in order to develop a better philosophy of the subject, later developing a successful career in

---

[42] "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." Jean-Louis Destouches to Léon Rosenfeld, 19 Dec 1951, RP.

[43] Freistadt worked both on the philosophical and technical aspects of the causal interpretation; on his activities on this subject in the context of American physics, see (Kaiser 2012, pp. 20–22). For Freistadt's works, see Freistadt (1953, 1955, 1957). Schiller, Bunge, and Tiomno worked with Bohm in Brazil and their cases are discussed in this chapter.

[44] For the role Bohm attributed to those papers, see Bohm (1981, pp. 114 and 118, notes 11 and 12), Bohm and Hiley (1993, p. 205), Pauli (1953) and Bohm (1953a); a simplified and shortened version of this paper was presented in New research techniques in physics (1954, pp. 187–198). "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 Jan 1992 (Bohm and Vigier 1954, 1958; Broglie et al. 1963). A lacuna in the history of physics in the twentieth century—an analysis of the activities of the de Broglie-Vigier group—is now being filled by the works of Vals (2012) and Besson (2011).

the philosophy of science in Canada. In the mid-1960s, disenchanted with the hidden variable interpretation, he gave up on it, accepted indeterminism as part of physics theories, and focused his criticisms of quantum mechanics on the role played by observation in the complementarity view.[45]

The collaboration between Bohm and Vigier was aided by an irony typical of the Cold War. Had Bohm remained in the U.S., Vigier might not have been able to visit and work with him. Vigier had made a name for himself in the Communist Party in France and, 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, had an especially hard time. As much as 70–80 % of visa requests from French scientists were unduly delayed or refused." However, supporters who just applauded the causal interpretation on ideological grounds without trying to develop it did not help Bohm much; apparently, this was the case of French astrophysicist, and Marxist, Évry Schatzman.[46] After all, the causal interpretation needed to win the technical challenges promised by Bohm himself.

2.4.2 Mixed Reactions

Not all reactions were clear-cut criticisms or support. The contributions of two people—Einstein and Feynman—were especially meaningful for Bohm. Einstein, the iconic critic of complementarity, had influenced Bohm while at Princeton to see quantum theory as an incomplete theory. On political grounds, Einstein was an enduring supporter of Bohm against McCarthyism. When the causal interpretation came out, however, he did not support it. "Have you noticed that Bohm believes (as de Broglie did, by the way, 25 years ago) that he is able to interpret the quantum theory in deterministic terms? That way seems too cheap to me," was his comment in a letter to Max Born. Moreover, he wrote a paper to a *Festschrift* for Max Born saying that Bohm's model led to the unacceptable consequence that particles in stationary states, such as an electron in a hydrogen atom, were at rest. Einstein may have used the opportunity to distance himself from the widespread opinion that he was stubbornly attached to determinism. "For the presentation volume to be dedicated to you, I have written a little nursery song about physics, which has startled Bohm and de Broglie a little. It is meant to demonstrate the indispensability of your statistical interpretation of quantum mechanics, which Schrödinger, too, has recently tried to avoid. [...] This may well have been so contrived by that same 'non-dice-playing God' who has caused so much bitter resentment against me, not

[45] Bohm et al. (1955) and Bohm and Schiller (1955). On Tiomno, see Freire Jr. (1999, p. 95). Mario Bunge to the author, 1 Nov 1996, and 12 Feb 1997.

[46] Wang (1999, p. 279) and Schatzman (1953).

only amongst the quantum theoreticians but also among the faithful of the Church of the Atheists." Einstein, however, was kind enough to let Bohm read this paper before its publication and accepted Bohm's request to publish his reply in the same volume. Bohm showed that an adequate use of his model, including changes in the system due to measurements, could save it.[47]

Bohm's main hope for an ally among the foreign visitors he met in Brazil was Richard Feynman, who had been his colleague at Berkeley and spent his sabbatical year in 1951 at the Centro Brasileiro de Pesquisas Fisicas (CBPF) in Rio de Janeiro. Bohm liked Feynman's initial reaction: "At the scientific conference in 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:

> 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.

This letter is evidence of how disconnected Bohm was at the time with the main themes of research on the physics agenda as he was criticizing as "dreary" the kind of calculations which were exciting not only Feynman and Hans Bethe, but almost all physicists involved with quantum field theories. Bohm's hopes about Feynman were unfounded as "in his physics Feynman always stayed close to experiments and showed little interest in theories that could not be tested experimentally" (Schweber 2005). 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. Furthermore, it came out in a general paper published in a Brazilian science journal. That could scarcely nourish Bohm's hopes.[48]

2.4.3 *The Old Guard*

From the old guard of quantum theory, let us now look at the cases of Niels Bohr, Erwin Schrödinger, and John von Neumann. Bohm particularly looked for reactions from Bohr and von Neumann, which is no surprise given that their views were the targets of his hidden variable interpretation. Bohm received the first report of Bohr's views through the American theoretical physicist Arthur Wightman, who

---

[47] Einstein to Born, 12 May 1952 and 12 Oct 1953 (Einstein et al. 1971; Einstein 1953; Bohm 1953b). For Einstein's stances, see Paty (1993, 1995).

[48] On Feynman in Brazil, see Lopes (1990) and Mehra (1994, pp. 333–342). David Bohm to Hanna Loewy, [w/d], 4 Dec 1951, BP (C.38) (Feynman 1954). For the role played by Feynman, Bethe, and the renormalization calculations in physics at that time, see Schweber (1994).

was then in Copenhagen. As Bohm wrote to Melba Phillips: "the elder Bohr [Niels] didn't say much to Art[hur] Wightman, but told him he thought it 'very foolish.'" The distinction between the "two Bohrs" was particularly important as Bohm had met the younger, Aage Bohr, in the spring of 1948 while at Princeton,[49] and was pleased to discover that Aage Bohr was more sympathetic to the causal interpretation than his father, Niels Bohr. As Bohm reported to Wightman, "I am glad that Aage Bohr admits its logical consistency."[50] Indeed, the younger Bohr [Aage] was more receptive to Bohm's proposal—"it would be nice to meet some time and discuss things, also the epistemological problems"—while he respected the value of the complementarity view: "there it seems to me that the very fact that one can give a logically consistent non-deterministic description of natural phenomena is a very great lesson which gives one a much freer way of thinking about things." The conversation continued and Bohm explained to Aage Bohr the two assumptions he considered to be "unnecessarily dogmatic" in the principle of complementarity: (1) "that the quantum of energy will remain indivisible and unanalyzable at all levels . . .", and (2) "that the statistical laws of quantum mechanics are final, in the sense that no deeper causal laws will ever be found . . .". [51] As for the elder Bohr, there was never any sign of empathy towards the causal interpretation, even after they had the opportunity of having personal conversations, for Bohm visited Copenhagen twice, in 1957 and 1958. As Bohm recorded 5 years later, Niels Bohr had "expressed especially strong doubts that such a theory [causal interpretation] could treat all significant aspects of the problem of *indivisibility* of the quantum of action" (Bohm 1962, p. 363).

However, the main interest of Aage Bohr in the exchange with Bohm was not related to the epistemological issues in quantum mechanics, but to Bohm and David Pines' work on plasma, metals as electron gas, and collective variables. Aage Bohr was extending the collective variable approach to his own work on nuclear physics. He sent Bohm a preprint of a paper written with Ben Mottelson, and observed, "I would be also very interested in any comments from you on this, admittedly still rather primitive attempt of ours to develop a more comprehensive and self-consistent treatment of a many-body system such as the nucleus. In some ways, there are parallelities, I think, to your treatment of the electron gas, even though the forces and the geometry are quite different." Bohm, who was still in Brazil, was interested in Aage Bohr's work on nuclear physics, comparing it with results from the Van der Graaf accelerator being built in São Paulo. This would produce slow neutrons with very accurately determined energy.[52]

[49] David Bohm to Melba Phillips, n.d., BP (C.46–C.48). Letter from Aage Bohr to the author, 17 Oct 1997.

[50] David Bohm to Arthur Wightman, [1953], Niels Bohr Archive, Copenhagen.

[51] Aage Bohr to David Bohm, 3 Oct 1953; Bohm to Aage Bohr, 13 Oct 1953, emphasis in the originals, Aage Bohr Papers, Niels Bohr Archive, Copenhagen.

[52] Aage Bohr to David Bohm, 3 Oct 1953; Bohm to Aage Bohr, 24 Sep 1953, ibid.

Thus in the late 1950s when Bohm was already in Israel and Pines visited Copenhagen, Bohm wrote to Aage Bohr. "I would very much like to spend [the summer] in Copenhagen and to work with Pines on plasma theory, on which subject both of us have interesting new ideas."[53] Bohm visited Copenhagen between 08 August and 29 September 1957 and then from 07 July 1958 to 13 September 1958. The influence of Pines and Bohm's plasma work on nuclear physics in Copenhagen was acknowledged by Ben Mottelson, the American physicist living in Copenhagen who went on, with Aage Bohr and Leo Rainwater, to win the 1975 Physics Nobel Prize for "the discovery of the connection between collective motion and particle motion in atomic nuclei and the development of the theory of the structure of the atomic nucleus based on this connection." In the Nobel acceptance speech, Mottelson recalled that: "It was a fortunate circumstance for us that David Pines spent a period of several months in Copenhagen in the summer of 1957, during which he introduced us to the exciting new developments in the theory of superconductivity. Through the discussions with him, the relevance of these concepts to the problem of pair correlations in nuclei became apparent."[54]

As for von Neumann, Bohm considered his reaction a little better than Bohr's. Bohm reported that "von Neumann thinks my work correct, and even 'elegant,' but he expects difficulties in extending it to spin." 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." In a recent analysis, the philosopher Michael Stöltzner suggested that "von Neumann could accept Bohm's proposal as an interesting model, but not as a promising interpretation."[55] As for Schrödinger, in spite of criticisms of the complementarity view, his insistence on the wave function ontology of the quantum world and absence of interest in the recovery of determinism hampered the dialogue with those, such as Bohm, Vigier, and de

[53] Bohm to Aage Bohr, 18 Dec 1956, ibid. Aage Bohr replied, "I hope very much you can manage to come here next summer, when we also expect Pines to be here. We should, of course, be very pleased if you would tell us a little about plasma theory." Aage Bohr to Bohm, 26 Jan 1957, ibid. For the next summer, Aage mentioned they wanted to hear Bohm on superconductivity, reflecting the interest arose by the work of Bardeen and colleagues, Aage Bohr to Bohm, 25 Oct 1957, ibid.

[54] Visitors records, Niels Bohr Archive. "The Nobel Prize in Physics 1975", http://www.nobelprize.org/nobel_prizes/physics/laureates/1975/. "Ben R. Mottelson - Nobel Lecture", http://www.nobelprize.org/nobel_prizes/physics/laureates/1975/mottelson-lecture.html, on page 240. Both information accessed on 11 Jan 2014 (Bohr et al. 1958).

[55] J. von Neumann's reaction is in David Bohm to Wolfgang Pauli, [Oct 1951], in Pauli and Meyenn (1996, pp. 389–394). John von Neumann to H. Cirker, [President of Dover Pub], 3 Oct 1949. John von Neumann Papers (Box 27, Folder 8), Library of Congress, Washington, DC (Von Neumann 1955; Stöltzner 1999).

Broglie, who worked with a world populated by particles in a deterministic framework.[56]

2.4.4 Bohm's Proposal and Philosophers of Science

Bohm's causal interpretation also contributed to enticing philosophers of science to enter the quantum debate. Indeed they were never absent, as in the early stages of the debate some philosophers, such as Karl Popper, Hans Reichenbach, Gaston Bachelard, Grete Hermann, and Alexandre Kojève had ventured into this field.[57] Now, with the reheated controversy, there was new fuel for the philosophy of science. However, while in the 1930s philosophers mostly produced works more of an epistemological nature, in the sense of providing a critical analysis of an existent scientific theory, now they divided themselves along the same lines as the physicists. Some were sympathetic towards Bohm's enterprise, as in the case of Paul Feyerabend, who praised Bohm's *Causality and Chance* as containing "an explicit refutation of the idea that complementarity, and complementarity alone, solves all the ontological and conceptual problems of microphysics." Others aligned with Bohr's point of view, notably Norwood Hanson, who maintained that "when an interpretation of a theory has been as successful as this one [Copenhagen interpretation] has been, there is little practical warrant for the 'alternative interpretations' which have, since Bohm, been receiving prominence." And yet, there were cases, such as Bachelard, who retired from the debate as it became heated because de Broglie reconverted to the deterministic description of the quantum phenomena. Since then, the debate on the foundations of quantum physics has been an attractive topic for philosophers of science and deserves further historical research.[58]

[56] Schrödinger (1953). On Schrödinger's philosophical views, see Michel Bitbol's comments in Schrödinger and Bitbol (1992, pp. 140–141) and Bitbol (1996a). In private, Schrödinger kept high his fight against the complementarity view, as in this letter to Max Born, on October, 10, 1960: "The impudence with which you assert time and again that the Copenhagen interpretation is practically universally accepted, assert it without reservations, even before an audience of the laity–who are completely at your mercy–it's at the limit of the estimable . . . Have you no anxiety about the verdict of history?" (Moore 1989, p. 479).

[57] Popper and Bartley (1982), Reichenbach (1944), Bachelard (1934), Hermann et al. (1996), and Kojève and Auffret (1990).

[58] Feyerabend (1960) and Hanson (1959). On Bachelard, see Freire Jr. (2004a). An illustrative example of how attractive this topic may be is Mara Beller's criticism of Kuhn's paradigms (Beller 1999). In her view, the appearance of the notion of paradigm is related to the quantum controversy. I discussed these issues in Freire Jr. (2014c). Popper, who was interested in the foundations of quantum mechanics from the 1930s, only became an active protagonist in the quantum controversy in the early 1980s. See Freire Jr. (2004b) and Popper and Bartley (1982).

2.5 Waning Causality and Disenchantment with Communism (Late 1950s–Early 1960s)

In 1955 David Bohm left Brazil for Israel and in 1957 moved again, this time to the UK, first to Bristol and then to the Birkbeck College in London. Bohm's main motivation for leaving Brazil was the possibility of travelling abroad—Europe—in order to discuss and defend his causal interpretation for quantum mechanics. Life, however, brings unexpected turns and he went on to experience a major intellectual change from 1956 on. Politically he broke his ideological ties with Marxism, in philosophical terms he weakened his beliefs on the centrality of causality for science and society, and in the scientific arena he gave up the causal interpretation discouraged with its developments. All these changes were not unrelated, as we will argue. He began to look for new research directions but they only would coalesce in the late 1960s. The peregrination to Israel was tainted by the worsening of his situation with the US government related to the confiscation of his passport. In order to travel to Israel he applied for Brazilian citizenship, which led to the loss of his American citizenship. Only in the UK, in 1960, would he face the obstacles to get it back. This period of transition did not only bring unpleasant experiences. He made new and lasting acquaintances; got married to Sarah Woolfson, with whom he would spend the rest of his days, in Israel; and met Basil Hiley, who would become his enduring collaborator on the new perspectives of research, in Britain. In addition, he found two graduate students deeply interested in touching upon the foundations of quantum mechanics, which, as we already saw, had become the intellectual pet of David Bohm. They were Yakir Aharonov in Haifa, and Jeffrey Bub in London.

2.5.1 Break with Communism

As one would expect, Bohm was very sensitive to the reactions to his reinterpretation of quantum theory in terms of hidden variables. In particular, he paid attention to the way Marxists, physicists and philosophers, reacted to it, which is no surprise given Bohm's Marxist background. He 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." The connection appeared so obvious to Bohm that he complained when fellow travelers like the American physicist Philip Morrison did not support him. "This type of inconsistency in Phil [Morrison] disturbs me. He should be helping, instead of raising irrelevant obstacles." 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. "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."[59] If it is hard for historians to chart the precise influence on Bohm's shift towards the causal interpretation, there is no doubt that the influence of Marxism was effective in supporting the causal interpretation, especially among the French team led by Vigier, and that such support was influential on Bohm himself, albeit weaker than Bohm had hoped for. The unfulfilled expectations were mainly related to the USSR, as evidenced in a letter he sent in 1955 to the American physicist Melba Phillips[60]:

> At times I feel discouraged about the state of the world. A thing that particularly strikes home to me is the report I got from Burhop (confirmed by others) on Russian physicists. Apparently, they are all busy on doing calculations on electrodynamics according to Feynman, Dyson, et al. Their orientation is determined strongly by the older men, such as Fock and Landau, who in addition to their training, are influenced by the fear of a sort of "Lysenko affair" in physics. The typical physicist appears to be uninterested in philosophical problems. He has not thought much about problems such as the re-interpretation of qu. mchs, but tends to like the word of the "big-shots" that ideas on this such as mine are "mechanistic". Actually, the standard procedure is just to label such a point of view, and then most people accept the label without even bothering to read about such questions. There are some philosophers in Moscow who criticized the usual interpretation, but they haven't had much influence on the physicists. All in all, the situation in Soviet physics doesn't look very different from that in Western physics. 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 work and think.

What Bohm did not realize was that part of the support for complementarity and resistance to the causal interpretation was also based in commitment to Marxism. This was the case of Rosenfeld, as we have already seen, and also of Vladimir Fock, who supported Bohr's views in the USSR basing his position on dialectical materialism. From 1957 on, after Stalin's death and the ideological thaw in the USSR, Fock would become an outspoken defender of Bohr's views. In addition, a number of Soviet physicists, such as Blokhintsev and Terletsky, while being critics of complementarity were not supporters of the causal interpretation either. Indeed, the former became a leader in the defense of the ensemble interpretation, which says quantum theory does not describe states of single systems but only an equally prepared ensemble of them.[61] The latter devoted his energies to attempts to include non-linearities in the standard quantum mechanics, an approach which resonated with de Broglie's proposal of a "double solution." Indeed, we may see in hindsight,

---

[59] David Bohm to Miriam Yevick, 5 Nov 1954, BP. David Bohm to Melba Phillips, n.d. BP. David Bohm to Miriam Yevick, 7 Jan 1952, BP.

[60] Bohm to Melba Phillips, 18 March 1955, BP (C49). Andrew Cross (1991) saw Bohm's work as just 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. For the critique of this position, see (Freire Jr. 1992).

[61] For a description of the ensemble interpretation, see Home and Whitaker (1992).

the relationship between Marxism and the spectrum of stances in the quantum controversy was not one-to-one. Instead, Marxism influenced both critics as well as defenders of complementarity. This multi-sided relationship should be no surprise as when speaking of Marxism in the twentieth century it is better to use the plural Marxisms than the singular Marxism.[62]

At any rate in 1955 Bohm could still think that Soviet and Marxist physicists should support his causal interpretation in a stronger manner. The vicissitudes of the times, however, would make such a matter meaningless for Bohm. By late 1956 or early 1957, a crisis point in his commitment to Marxism was reached, triggered by Khrushchev's report on Stalin's crimes and by the invasion of Hungary by Soviet troops. Bohm's break with Communism, while he was visiting Paris to work with Jean-Pierre Vigier and Louis de Broglie, was witnessed by the physicist Jan Meyer and is well-recorded in two long letters to Melba Phillips. How dramatic Bohm's involvement was with these critical events may be seen from the following fragments[63]:

> It is clear from the above that what is needed in the left-wing movement today is a certain measure of disengagement from Russia. Russia has made an enormous number of errors. ... This raises the question of the probable future of the C.P.'s [Communist Parties] throughout the world. ... As soon as a man opposed the direction of the C.P. he became a traitor guilty of the most heinous crimes. Confessions were manufactured and extorted on a large scale. The truth had nothing to do with the case; what was published was only what would be convenient for the interests of the gov't. This was a direct perversion of the principle that dialectical materialism should be scientific and objective. Perhaps some people said that false confessions served the interests of a "larger truth". Similarly, Humanité [the official newspaper of the French Communist Party] still publishes lies about Hungary; quite cynically since the truth is evident. It is clear also that the Russian gov't publishes whatever it thinks is convenient about world affairs. Perhaps they have already ceased to lie consciously, and they may be only deceiving themselves.

Thus by 1958 Bohm's relation to Marxism came to an end. It had lasted from the late 1930s, when he approached the US Communist Party at Berkeley, in the wake of the Great Depression and the rise of Nazism in Europe, to 1956–1957 following Khrushchev's report and the invasion of Hungary by the USSR. That history had cost him the right to live in his home country and would still cause a lasting battle to recover his American citizenship. He lived the main political passions of his times and was a man trapped in the Cold War storm. And yet, his history, including adhesion to and a later break with Communism, was not exceptional, indeed it was

---

[62] On Marxism and the controversy over the interpretation of quantum theory, see Freire Jr. (2011c). See also Graham (1987, pp. 320–353), on Fock and Blokhintsev; Kuzemsky (2008), on Blokhintsev; Pechenkin (2012), on the early ensemble interpretation in the USSR and in the US; Forstner (2008), on Bohm; Jacobsen (2007, 2012), on Rosenfeld; Kojevnikov (2011), on ensembles; Pechenkin (2013), on Mandelstam; Kojevnikov (2004), on Soviet physics, and Besson (2011), on Vigier.

[63] For an account of those events, see Gaddis (2005, pp. 83–194). Jan Meyer, conversation with Olival Freire, 30 January 1997; Bohm to Phillips, undated, BP (C49). This rupture is also noted by Kojevnikov (2002, p. 191) and Peat (1997, p. 178).

typical of the generation of intellectuals in the mid-twentieth century, around World War II.[64]

2.5.2 Causality Relativized

After Bohm's break with Communism he made few references to Marxist ideas. However, one of them is very meaningful for the philosophy of science as it concerns the role of determinism in society. It appears in a letter to the American artist Charles Biederman, with whom he exchanged a large correspondence, over 4,000 pages between March 1960 and April 1969, now being edited by the Finnish philosopher Paavo Pylkkänen. The reference came in the middle of a discussion about determinism, on which I will comment later: "For they [Marxists] felt that by studying the evolutionary process of the past, they could pick out the main direction in which history was moving. They became so attached to their theories that they were unable to review their own role objectively, or to admit new and unexpected developments not fitting into these theories." How much Marx's historical materialism depends on adopting determinism in history is debatable, however. For the purposes of our analysis, nonetheless, it is enough to consider that Bohm's rupture with Marxism may have destroyed his general belief in determinism as a feature of society and its history.[65]

The connection between the break with Marxism and abandonment of determinism in science, particularly in physics, and not only in society, in Bohm's path is a guess, albeit a plausible one. The best evidence of how and when Bohm shifted his focus away from the philosophical priority for causal laws in physics can also be found in the correspondence with Biederman. The intellectual turn was acutely noted by Pylkkänen, "here we have Bohm, who is internationally known as a defender of a deterministic interpretation of the quantum theory, and thus for many a defender of strict determinism in nature, arguing strongly for the objective existence of properties such as contingency, chance, determinism, etc. Of course, Bohm does this already in *Causality and Chance*, but here the point is made more vividly, given that Bohm is defending the role of indeterminism rather than questioning it, as he most famously did in his 1952 papers."[66]

From this extensive correspondence between Bohm and Biederman, I have selected fragments from a few letters to provide the reader with an idea of the issues at stake. In his very first letter, in 1960, Biederman was clear-cut in his

---

[64] Ory and Sirinelli (2004), Hobsbawm (2011), Chaps. 11 and 14, Caute (1967).

[65] Bohm to Biederman, 2 February 1961, (Bohm et al. 1999, p. 95). As the historian Eric Hobsbawm remarked, at least two features of Marxism should not be abandoned unless one gives up historical materialism as a way to change the world: (a) the triumph of socialism is the logical end of all historical evolution until the present, and (b) socialism marks the end of prehistory as it cannot and will not be an antagonistic society (Hobsbawm 1997, Chap. 11).

[66] Paavo Pylkkänen's statement is in the introduction of Bohm et al. (1999, p. xix).

defense of determinism: "To explain my interest in your book [*Causality and Chance*]. To put it briefly, the notion of indeterminism has always seemed contrary to experience, which, even after reading your very fine book, I cannot accept even as an eventually limiting case." And yet, "I sympathize with your belief that a deeper penetration will reveal a nature of causality. But there is the possibility that this will also dispel the basis for the present 'lawless' view of nature and, rather than make it a limited case, will dispense with it entirely." Bohm's answer to Biederman is that time implies a certain ambiguity. "Thus, there is some ambiguity in past and future. We experience this ambiguity in certain ways directly. For when we try to say 'now,' we find that by the time we have said it, the time that we meant is already past, and no longer 'now.'" He continues, citing an example closer to physics, "and if we try to do it with clocks, so as to be more precise, quantum theory implies that a similar ambiguity would arise because of the quantal structure of matter. In fact, there is no known way to make an unambiguous distinction between past and future." Thus, "it becomes impossible that the past shall completely determine the future, if only because there is no way to say unambiguously what the past really was until we know its future." As Biederman might have compared that letter with the book which was the catalyst of their correspondence, Bohm anticipated this, "as you may perhaps have noticed, my ideas on determinism and indeterminism have developed since I wrote *Causality and Chance*, although what I now think about these questions was, to a considerable extent, implicit in the point of view expressed in the book." His conclusion, in short, is that "neither determinism nor indeterminism (causality or chance) is absolute. Rather, each is just the opposite side of the whole picture," and that "in the question of determinism vs. indeterminism, there is as I have said, a necessary complementary relation of the two ideas."[67]

Bohm's reference to *Causality and Chance* deserves some attention. The philosophical convictions he held while writing this book weakened the prominence he attributed to causal laws in science, as he concluded that causal and probabilistic laws should be accorded the same philosophical status. Also noteworthy is the fact that these philosophical studies were motivated, at least partially, by his ideological commitment to Marxism. For our purposes, however, the most meaningful remark in his letter to Biederman was the comment that "my ideas on determinism and indeterminism have developed since I wrote *Causality and Chance*." This can be seen as a clue to the kind of change Bohm experienced after writing the book and before the first letter from Biederman. The book was finished in 1955 while he was in Brazil, then he left for Israel, visited Paris and Bristol, and eventually settled in London, and the single most relevant change he experienced during this time was his break with Marxism.

[67] Biederman to Bohm, 6 March 1960; Bohm to Biederman, 24 April 1960; both in Bohm et al. (1999, pp. 3–4 and 8–19).

2.5.3 Abandonment of the Causal Interpretation

Throughout the 1950s Bohm worked consistently on the development of the causal interpretation. Two directions of research were particularly prioritized. The first was to develop a relativistic generalization of the initial approach and was considered by him and his supporters to be the main goal of their work. "The day that we defeat the Dirac equation, we are going to have a special victory party, with a case of champagne," he confessed to a correspondent in the mid-1950s. Till today this remains an unreached goal, considered by many to be a shortcoming of the causal interpretation. The second direction was related to Bohm's promises that his approach, conveniently modified, could approach the domain of intra-nuclear particles, which he labeled in 1952 as the domain of 10^{-13} cm distances. Bohm joined a collaboration between the French team led by Vigier and Japanese physicists which included the Nobel Prize winner Hideki Yukawa as its most prominent name. They looked to classify the myriad of recently discovered intra-nuclear particles through representing them as extended bodies in space-time and relating the number of degrees of freedom from these models to their quantum numbers.[68] While this approach was neither a clear-cut extension of the 1952 model of electrons nor based on the requirement of causality, before the quark model, in the late 1950s, this was an exciting adventure in a new physical territory. And yet, if philosophically it was not entirely based on the causal interpretation it was not strange to it. Indeed its philosophical assumptions were realism and the primacy of descriptions in the arena of space-time instead of abstract mathematical spaces.

In the late 1950s however, Bohm's research departed from that of his collaborators like Vigier and de Broglie. While they persevered in their research into the causal interpretation, Bohm gave it up. A number of factors may have played a role in his decision, including discouragement over the limited response to these ideas and, as he would acknowledge later, "because I did not see clearly, at the time, how to proceed further, my interests began to turn in other directions" (Bohm 1987, p. 40).[69] An inspection of the list of Bohm's publications related to the foundations of quantum mechanics suggests the late 1950s and the early 1960s as the time when this abandonment occurred. Indeed, while in the 1950s he wrote an average of 1.6 papers per year on these topics, in the 1960s and 1970s this figure drops to half, reversing in the 1980s to increase to 2.2 papers per year. Closer inspection reveals however, that most of the papers from the 1960s were related to the reaction to external challenges such as the appearance of John Bell's paper, a subject we will deal with in Chap. 7, or new perspectives he was adopting (Freire Jr. 1999, pp. 167–170). As a matter of fact from 1960 on Bohm gradually began to search for a new

[68] Bohm et al. (1960a, b); a review of the state of the art of this research is Broglie et al. (1963). Virgile Besson is studying the French side of the mentioned collaboration while Pablo Ruiz de Olano is studying the Japanese side.

[69] A balance of how far Bohm went with hidden variable theory is provided by Bohm (1962). See particularly pp. 359–363 for his evaluation of the criticisms it suffered.

approach to the interpretation of quantum mechanics. The new approach would take more than 10 years to mature. Indeed, it was only around 1970 that the first papers appeared suggesting "a new mode of description in physics" and taking "quantum theory as an indication of a new order in physics." We return to these new perspectives later.

2.5.4 Citizenship Lost, Dignity Preserved

Let us go back to early 1952 in Brazil, after the American officials confiscated Bohm's passport. Under pressure to travel abroad to discuss his causal interpretation with wider audiences he early began to consider applying for Brazilian citizenship. It would be a difficult choice, as he wrote to Hanna Loewy: "also, if I want, I can apply for citizenship. This would have some advantages: as with it, I could travel. But the disadvantage is that I could not return to the U.S., at least for a long time. For according to the McCarran act they can exclude any non-citizen from the U.S., who, in their opinion, was ever connected with Communism. So it's a tough decision, isn't it?" In the middle of 1954, anxious because of the tension of the political times (a year before the Rosenberg couple, accused of espionage, had been executed in the U.S., and in August 1954 the Brazilian President Getúlio Vargas had committed suicide in the middle of a serious political crisis), and having received a job offer from Israel sent by Nathan Rosen, Bohm decided to apply for Brazilian citizenship.[70] Helped by Brazilian scientists and politicians the whole process was quick. He applied for citizenship on 15 September 1954, received the presidential decree on 22 November 1954, and took the oath on 20 December 1954. In early 1955 he left the country for Israel. Getting Brazilian citizenship, however, was a fateful decision for Bohm as it led to the loss of his American citizenship. Indeed, in accord with the oath, he gave up his former citizenship. As early as April 1955, Marc Severe, an official from the American consulate, required the Brazilian Police Department to give the US government information about the Brazilian

[70] Bohm tried to convince Einstein to support his move to Israel, but Einstein was reluctant, writing, "to go there with the intention to leave on the first occasion would be regrettable." Einstein to Bohm, 22 Jan 1954. Bohm, however, was decided to go: "I have decided to go to Israel. This decision was precipitated by the receipt of an offer of a job in Haifa from Rosen [. . .] I have cited you as a possible recommendation, so you may be receiving a letter from them soon." He also promised to stay there for years to Einstein (". . . do not plan to leave unless after several years of effort"), a promise he would not keep. In addition, Bohm was considering the possibility of getting a passport without losing American citizenship—"I am informed that the Israeli Embassy in Brazil may issue a passport for me to go to Israel, if the Technion request it."—which did not materialize, Bohm to Einstein, 3 Feb 1954. Then Einstein changed his views and supported Bohm's plans. Einstein to Bohm, 10 Feb 1954; and Einstein to Nathan Rosen, 11 March 1954. The Albert Einstein Archives, The Hebrew University of Jerusalem. My thanks to Michel Paty and Amit Hagar for providing me with copies of these letters.

nationality of Bohm. Brazilian authorities took time replying, but eventually they confirmed that Bohm had been granted Brazilian citizenship.[71]

Bohm lost his American citizenship on 5 December 1956, but only in 1960, already in London, did he try to recover it or even to get a visa, so that he could accept the position that Brandeis University had offered him. His attempts were unsuccessful. He tried again in 1965–1967, with the support of Stirling Colgate, President of the New Mexico Institute of Mining and Technology, in Socorro. Colgate became engaged in the fight as a result of the job offer he had made to Bohm. Again he did not succeed. The backgrounds of these attempts is revealing of the enduring constraints of the Cold War era. In 1960 in London, he was asked by the American Consul about his previous relationship with the Communist Party. Thus, Bohm made a notarized statement on 23 March 1960 about his former links with the Communist Party, and about his current withdrawal from Communist views. Although he had made a notarized statement, Bohm did not intend to make it public. However, this was exactly what the American officials expected from him. Indeed, it would be necessary to demonstrate an active attitude against Marxism, i.e. to make public pronouncements against Communism.[72]

At that stage, Bohm faced a dilemma: either to keep his dignity and not recover his American citizenship or recover it, even if it meant losing his dignity. Bohm decided not to pay the price required by the American authorities. His decision is well documented in a letter to Aage Bohr, "It seems that while they are satisfied that I am not a Communist, the McCarran act requires that I prove 'active anti-Communism', e.g. by writing political articles; and this I am not prepared to do." Later, in 1966, Bohm stuck to his decision, as one can see from the letter to Ross Lomanitz, who had been instrumental in recommending him to Colgate, "My principal objection to [publishing something of an 'anticommunist nature'] is that it is not really compatible with dignity. [. . .] I feel it wrong to say it [his criticisms to Communism] in order to regain American citizenship. For then, I am saying something not mainly because I think it is true, but rather, for some ulterior purpose. It's rather like writing a scientific article in order to impress one's superior, so as to get a better job." It is worth noting that Stirling Colgate understood and supported Bohm's attitude, writing to the US State Department, "He could apply for a visa as an immigrant, and I believe this would require a full demonstration of active opposition to communism with a question on his mind, I am sure, of just how

[71] Bohm to Hanna Loewy [Beginning of 1952]. BP (C.40). According to physicist José Leite Lopes [Interview with A.M.R. Andrade, 18 March 2003], Brazilian physicists had asked João Alberto Lins de Barros, a very influential politician and supporter of Brazilian physics, to accelerate Bohm's Brazilian citizenship application. File 40.135/54. Archives of the "Instituto de Identificação Ricardo Gumbleton Daunt", SSP—Polícia Civil, São Paulo.

[72] For the date of the "Certificate of Loss of Nationality", see Stirling Colgate folder in BP (C.8). "I would like very much to get the question of my US citizenship settled again". Bohm to Stirling Colgate, 28 April 1965, BP (C.8). I am thankful to Basil Hiley for his kindness in sending me a copy of the notarized documents.

active is active. This question relates, of course, to a sense of personal dignity among his friends and peers."[73]

Let us now break the chronology to report the outcome of Bohm's citizenship affair. In the twilight of the Cold War, Bohm eventually won the right to recover his American citizenship after living more than 30 years as a Brazilian citizen. He used his letters to Einstein written from Brazil, in which it was clear that Bohm did not intend to give up American nationality, and that he had applied for Brazilian citizenship only in order to get a passport. He succeeded in the legal process in 1986: "Dear Dr. Bohm. I am pleased to inform you that the Department of State has today notified the Embassy that your citizenship case has been reconsidered. It has now been determined that your naturalization which took place in Brazil, in November 1954, was an involuntary act. Consequently your loss of United States citizenship has been overturned; and the Certificate of Loss of Nationality that was initially prepared has been vacated." The victory came too late as he had no income to live in the US as a retiree. In Cold War times, keeping dignity came at a high price.[74]

2.5.5 New Acquaintances: Students and Collaborators

During this period of transition Bohm also had pleasant professional experiences meeting people who would collaborate in the new directions he would undertake. At Technion in Israel he met two new students, Yakir Aharonov and Gideon Carmi.[75] Aharonov analyzed the role of electromagnetic potentials in quantum theory and suggested a new effect, now known as the Aharonov-Bohm effect. Aharonov and Bohm illustrated this effect arguing that when an electron beam is

[73] Bohm to Aage Bohr, November 17, 1960, Aage Bohr Papers, Niels Bohr Archive, Copenhagen. The distinction between declaring not to be Communist and expressing active anti-Communism was not understood by Bohm's biographer David Peat (1997, pp. 254–255). Peat also asked "Why did he place his rejection of Communism at the end of the Second World War when in fact his letters from Brazil are staunchly pro-Communist?" I think Peat was not very sensitive to the carefully diplomatic manner in which Bohm wrote, in the statement previously cited: "Gradually however, and especially after the war was over, I began to see that . . ." He was simply avoiding any great disparity between that statement and what he had declared before the HUAC, in 1949–1950. Bohm to Ross Lomanitz, 21 Nov 1996, BP (C.42), underlined in the original. Stirling Colgate to George Owen (Deputy Director Visa Office—US State Dept), 4 Nov 4, 1966, BP (C.8).

[74] Bohm's lawyer, Edward S. Gudeon, based his petition on the decision of the Supreme Court, in 1967, in the case Afroyim v Rusk, which stated that an American citizen could only lose his citizenship if required by himself. Edward Gudeon to Ehud Benamy, 11 Feb 1986, BP [Probably C.8]. Richard Haegele—American Consul in London—to David Bohm, 11 Feb 1986, BP [Probably C.8]. "I cannot see how I could settle there permanently, because my pension could not be adequate for this". Bohm to Hanna Loewy, 3 March 1986, BP (C.41).

[75] David Bohm, interviewed by Maurice Wilkins, sessions 4 and 7, 25 Sept 1986 and 30 Jan 1987, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD.

split around a region where an electromagnetic field is confined, the beam passing through a field-free region may undergo a physical change. They then argued that this was a quantum effect related to the vector potential, which is classically considered to be without physical meaning. This paper stirred up a flood of experiments and theoretical explanations and is by far the most influential paper authored by David Bohm, amounting to 3,500 citations as of May 2012 (Aharonov and Bohm 1959; Peshkin and Tonomura 1989).[76] It brought wide recognition to both, which included the 1998 Wolf Prize to Aharonov. However, the Aharonov-Bohm effect did not appear at Technion. Aharonov had followed Bohm to Bristol, where he got his PhD. Bristol was then a thrilling center for physics under the leadership of Maurice Pryce, the head of the Department. "Pryce appointed David Bohm (1917–1994), who arrived in 1957 with his student Yakir Aharonov (b. 1932). Their discovery [. . .] was central to the formulation of modern gauge theories of fundamental interactions." These are the recollections of Michael Berry and Brian Pollard (2008).

In London Bohm met Jeffrey Bub, who began to work with him as a graduate student on problems related to the foundations of quantum physics. Bub came from Cape Town, where he had become interested in foundational issues in quantum mechanics through the mathematician and mystic Michael Whiteman (Bub 1997, p. xi). He went to London to study under Karl Popper but at the time Popper was in the US. Bub was advised by G.J. Whitrow to work either with Bohm or Rosenfeld if he wanted to work on foundations of quantum mechanics. Bub chose Bohm because his scholarship funds were insufficient to support a move to Denmark, and he thought the language would present a problem. The research directions Bub would have followed, had he chosen Rosenfeld, we can only wonder. He began to work under Bohm in early 1963, however, Bohm was no longer interested in hidden variables. According to Bub's recollections,[77]

> At the time Bohm was no longer interested in hidden variables. He was trying to develop a general framework for physics based on a discrete space-time structure for events and held a weekly seminar where he discussed ideas on algebraic topology using Hodge's book on harmonic analysis. It was rather too abstract for me. We graduate students tried to make sense of Bohm's ideas with Hiley, but it seemed that every few days ideas he had talked about earlier were scrapped for new ideas, so it was rather frustrating.

In hindsight we can see that Bub was experiencing the attempts Bohm was making to develop new perspectives for his research. Eventually Bub found his own way through the reading of a paper by Margenau on the measurement problem; subsequently Bohm suggested he "read a paper by Wiener and Siegel, 'The differential space theory of quantum systems,' and consider treating the collapse problem in the framework of a hidden variables theory." More particularly, "Bohm's thought was that one should be able to exploit the Wiener-Siegel

[76] For the debate on the theoretical interpretation of the Aharonov-Bohm effect, see Lyre (2009).

[77] Talk with Jeffrey Bub, 22 May 2002, American Institute of Physics, College Park, MD. E-mails from Bub to the author, 29 May 2014.

'differential space' approach to quantum mechanics to construct an explicit nonlinear dynamical 'collapse' theory for quantum measurement processes" (Bub 1997, p. xii). Thus, with Bub as a student, Bohm came back to the hidden variable approach while in a different manner from that in the early 1950s. Bub coped with the suggestion and a thesis and papers resulted (Bohm and Bub 1966a, b).[78]

Bub was probably one of the first students to get a PhD in physics working on foundations of quantum mechanics. After a string of positions he eventually became a Distinguished Professor at University of Maryland. After working with Bohm, Bub's interests moved to quantum logic. Bub ultimately evolved for a kind of reconciliation between the two themes he had worked through his life: hidden variables and quantum logic (Bub 1997, p. xiii). In 1998 he won the prestigious Lakatos Award with the book *Interpreting the Quantum World* (Bub 1997) where this reconciliation is presented. Thus Bub's story is a success story of somebody who began and endured in the field of foundations of quantum mechanics. However, the very fact that most of his academic career was developed in philosophy departments, a standard followed by many quantum foundationalists till today, is reminiscent of the adversities such physics researchers have found among their fellow physicists. When he began his doctoral dissertation, Bohm had warned him that with such a subject he would not get a position in a physics department.[79] Bohm was premonitory.

2.6 New Perspectives: Wholeness and Implicate Order

Looking for new perspectives to understand quantum mechanics, Bohm drew heavily on analogies and images to convey the content of his new ideas on order, the most well-known being the image of a drop of ink falling into a rotating cylinder full of glycerin. When the cylinder rotates in one direction the ink disappears in the glycerin, which Bohm referred to as the implicate order. When it rotates in the opposite direction, the drop reappears, namely the explicate order. Bohm would associate the explicate order with classical or macroscopic phenomena and the implicate order with quantum phenomena. For Bohm, the usual interpretation of quantum mechanics was not the final word in quantum physics, and he went on to associate the implicate order with a physical theory yet to be worked out that has standard quantum mechanics as a limiting case.[80]

Bohm's ideas of implicate and explicate order resulted from diverse influences and inspirations. As he recalled, there was his search for new ideas and his enduring reflection about what was common to his previous approach and standard quantum

[78] Margenau's paper was Margenau (1963) and the papers by Norbert Wiener and Armand Siegel were Wiener and Siegel (1953, 1955) and Siegel and Wiener (1956).

[79] Jeffrey Bub, talk with the author, 3 April 2014.

[80] Bohm et al. (1970), Bohm (1971, 1973), Bohm (1981).

mechanics (a task that was eased by John Bell's 1965 work pointing to non-locality as the irreducible quantum feature, as we will see in Chap. 7). In addition, there were the insights from a TV program in which he saw the demonstration with ink and glycerin and the fruitful interaction with mathematicians and mathematical physicists. The question remains of how much Bohm was influenced in the early 1960s by his dialogues with the influential Indian writer Jiddu Krishnamurti, with whom Bohm kept a longstanding interaction (Peat 1997, Chap. 11). Bohm once acknowledged some influence from Krishnamurti's psychological ideas on the non-separability between observer and observed, which reinforced his ideas on the analogous problems in quantum measurement. Later, however, he did not mention this influence again. Basil Hiley, Bohm's longstanding collaborator to whom we will refer later, thinks that these dialogues were not influential in Bohm's physics; rather, they played a role in Bohm's reflections about society, thoughts, and creativity. A reflection on the relationship between observer and observed had been an essential feature of Bohm's early reflections on the foundations of quantum mechanics, see for instance how he treated measurement both in his 1951 book and 1952 causal interpretation. Thus, it seems that the influence of these dialogues on his physics, if any, was superseded by his enduring reflection on measurement in quantum physics.[81]

Implicate and explicate order would have remained mere philosophical or scientific intuitions if it had not been for the mathematical elaboration they later received. To accomplish this Bohm did not work alone. He counted on the collaboration of Basil Hiley, who was born in Burma, then part of the British Raj. He came to England when India gained independence. Hiley did his degree and doctoral studies at King's College working with the theory of condensed matter, but he was interested in abstract mathematics and foundational physics. He attended a lecture by Bohm at the end of his degree and was spellbound. Professional interaction with Bohm, however, came later, after Hiley was hired by Birkbeck College in 1961. Bohm was also there and he became Bohm's assistant. At the beginning of their collaboration there was no connection with Bohm's previous work on the causal interpretation. "When I started with Bohm we did not mention or discuss his '52 Hidden Variable approach at all" and "for about the first 10 years we didn't discuss the Hidden Variable Theory hardly at all," Hiley stated. Furthermore, according to Hiley's recollections, he "was brought up in an atmosphere where it was generally agreed that there was something basically wrong with the '52 paper of Bohm." Instead of hidden variable models, Hiley engaged with new mathematical objects with Bohm and the mathematician Roger Penrose, in a seminar they informally ran on Thursday afternoons.[82]

---

[81] Bohm (1982, 1987). Basil Hiley 2008, American Institute of Physics, ibid.

[82] Basil Hiley interviewed by Olival Freire, 11 Jan 2008, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD. See also Basil Hiley interviewed by Alexei Kojevnikov, 05 Dec 2000, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD.

Bohm and Hiley's strategy was to analyze the algebraic structures behind quantum mechanics' mathematical formalism and subsequently look for more general algebras which could be reduced to the quantum algebras as special cases. This strategy was informed by the fact that they did not want to take any kind of space-time geometry as assumptions in their reasoning. Instead they tried to develop algebraic structures from which space-time could emerge. Here the algebraic primary structure would be the implicate order and the emerging space-time geometry would be the explicate order. With the benefit of hindsight, we can identify Hiley's unique contribution in this sense. Indeed Hiley was, and still is, the mathematical mind behind the research program related to the idea of order. A number of different factors also contributed to the development of this mathematical approach, such as new and mathematically talented students including Fabio Frescura, interactions with the mathematician Roger Penrose at Birkbeck College, and inspiration from the Brazilian physicist Mario Schönberg's early works on algebras and geometry. Highly sophisticated from the mathematical point of view, such an approach has, however, suffered from little contact with experimental results, which could help to inform the mathematical choices to be made.[83]

2.6.1 Returning to the Quantum Potential

In the late 1970s a new stage in Bohm's quest for a new approach to quantum mechanics began, albeit strongly overlapping the previous one. To a certain extent it meant a return to Bohm's 1952 ideas. This return, almost 30 years later, is vividly described by Basil Hiley[84]:

> We had a couple of research students working for us, Chris Dewdney and Chris Philippidis. They came to me one day with Bohm's 1952 paper in their hand. And, they said, "Why don't you and David Bohm talk about this stuff?" And I then started saying, "Oh, because it's all wrong." And then they started asking me some questions about it and I had to admit that I had not read the paper properly. Actually I had not read the paper at all apart from the introduction! And when I took it and, so, you know, I was now faced with embarrassment that our research students [Laugh] were putting me in, in a difficult position, and so I went back home and I spent the weekend working through it. As I read it, I thought, "What on earth is wrong with this? It seems perfectly all right. Whether that's the way nature behaves is another matter." But as far as the logic, the mathematics, and the arguments were concerned, it was sound. I went back again to see the two again, I said, "Okay, let's now work out what the trajectories are, work out what the quantum potential looks like in various situations.

The students and the surprised Hiley went on to calculate the trajectories allowed by Bohm's quantum potential using the recently-arrived desktop computer

[83] See Bohm and Hiley (1981), Frescura and Hiley (1980a, b). Reference to Schönberg is in Frescura and Hiley (1980b).

[84] Basil Hiley, ibid.

resources to plot these trajectories, creating images of quantum phenomena (Philippidis et al. 1979). Motivated by students and collaborators, Bohm returned to his 1952 approach, but now he had a new problem: how to interpret such an approach and its deterministic trajectories shaped by the nonlocal physical interactions resulting from the quantum potential. Here there is a crucial point to consider while charting Bohm's thoughts on quantum mechanics. While he and his colleagues kept the mathematics and the model used in the 1952 paper, they changed many of their philosophical and conceptual assumptions. The quantum potential was no longer considered a new physical potential. Instead it was interpreted as an indication of a new order, in particular a kind of "active information." Emphasis was no longer put on the causality embedded in such an approach. According to Bohm and Hiley (1993), in the book synthesizing their ideas on quantum physics, *The Undivided Universe*, after considering terms such as "causal" and "hidden variable" interpretations "too restrictive" and stating that "nor is this sort of theory necessarily causal," they concluded that "the question of determinism is therefore a secondary one, while the primary question is whether we can have an adequate conception of the reality of a quantum system, be this causal or be it stochastic or be it of any other nature." Their main philosophical stance was to look for an ontological view of quantum phenomena, while the main scientific challenge remained how to tie such a requirement to the mathematical work related to the idea of an "implicate order." This challenge has survived Bohm and is a task on which Hiley remains focused.[85]

It is time now to ask about the share of continuity and the share of change in Bohm's enduring research on the foundations of the quantum theory. Continuity was related to the philosophical commitment to the quest for an ontology, an explanation of the kind of world described by quantum physics. From the *Quantum Theory* 1951 textbook to the 1993 *The Undivided Universe*, there was a permanent commitment to a kind of scientific realism. The changes were also formidable. Determinism, the leitmotif of the causal interpretation, was abandoned. The style of scientific research also changed along the way, with the building of physical models being replaced by a more abstract research on the algebras underlying the mathematical structure of quantum physics. Influences from Marxism were replaced by Eastern thinking. As influential as Bohm's thoughts on quantum physics may be, it has been hard to identify which part or stage of his thinking is being considered when his ideas are invoked by his current readers. An early example of this was Fritjof Capra and his bestseller *The Tao of Physics*, in which Bohm's ideas on order in quantum theory were presented while Bohm's previous ideas on a causal interpretation of the same theory were ignored. Bohm did not help his readers to make sense of the evolution of his thoughts and in the most widely influential of his books, *Wholeness and the implicate order*, he conflated different stages of his interpretation of quantum mechanics. Even in a paper showing the connections

[85] Philippidis et al. (1979) and Bohm and Hiley (1993, p. 2). For Hiley's recent work, see Hiley and Callaghan (2012).

between two of his most important approaches to quantum mechanics, when "asked to explain how [his] ideas of hidden variables tie up with those on the implicate order," he emphasized the continuity more than his change of emphasis.[86]

2.7 On the Legacy of a Notable Quantum Dissident[87]

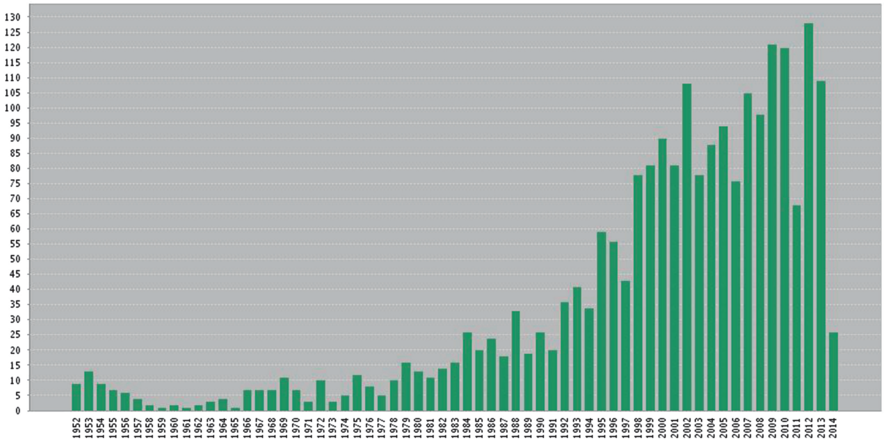
Recognition was erratic in David Bohm's case. In the late 1940s he was considered among the most promising young American theoretical physicists. During the 1950s, his work on the causal interpretation was poorly received, casting doubts on that promise. Few were those who, like the Scottish engineer Lancelot L. Whyte, considered the causal interpretation anything but fleeting. Whyte considered Bohm's work comparable to Kepler's in mechanics, which was a compliment for a physicist. Later, in the 1980s, to some extent reflecting the physicists' changed mood about research on the foundations of quantum physics, his whole work fared better. A sign of the late prestige accorded to Bohm and to the field in which he mostly worked was the volume in honor of the centenary edition of *Physical Review*, the most influential American physics journal. It included commentaries and reprints from the most important papers ever published in this periodical. In the section on "Quantum Mechanics", edited by Sheldon Goldstein and Joel Lebowitz, all the papers, including Bohm's 1952 paper on the causal interpretation, concern the foundations of quantum mechanics and a photo of Bohm opens the section. The Festschrift honoring his 70th birthday had already brought tributes from scientists such as Ilya Prigogine, Maurice Wilkins, and Richard Feynman, all Nobel Prize laureates at the time the book appeared, Anthony Leggett, who would go on to win the 2003 Physics Nobel Prize, John Bell, Roger Penrose, David Pines, Bernard d'Espagnat, and Jean-Pierre Vigier, in addition to a number of Bohm's collaborators. The ultimate accolade was to be elected Fellow of the Royal Society in 1990. After his death, his prestige continued to grow, as remarked by his long-standing friend, the American physicist Melba Phillips (1907–2004), "it is too bad, very sad indeed, that he did not live to see how his reputation has shot up recently. His interpretation of quantum mechanics is becoming respected not only by philosophers of science but also by 'straight' physicists."[88]

---

[86] Capra (1991), Bohm (1981) and Bohm (1987).

[87] While the use of the term dissident for Bohm and his works in quantum mechanics is almost self-evident, I use the term for a wide description of those physicists who contributed to develop the research on the foundations of quantum mechanics after 1950. I postpone a justification for my use of the term to Chap. 9. The first to use a similar term in this context, as far as I know, was Karl Popper (Popper and Bartley 1982, p. 100): "Unlike the orthodoxy, the dissenters are far from united. Not two of them agree (except perhaps Bohm and de Broglie)."

[88] Stroke (1995). Lancelot Whyte to Léon Rosenfeld, 8 Apr 1958, RP (Bohm et al. 1987). Melba Phillips to David Peat, 17 Oct 1994, A22, BP.



Picture 2.3 Citations of Bohm's causal interpretation papers from 1952 to 2014—Source of the data: Web of Science

With the benefit of hindsight, how can Bohm's legacy be assessed in the first decades of the twenty-first century? The question requires a multifaceted answer. First, there were achievements not directly related to his research on the foundations of quantum theory. This was the case of his work on collective variables, in plasma and metals, conducted with Eugene Gross and David Pines, a work which had begun before his shift towards the causal interpretation. And then, there was the Aharonov-Bohm effect, published in 1959, which became a landmark when one speaks about quantum effects without classical equivalent. These achievements are beyond debate; they are considered feats in the history of physics in the twentieth century.[89] In addition, if one takes scientometric data, the number of citations, into consideration, the aforementioned are by far among the most influential contributions by David Bohm. Second, the research lines on the interpretational issues he worked on have survived him and are fields of live research with their value to the future development of physics still subject to controversial assessments. They may be grouped into three different strands. The first continues work on Bohm's original 1952 proposal, not only trying to extend the first physical models but also keeping Bohm's early philosophical commitments to determinism and realism. This is, for instance, the path chosen by Peter Holland (1993). More recently, this trend has been renewed by Antony Valentini. He has worked with deterministic hidden-variables theories in the direction of relaxing the equality between distribution of hidden variables and probability distribution from standard quantum theory. As for him quantum physics may be a mere case of an effective theory of an equilibrium state, we should look for discrepancies between hidden variables and quantum

[89] Basil Hiley cited these achievements and Bohm's contributions to our understanding of quantum non-locality when asked for the background for Bohm's nomination for the Nobel Prize. B. Hiley to Sessler, 9 Jan 1989, A172, BP.

theory predictions in situations of nonequilibrium. Still, for him, we should look for this in astrophysical and cosmological tests (Valentini 2007, 2010). The second strand concerns Bohmian mechanics, a name coined by Detlef Dürr, Sheldon Goldstein, and Nino Zanghi. They construed Bohm's proposal in a very clean and elegant way. In his original paper Bohm had worked out analogies between Schrödinger's equation and classical Hamilton-Jacobi equations, which led to an emphasis on the role of the non-classical potential that Bohm christened the "quantum potential." Dürr and colleagues, however, adopted just two premises: the state which describes quantum systems evolves according to Schrödinger's equation and particles move, that is, they have a speed in the configuration space. Thus for them, "Bohmian mechanics is a version of quantum mechanics for non relativistic particles in which the word 'particle' is to be understood literally: In Bohmian mechanics quantum particles have positions, always, and follow trajectories. These trajectories differ, however, from the classical Newtonian trajectories." With this approach, without referring to the quantum potential and the difficult problem of its physical interpretation, they derived the same results one gets both with standard quantum mechanics and with Bohm's original approach for nonrelativistic phenomena. This approach has been useful for discussing quantum chaos, and for this reason it has received widespread acceptance, well beyond physicists just interested in the foundations of quantum mechanics. One should note that when these physicists define what they understand to be a Bohmian theory, the preference for determinism disappears and they consider that "a Bohmian theory should be based upon a clear ontology," meaning by ontology "what the theory is fundamentally about." While for non-relativistic physics they have adopted a particle ontology, they admit that they "have no idea what the appropriate ontology for relativistic physics actually is." This way, the commitment to a quantum ontology comes before an engagement with a causal pattern for physical theories, a position analogous to what was adopted by David Bohm and Basil Hiley since the 1960s.[90]

The third strand of Bohm's scientific legacy is represented by Basil Hiley, who continues to work on research that he and Bohm had been carrying out before Bohm's death. This research tries to connect the insights of implicate order and active information with the quest for algebraic structures able to underpin space-time geometry and standard quantum mechanics. This program has inherited from the causal interpretation the major challenge of obtaining a fully relativistic treatment in order to match the level attained by standard quantum mechanics with the Dirac equation.

Rather than one specific and lasting contribution, I think he should be acknowledged for his attitude to the importance of the research on the foundations of this theory. His late recognition was not independent of this role. The point is that the most influential single theoretical result in the foundations of quantum theory after WWII was Bell's theorem, which jointly with its experimental tests led to the

[90] Dürr et al. (1992, 1996, 2009).

recognition of entanglement as a physical property with far-reaching implications both for science and technology. However, John Bell's work has a close historical connection with Bohm's work on a hidden variable interpretation. Max Jammer wrote that "it was due to Bohm that many physicists and philosophers of science [. . .] examined more closely the logic of von Neumann's argument and that finally, in 1964, J. S. Bell clarified completely the nature of von Neumann's unnecessarily restrictive assumptions with the removal of which his proof breaks down." According to the recollections of Bell himself, "Smitten by Bohm's papers," he attempted to determine what was wrong with von Neumann's proof, since it did not allow for hidden variables in quantum mechanics. Here is not the place to chart the origins of Bell's theorem, which will be done in Chap. 7. For our purposes, suffice to say Bell was directly motivated by the very existence of Bohm's proposal and by its reception among physicists. His statements—"In 1952 I saw the impossible done," and "Bohm's 1952 papers on quantum mechanics were for me a revelation"—hide more truth than is usually recognized, "the impossible done" referring to the appearance of the causal interpretation which was considered by prevailing wisdom an impossible feat.[91]

2.7.1 Historiography on Bohm's Interpretation

The initial poor reception of Bohm's causal interpretation has attracted the attention of commentators. Some of them have looked to the political climate of the Cold War and Bohm's exile to explain this. "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," stated Bohm's assistant, Basil Hiley. F. David Peat, a science writer and former Bohm collaborator, also advanced the political explanation for the unfavorable reaction to Bohm's work, but limited its force to the Princeton physics community. The historians Russel Olwell and Shawn Mullet blamed Bohm's Brazilian exile for the poor response to his causal interpretation theory. Others, such as James Cushing, underestimated the number of physicists who analyzed Bohm's papers, writing "[Bohm's proposal] was basically ignored, rather than either studied or rebutted." Our analysis, however, suggests otherwise, more related to the practice of physics as a cultural field. As pointed out by Max Jammer and Mara Beller, the dominance of the Copenhagen school in the early 1950s was very effective. The main critics of Bohm's ideas were Europeans, aligned with Bohr's complementarity, and not influenced by McCarthyism. Some of them were even Marxists. The record of debates about Bohm's papers and about his activities

---

[91] Jammer (1988, p. 694), Bernstein (1991, pp. 65–68) and Bell (1982, 1987). For the history of Bell's theorem and its experiments, see Chap. 7.

in Brazil and Israel should not lead us to underestimate these debates. In addition to the dominance of complementarity, other factors were also influential.

We have seen that Bohm and his collaborators searched in vain for predictions not foreseen by the usual quantum mechanics and also failed to find a satisfactory relativistic generalization of their approach.[92] Indeed, as most results of the causal interpretation were to replicate results already obtained with standard quantum physics, the idea grew that the controversy over the interpretation of quantum physics was a matter of philosophical taste, without implications for the workings of physics. Even physicists who were not open critics of the causal interpretation concluded this. We have seen in Chap. 1 the case of A. Messiah's influential textbook. He stated that the controversy "belongs to the philosophy of science rather than to the domain of physical science proper" (Messiah 1961, p. 48). A similar example is Fritz Bopp's statement: "what we have done today was predicting the possible development of physics—we were not doing physics but metaphysics" (in Körner 1957, p. 51). It was not by chance that in the 1950s the only conference dedicated to the subject was organized by philosophers rather than by physicists (Körner 1957). The idea of a philosophical controversy survived in the common discourse on the subject even when the context changed, as was the case when Max Jammer entitled his 1974 book *The Philosophy of Quantum Mechanics*. One should acknowledge, however, that at least in the special context of the young French Marxist physicists around Jean-Pierre Vigier, the philosophical bias of the dispute may have been considered more appealing than simply a diverting factor. And yet, the absence of new results reinforced the derogatory label of "philosophical" applied by the opponents of the causal interpretation, further discouraging young physicists from working on a subject that ultimately was more a question of philosophy than of physics.[93]

The ensemble of these reasons explains why Bohm's ideas challenged the dominance of the complementarity view among physicists, but did not weaken it enough to create a favorable space for the immediate development of alternative interpretations.

---

[92] Hiley (1997, p. 113), Peat (1997, p. 133) and Olwell (1999, p. 750). 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 from 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. Cushing (1994, p. 144), Jammer (1974) and Beller (1999).

[93] However, Messiah 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. About Bopp, by the way, he was then working on another alternative interpretation, the so-called "stochastic interpretation."

References

Aharonov, Y., Bohm, D.: Significance of electromagnetic potentials in the quantum theory. Phys. Rev. **115**(3), 485–491 (1959)
Anderson, P.: Considerations on Western Marxism. NLB, London (1976)
Andrade, A.M.R.: Físicos, mésons e política: a dinâmica da ciência na sociedade. HUCITEC, São Paulo (1999)
Bacciagaluppi, G., Valentini, A.: Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference. Cambridge UniversityPress, Cambridge (2009)
Bachelard, G.: Le Nouvel Esprit Scientifique. Félix Alcan, Paris (1934)
Bell, J.S.: On the impossible pilot wave. Found. Phys. **12**(10), 989–999 (1982)
Bell, J.S.: Beables for quantum field theory. In: Hiley, B.J., Peat, F.D. (eds.) Quantum Implications: Essays in Honour of David Bohm, pp. 227–234. Routledge & Kegan, London (1987)
Bell, J.S.: Speakable and Unspeakable in Quantum Mechanics: Collected Papers on Quantum Philosophy. With an Introduction by Alain Aspect. Cambridge University Press, Cambridge (2004)
Beller, M.: Quantum Dialogue—The Making of a Revolution. The University of Chicago Press, Chicago (1999)
Bernstein, J.: Quantum Profiles. Princeton University Press, Princeton (1991)
Berry, M., Pollard, B.: The Physical Tourist Physics in Bristol. Phys. Perspect. **10**, 468–480 (2008)
Besson, V.: Les premiers travaux de Jean-Pierre Vigier sur la théorie des quanta: une rencontre entre science et marxisme (1951-1954). Master dissertation, Université Claude Bernard Lyon 1 (2011)
Bitbol, M.: Schrödinger's Philosophy of Quantum Mechanics. Kluwer, Dordrecht (1996)
Blokhintsev, D.I.: Critique de la conception idéaliste de la théorie quantique. Questions scientifiques—Physique, pp. 95–129. Les éditions de la nouvelle critique, Paris (1952).
Bohm, D.: Quantum Theory. Prentice-Hall, New York (1951)
Bohm, D.: Reply to a criticism of a causal re-interpretation of the quantum theory. Phys. Rev. **87** (2), 389–390 (1952a)
Bohm, D.: A suggested interpretation of the quantum theory in terms of hidden variables—I & II. Phys. Rev. **85**(2), 166–179 and 180–193 (1952b)
Bohm, D.: Proof that probability density approaches (Psi)2 in causal interpretation of the quantum theory. Phys. Rev. **89**(2), 458–466 (1953a)
Bohm, D: A Discussion of Certain Remarks by Einstein on Born's Probability Interpretation of the [psi]—Function. Scientific Papers presented to Max Born. Edinburgh, Oliver and Boyd, pp. 13–19 (1953b)
Bohm, D.: Hidden variables in the quantum theory. In: Bates, D.R. (ed.) Quantum Theory—III—Radiation and High Energy Physics, pp. 345–387. Academic, New York (1962)
Bohm, D.: Quantum theory as an indication of a new order in physics. Part A. The development of new order as shown through the history of physics. Found. Phys. **1**(4), 359–381 (1971)
Bohm, D.: Quantum theory as an indication of a new order in physics. Part B. Implicate and explicate order in physical law. Found. Phys. **3**(2), 139–168 (1973)
Bohm, D.: Wholeness and the Implicate Order. Routledge & Kegan Paul, London (1981)
Bohm, D.: Interview. New Sci. **96**(1331), 361–365 (1982)
Bohm, D.: Hidden variables and the implicate order. In: Hiley, B., Peat, D. (eds.) Quantum Implications: Essays in Honour of David Bohm, pp. 33–45. Routledge, London (1987)
Bohm, D., Bub, J.: A proposed solution of measurement problem in quantum mechanics by a hidden variable theory. Rev. Mod. Phys. **38**(3), 453–469 (1966a)
Bohm, D., Bub, J.: A refutation of proof by Jauch and Piron that hidden variables can be excluded in quantum mechanics. Rev. Mod. Phys. **38**(3), 470–475 (1966b)
Bohm, D., Gross, E.P.: Theory of plasma oscillations. A. Origin of medium-like behavior. Phys. Rev. **75**(12), 1851–1864 (1949a)

Bohm, D., Gross, E.P.: Theory of plasma oscillations. B. Excitation and damping of oscillations. Phys. Rev. **75**(12), 1864–1876 (1949b)
Bohm, D., Hiley, B.J.: On a quantum algebraic approach to a generalized phase-space. Found. Phys. **11**(3–4), 179–203 (1981)
Bohm, D., Hiley, B.J.: The Undivided Universe : An Ontological Interpretation of Quantum Theory. Routledge, London (1993)
Bohm, D., Pines, D.: A collective description of electron interactions. 1. Magnetic interactions. Phys. Rev. **82**(5), 625–634 (1951)
Bohm, D., Pines, D.: A collective description of electron interactions. 3. Coulomb interactions in a degenerate electron gas. Phys. Rev. **92**(3), 609–625 (1953)
Bohm, D., Schiller, R.: A causal interpretation of the Pauli equation (B). Nuovo Cimento Suppl **1** (1), 67–91 (1955)
Bohm, D., Vigier, J.P.: Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations. Phys. Rev. **96**(1), 208–216 (1954)
Bohm, D., Vigier, J.P.: Relativistic hydrodynamics of rotating fluid masses. Phys. Rev. **109**(6), 1882–1891 (1958)
Bohm, D., Schiller, R., Tiomno, J.: A causal interpretation of the Pauli equation (A). Nuovo Cimento Suppl **1**, 48–66 (1955)
Bohm, D., Hillion, P., Takabayasi, T., Vigier, J.P.: Relativistic rotators and bilocal theory. Prog. Theor. Phys. **23**(3), 496–511 (1960a)
Bohm, D., Hillion, P., Vigier, J.P.: Internal quantum states of hyperspherical (nakano) relativistic rotators. Prog. Theor. Phys. **24**(4), 761–782 (1960b)
Bohm, D., Hiley, B.J., Stuart, A.E.G.: On a new mode of description in physics. Int. J. Theor. Phys. **3**(3), 171–183 (1970)
Bohm, D., Hiley, B.J., Peat, F.D.: Quantum Implications: Essays in Honour of David Bohm. Routledge & Kegan Paul, New York (1987)
Bohm, D., Biederman, C.J., Pylkkänen, P.: Bohm-Biederman Correspondence. Routledge, London (1999)
Bohr, N.: Discussion with Einstein on epistemological problems in atomic physics. In: Schilpp, P.A. (ed.) Albert Einstein—Philosopher-Scientist, pp. 199–242. The Library of the Living Philosophers, Evanston (1949)
Bohr, A., Mottelson, B.R., Pines, D.: Possible analogy between the excitation spectra of nuclei and those of the superconducting metallic state. Phys. Rev. **110**(4), 936–938 (1958)
Broglie, L.: Nouvelles Perspectives en Microphysique. Albin Michel, Paris (1956)
Broglie, L.: La théorie de la mesure en mécanique ondulatoire interprétation usuelle et interprétation causale. Gauthier-Villars, Paris (1957)
Broglie, L.D., Vigier, J.P., Bohm, D., Takabayasi, T.: Rotator model of elementary particles considered as relativistic extended structures in minkowski space. Phys. Rev. **129**(1), 438–450 (1963)
Brownell, G. L.: Physics in South America. Physics Today July, pp. 5–12 (1952)
Bub, J.: Interpreting the Quantum World. Cambridge University Press, Cambridge (1997)
Capra, F.: The Tao of Physics : An Exploration of the Parallels Between Modern Physics and Eastern Mysticism. Shambhala Publications, Boston, MA (1991)
Carson, C.: Heisenberg in the Atomic Age: Science and the Public Sphere. German Historical Institute, Washington, DC (2010)
Caute, D.: Le Communisme et les intellectuels français, 1914-1966. Gallimard, Paris (1967)
Cross, A.: The Crisis in Physics: Dialectical Materialism and Quantum Theory. Soc. Stud. Sci. **21**, 735–759 (1991)
Cushing, J.: Quantum Mechanics—Historical Contingency and the Copenhagen Hegemony. The University of Chicago Press, Chicago (1994)
Dürr, D., Goldstein, S., Zanghi, N.: Quantum chaos, classical randomness, and bohmian mechanics. J. Stat. Phys. **68**(1–2), 259–270 (1992)

Dürr, D., Goldstein, S., Zanghi, N.: Bohmian mechanics at the foundation of quantum mechanics. In: Cushing, J.T., Fine, A., Goldstein, S. (eds.) Bohmian Mechanics and Quantum Theory: An Appraisal, pp. 21–44. Kluwer, Dordrecht (1996)

Dürr, D., Goldstein, S., Tumulka, R., Zanghi, N.: Bohmian mechanics. In: Greenberger, D., Hentsche, K., Weinert, F. (eds.) Compendium of Quantum Physics—Concepts, Experiments, History and Philosophy, pp. 47–55. Springer, Berlin (2009)

Einstein, A.: Remarks to the essays appearing in this collective volume. In: Schilpp, P.A., Einstein, A. (eds.) Albert Einstein—Philosopher-Scientist, pp. 665–668. The Library of the Living Philosophers, Evanston, IL (1949)

Einstein, A.: Elementare Überlegungen zur Interpretation der Grundlagen der Quanten-Mechanik. Scientific Papers presented to Max Born, pp. 33–40. Oliver and Boyd, Edinburgh . French translation in Albert Einstein, Oeuvres Choisies, ed. by F. Balibar, B. Jech, and O. Darrigol, Paris: Editions du Seuil-CNRS, 1989, pp. 1251–1256 (1953)

Einstein, A., Born, M., Born, H.: The Born-Einstein Letters: Correspondence Between Albert Einstein and Max and Hedwig Born from 1916-1955, with Commentaries by Max Born. Macmillan, London (1971)

Epstein, S.T.: The causal interpretation of quantum mechanics. Phys. Rev. **89**(1), 319 (1953a)

Epstein, S.T.: The causal interpretation of quantum mechanics. Phys. Rev. **91**(4), 985 (1953b)

Feyerabend, P.: Professor Bohm's philosophy of nature. Br. J. Philos. Sci. **10**(40), 321–338 (1960)

Feynman, R.: The present situation in fundamental theoretical physics. An. Acad. Bras. Cienc. **26** (1), 51–60 (1954)

Fock, V.A.: On the interpretation of quantum mechanics. Czechoslov. J. Phys. **7**, 643–656 (1957)

Forstner, C.: The early history of David Bohm's quantum mechanics through the perspective of Ludwik Fleck's thought-collectives. Minerva **46**(2), 215–229 (2008)

Freire Jr., O.: The crisis in physics—comment. Soc. Stud. Sci. **22**(4), 739–742 (1992)

Freire Jr., O.: David Bohm e a controvérsia dos quanta. Centro de Lógica, Epistemologia e História da Ciência, Campinas [Brazil] (1999)

Freire Jr., O.: Gaston Bachelard et Louis de Broglie, ont-ils toujours été en synthonie? Cahiers Gaston Bachelard **6**, 160–166 (2004a)

Freire Jr., O.: Popper, probabilidade e teoria quântica. Episteme [Porto Alegre] **18**, 103–127 (2004b)

Freire Jr., O.: Science and exile: David Bohm, the cold war, and a new interpretation of quantum mechanics. Hist. Stud. Phys. Biol. Sci. **36**(1), 1–34 (2005)

Freire Jr., O.: Causality in physics and in the history of physics: a comparison of Bohm's and Forman's papers. In: Carson, C., Kojevnikov, A., Trischler, H. (eds.) Weimar Culture and Quantum Mechanics: Selected Papers by Paul Forman and Contemporary Perspectives on the Forman Thesis, pp. 397–411. Imperial College & World Scientific, London (2011a)

Freire Jr., O.: Continuity and change: charting David Bohm's evolving ideas on quantum mechanics. In: Krause, D., Videira, A. (eds.) Brazilian Studies in Philosophy and History of Science, pp. 291–299. Springer, Heidelberg (2011b)

Freire Jr., O.: On the connections between the dialectical materialism and the controversy on the quanta. Jahrbuch Für Europäihce Wissenschaftskultur **6**, 195–210 (2011c)

Freire Jr., O.: On the influence of science milestones on the history and philosophy of science. In: Blum, A., Gavroglu, K., Renn, J. (eds.) Towards a History of the History of Science: 50 Years Since "Structure". Edition Open Access, Berlin (2014c)

Freire Jr., O., Lehner, C.: 'Dialectical materialism and modern physics', an unpublished text by Max Born. Notes Rec. R. Soc. **64**(2), 155–162 (2010)

Freistadt, H.: The crisis in physics. Sci. Soc. **17**, 211–237 (1953)

Freistadt, H.: Connection between recent theories of Bohm, de Broglie, Dirac, and Schrodinger. Phys. Rev. **98**(4), 1176 (1955)

Freistadt, H.: The causal formulation of quantum mechanics of particles: The theory of de Broglie, Bohm and Takabayasi. Nuovo Cimento Suppl **5**, 1–70 (1957)

Frescura, F.A.M., Hiley, B.J.: The implicate order, algebras, and the spinor. Found. Phys. **10**(1–2), 7–31 (1980a)

Frescura, F.A.M., Hiley, B.J.: The algebraization of quantum-mechanics and the implicate order. Found. Phys. **10**(9–10), 705–722 (1980b)

Gaddis, J.L.: The Cold War: A New History. Penguin, New York (2005)

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

Graham, L.R.: Science, Philosophy, and Human Behavior in the Soviet Union. Columbia University Press, New York (1987)

Halpern, O.: A proposed re-interpretation of quantum mechanics. Phys. Rev. **87**(2), 389 (1952)

Hanson, N.R.: Copenhagen interpretation of quantum theory. Am. J. Phys. **27**(1), 1–15 (1959)

Harding, S.G. (ed.): Can Theories be Refuted? Essays on the Duhem-Quine Thesis. Synthese Library v 81. D. Reidel Publishing Co., Dordrecht (1976)

Heisenberg, W.: Physics and Philosophy; the Revolution in Modern Science. Harper, New York (1958)

Hermann, G., Soler, L., Schnell, A.: Les fondements philosophiques de la mécanique quantique introd., présentation, postf. critique par Lena Soler trad. par Alexandre Schnell en collab. avec Lena Soler préf. de Bernard d'Espagnat. J. Vrin, Paris (1996)

Hiley, B.J.: David Joseph Bohm. Biogr. Mem. Fellows R. Soc. **43**, 106–131 (1997)

Hiley, B.J., Callaghan, R.E.: Clifford algebras and the Dirac-Bohm quantum Hamilton-Jacobi equation. Found. Phys. **42**, 192–208 (2012)

Hobsbawm, E.J.: On history. Weidenfeld & Nicolson, London (1997)

Hobsbawm, E.J.: How to Change the World : Marx and Marxism, 1840-2011. Little, Brown, London (2011)

Hoffmann, D.: Robert Havemann: antifascist, communist, dissident. In: Macrakis, K., Hoffmann, D. (eds.) Science Under Socialism : East Germany in Comparative Perspective, pp. 269–285. Harvard University Press, Cambridge, MA (1999)

Holland, P.R.: The Quantum Theory of Motion: An Account of the de Broglie-Bohm Causal Interpretation of Quantum Mechanics. Cambridge University Press, Cambridge, England (1993)

Home, D., Whitaker, M.A.B.: Ensemble interpretations of quantum-mechanics—a modern perspective. Phys. Rep. **210**(4), 223–317 (1992)

Institut International de Physique Solvay: Electrons et photons—Rapports et discussions du Cinquième Conseil de Physique tenu à Bruxelles du 24 au 29 octobre 1927. Gauthier-Villars, Paris (1928) [English translation in Bacciagaluppi and Valentini 2009]

Jacobsen, A.: Léon Rosenfeld's Marxist defense of complementarity. Hist. Stud. Phys. Biol. Sci. **37**(Suppl), 3–34 (2007)

Jacobsen, A.: Léon Rosenfeld—physics, philosophy, and politics in the twentieth century. World Scientific, Singapore (2012)

Jammer, M.: The philosophy of quantum mechanics—the interpretations of quantum mechanics in historical perspective. Wiley, New York (1974)

Jammer, M.: David Bohm and his work on the occasion of his 70th-birthday. Found. Phys. **18**(7), 691–699 (1988)

Kaiser, D.: The atomic secret in red hands? American suspicions of theoretical physicists during the early Cold War. Representations **90**, 28–60 (2005)

Kaiser, D.: How the hippies saved physics: science, counterculture, and the quantum revival. W. W Norton, New York (2012)

Keller, J.B.: Bohm's Interpretation of the Quantum Theory in Terms of Hidden Variables. Phys. Rev. **89**(5), 1040–1041 (1953)

Kojève, A., Auffret, D.: L' Idée du déterminisme dans la physique classique et dans la physique moderne. Présentation de Dominique Auffret. Librairie générale française, Paris (1990)

Kojevnikov, A.: David Bohm and collective movement. Hist. Stud. Phys. Biol. Sci. **33**, 161–192 (2002)

Kojevnikov, A.: Stalin's Great Science: The Times and Adventures of Soviet Physicists. Imperial College Press, London (2004)
Kojevnikov, A.: Probability, marxism, and quantum ensembles. Yearb. Eur. Cult. Sci. **6**, 211–235 (2011)
Körner, S.: Observation and Interpretation; A Symposium of Philosophers and Physicists. Butterworths, London (1957)
Kuzemsky, A.L.: Works by D. I. Blokhintsev and the development of quantum physics. Phys. Part. Nucl. **39**(2), 137–172 (2008)
Lenin, V.I.: Materialism and Empirio-Criticism; Critical Comments on a Reactionary Philosophy. Foreign Languages Publishing House, Moscow (1947)
Lopes, J.L.: Richard Feynman in Brazil: personal recollections. Quipu **7**, 383–397 (1990)
Lyre, H.: Aharonov-Bohm effect. In: Greenberger, D., Hentschel, K., Weinert, F. (eds.) Compendium of quantum physics: concepts, experiments, history and philosophy, pp. 1–3. Springer, Berlin (2009)
Margenau, H.: Measurement and quantum states—I & II. Philos. Sci. **30**, 1–16 (1963). 138–157
Mehra, J.: The Beat of a Different Drum : The Life and Science of Richard Feynman. Clarendon, Oxford (1994)
Messiah, A.: Quantum Mechanics. North Holland, Amsterdam (1961)
Moore, W.J.: Schrödinger : Life and Thought. Cambridge University Press, Cambridge [England] (1989)
Mullet, S.K.: Bohm, David Joseph. New Dictionary of Scientific Biography, pp. 321–326. N. Koertge, New York, Thomson - Gale. I (2008b)
Mullet, S.K.: Little Man: Four Junior Physicists and the Red Scare Experience. PhD Dissertation, Harvard University (2008a)
New research techniques in physics (1954). Rio de Janeiro and São Paulo, July 15–29, 1952
Olwell, R.: Physical Isolation and Marginalization in Physics - David Bohm's Cold War Exile. ISIS **90**, 738–756 (1999)
Ory, P., Sirinelli, J.-F.: Les Intellectuels en France de l'affaire Dreyfus à nos jours. Perrin, Paris (2004)
Osnaghi, S., Freitas, F., Freire Jr., O.: The origin of the Everettian heresy. Stud. Hist. Philos. Mod. Phys. **40**(2), 97–123 (2009)
Paty, M.: Sur les 'variables cachées' de la mécanique quantique—Albert Einstein, David Bohm et Louis de Broglie. La pensée **292**, 93–116 (1993)
Paty, M.: The nature of Einsteins objections to the copenhagen interpretation of quantum-mechanics. Found. Phys. **25**(1), 183–204 (1995)
Pauli, W.: Remarques sur le problème des paramètres cachés dans la mécanique quantique et sur la théorie de l'onde pilote. In: George, A. (ed.) Louis de Broglie – Physicien et Penseur, pp. 33–42. Editions Albin Michel, Paris (1953)
Pauli, W., Meyenn, K.V.: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u. a. Band IV Teil I 1950-1952. Springer, Berlin (1996)
Pauli, W., Meyenn, K.V.: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u. a. Band IV Teil II 1953-1954. Springer, Berlin (1999)
Peat, F.D.: Infinite Potential : The Life and Times of David Bohm. Addison Wesley, Reading, MA (1997)
Pechenkin, A.: The early statistical interpretations of quantum mechanics in the USA and USSR. Stud. Hist. Philos. Mod. Phys. **43**(1), 25–34 (2012)
Pechenkin, A.: Leonid Isaakovich Mandelstam: Research, Teaching, Life. Springer, New York (2013)
Peshkin, M., Tonomura, A.: The Aharonov-Bohm Effect. Springer, Berlin (1989)
Philippidis, C., Dewdney, C., Hiley, B.J.: Quantum interference and the quantum potential. Nuovo Cimento della Societa Italiana di Fisica B—Gen. Phys. Relat. Astron. Math. Phys. Methods **52** (1), 15–28 (1979)
Pinault, M.: Frédéric Joliot-Curie. O. Jacob, Paris (2000)

Pines, D.: A collective description of electron interactions. 4. Electron interaction in metals. Phys. Rev. **92**(3), 626–636 (1953)
Pines, D., Bohm, D.: A collective description of electron interactions. 2. Collective vs individual particle aspects of the interactions. Phys. Rev. **85**(2), 338–353 (1952)
Popper, K.R., Bartley, W.W.: Quantum Theory and the Schism in Physics. Rowan and Littlefield, Totowa, NJ (1982)
Rattner, H.: Tradição e mudança (a comunidade judaica em São Paulo). Atica, São Paulo (1977)
Reichenbach, H.: Philosophic Foundations of Quantum Mechanics. University of California Press, Berkeley, CA (1944)
Rodrigues, L.M.: O PCB: Os dirigentes e a organização. História geral da civilização brasileira, tomo III, vol. 3: O Brasil republicano—sociedade e política (1930-1964). B. Fausto. Rio de Janeiro, Bertrand Brasil, pp. 361–443 (1996)
Rosenfeld, L.: (1953) L'évidence de la complementarité. Louis de Broglie—physicien et penseur. A. George. Paris, Editions Albin Michel, pp. 43–65 [A slightly modified English version of this paper is Strife about complementarity, Science progress. **163**, 1393–1410 (1953), reprinted in Robert Cohen and John Stachel (eds.). Selected papers of Léon Rosenfeld (Dordrecht, D. Reidel, 1979)
Rosenfeld, L.: A filosofia da física atômica. Ciência e cultura **6**(2), 67–72 (1954)
Rosenfeld, L.: Physics and metaphysics. Nature **181**, 658 (1958)
Rosenfeld, L.: Heisenberg, physics and philosophy. Nature **186**, 830–831 (1960)
Rosenfeld, L.: Berkeley redivivus. Nature **228**, 479 (1970)
Rosenfeld, L.: Classical Statistical Mechanics. Livraria da Física & CBPF, São Paulo (2005)
Saidel, R.G., Plonski, G.A.: Shaping modern science and technology in Brazil. The contribution of refugees from national socialism after 1933. Leo Baeck Inst. Year Book **39**, 257–270 (1994)
Schatzman, E.: Physique quantique et realité. La pensée **42–43**, 107–122 (1953)
Schilpp, P.A., Einstein, A.: Albert Einstein, Philosopher-Scientist. Library of Living Philosophers, Evanston, IL (1949)
Schönberg, M.: On the hydrodynamical model of the quantum mechanics. Nuovo Cimento **XII**(1), 103–133 (1954)
Schönberg, M.: Quantum theory and geometry. In: Kockel, B., Macke, W., Papapetrou, A. (eds.) Max-Planck-Festschrift 1958, pp. 321–338. Deutscher Verlag der Wissenschaften, Berlin (1959) [Reprinted in M. Schönberg, Obra Científica de Mario Schönberg, São Paulo: EDUSP, 2013]
Schönberg, M., Hamburger, A.I.: Obra Científica de Mario Schönberg—Volume 1—1936–1948. EDUSP, São Paulo (2009)
Schönberg, M., Hamburger, A.I.: Obra Científica de Mario Schönberg—Volume 2—1949–1987. EDUSP, São Paulo (2013)
Schrecker, E.: No Ivory Tower : McCarthyism and the Universities. Oxford University Press, New York (1986)
Schrödinger, E.: The meaning of wave mechanics. In: George, A. (ed.) Louis de Broglie—physicien et penseur, pp. 16–32. Albin Michel, Paris (1953)
Schrödinger, E., Bitbol, M.: Physique quantique et représentation du monde introd. et notes par Michel Bitbol. Ed. du Seuil, Paris (1992)
Schweber, S.S.: QED and the Men Who Made It : Dyson, Feynman, Schwinger, and Tomonaga. Princeton University Press, Princeton, NJ (1994)
Schweber, S.S.: Feynman, Richard. In: Heilbron, J.L. (ed.) The Oxford Guide to the History of Physics and Astronomy, pp. 118–120. Oxford University Press, New York (2005)
Siegel, A., Wiener, N.: Theory of measurement in differential-space quantum theory. Phys. Rev. **101**(1), 429–432 (1956)
Stöltzner, M.: What John von Neumann thought of the Bohm interpretation. In: Greenberger, D., Reiter, W.L., Zeilinger, A. (eds.) Epistemological and Experimental Perspectives on Quantum Mechanics, pp. 257–262. Springer, Dordrecht (1999)

Stroke, H.H.: The Physical Review—The First Hundred Years: A Selection of Seminal Papers and Commentaries. American Institute of Physics, New York (1995)

Takabayasi, T.: On the formulation of quantum mechanics associated with classical pictures. Prog. Theor. Phys. **8**(2), 143–182 (1952)

Takabayasi, T.: Remarks on the formulation of quantum mechanics with classical pictures and on relations between linear scalar fields and hydrodynamical fields. Prog. Theor. Phys. **9**(3), 187–222 (1953)

Terletsky, I.P.: Problèmes du développement de la théorie quantique. Questions scientifiques—Physique, pp. 131–146. Les éditions de la nouvelle critique, Paris (1952)

Valentini, A.: Astrophysical and cosmological tests of quantum theory. J. Phys. A Math. Theor. **40** (12), 3285–3303 (2007)

Valentini, A.: Inflationary cosmology as a probe of primordial quantum mechanics. Phys. Rev. D **82**(6) (2010)

Vals, A.V.: Louis de Broglie et la diffusion de la mécanique quantique en France (1925-1960), PhD dissertation, Université Claude Bernard Lyon 1 (2012)

Vieira, C.L., Videira, A.A.P.: Carried by history: Cesar Lattes, nuclear emulsions, and the discovery of the Pi-meson. Phys. Perspect. **16**(1), 3–36 (2014)

Von Neumann, J.: Mathematical Foundations of Quantum Mechanics. Princeton University Press, Princeton, NJ (1955)

Wang, J.: American Science in an Age of Anxiety: Scientists, Anticommunism, and the Cold War. University of North Carolina Press, Chapel Hill, NC (1999)

Whyte, L.L.: The Scope of Quantum-Mechanics—Discussion. Br. J. Philos. Sci. **9**(34), 133–134 (1958)

Wiener, N., Siegel, A.: A new form for the statistical postulate of quantum mechanics. Phys. Rev. **91**(6), 1551–1560 (1953)

Wiener, N., Siegel, A.: The differential-space theory of quantum systems. Il Nuovo Cimento **2** (4 Suppl), 982–1003 (1955)

http://www.springer.com/978-3-662-44661-4

The Quantum Dissidents
Rebuilding the Foundations of Quantum Mechanics (1950-1990)
Freire Junior, O.
2015, XVI, 356 p. 26 illus., 14 illus. in color. With a Foreword by Silvan S. Schweber., Hardcover
ISBN: 978-3-662-44661-4