

Chris Talbot *Editor*

David Bohm's Critique of Modern Physics

Letters to Jeffrey Bub, 1966–1969

Foreword by Jeffrey Bub

 Springer

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Foreword

Beginning in October 1966, shortly after I began working as a post-doctoral research specialist in the Chemistry Department at the University of Minnesota, I began a correspondence with David Bohm that continued until August 1969, when I moved to New Haven to take up a position as Assistant Professor in the Physics and Philosophy Departments at Yale. It is, in some ways, a disturbing, even painful experience reading these letters again after so many years. One sees the gradual disintegration of our relationship as I began to develop my own ideas about the foundations of quantum mechanics. Bohm was something of a father figure for me, and there is probably an Oedipal element to my breaking the bond. I was Bohm's graduate student for three years at Birkbeck College, London University, from 1963 to 1965, and very much under the influence of his approach to physics in general and his ideas on the conceptual problems of quantum mechanics in particular at the beginning of the correspondence, less so at the end.

Unfortunately—or perhaps fortunately for the reader!—I did not keep my own letters to Bohm. But it's not hard to reconstruct the broad outlines from Bohm's often lengthy replies and the useful explanatory footnotes by Chris Talbot. I hope these comments provide some additional context.

When I arrived as a student at Birkbeck, Bohm was going through a book by Hodge¹ on harmonic integrals in his seminar, exploring ideas from algebraic topology as a new language for physics. I understood very little of the discussion and, since it was up to me to find a dissertation topic, I spent a lot of time exploring the stacks at the Senate House Library, a short walk from Birkbeck on Malet Street in Bloomsbury. There I discovered the measurement problem of quantum mechanics in articles by Henry Margenau² and decided that this was what I wanted to work on. Following Bohm's suggestion, I looked at a paper by Wiener and Siegel³ on a 'differential-space' theory of quantum mechanics, which eventually led to my dissertation, parts of which appeared as jointly authored papers in

¹[Hodge, 1959].

²[Margenau, 1963a, Margenau, 1963b, Margenau, 1958].

³[Wiener and Siegel, 1955].

Reviews of Modern Physics in 1966.⁴ The core idea, which Bohm proposed, was to modify the unitary dynamics of quantum mechanics by adding a nonlinear term that produced the notorious ‘collapse’ of the quantum state in a measurement to an outcome determined by additional ‘hidden variables.’ As such, the theory was an early ‘dynamical collapse’ theory, a somewhat primitive precursor of the later Ghirardi-Rimini-Weber theory⁵ and its variants, very different from Bohm’s 1952 hidden variable theory. Surprisingly, Bohm never talked to me about the 1952 theory during my three years at Birkbeck, and there are only some brief references to the theory in the correspondence.

During my first year at the University of Minnesota, I worked with Alden Mead, a physical chemist who was interested in applying the idea of a fundamental length and time to the Mössbauer effect. Mead spent a sabbatical year at Birkbeck during my last year there and offered me a position as his assistant, largely on the strength of my derivation of the Born probabilities in the collapse theory. For Bohm, it was a mathematical detail he left to me, but it took me months to come up with an acceptable proof. Bohm’s method of working through new ideas, which he produced with astonishing frequency, was entirely informal. He had a remarkable physical intuition and was dismissive of mathematical proof, which he felt obscured rather than illuminated informal thinking—a recurring theme in the letters. As I recall, he was never wrong.

After a year in the Chemistry Department, I moved across the mall to Ford Hall, where Herbert Feigl directed the Minnesota Centre for Philosophy of Science. Feigl arranged a two-year position for me as a research associate, presumably a step up since it came with a slightly increased salary. At the Centre, I met Bill Demopoulos and we began a long collaboration that continued until his death in 2017. Demopoulos was interested in Whitehead’s process philosophy,⁶ in which process and relations are primary, rather than objects with properties, and we saw echoes of Whitehead in Bohm’s concept of ‘implicate order’ and his emphasis on relations, orders, and structures as relevant for physics. Our position shifted radically after Hilary Putnam visited the Centre and argued that the failure of the distributive law of classical logic was the source of conceptual puzzles in the two-slit experiment and other quantum phenomena. We began to move away from the ideas of Bohm and Whitehead and came to see quantum logic as the core idea in deflating the puzzles of quantum mechanics. Around that time we discovered the more sophisticated work of Kochen and Specker⁷ in which the crucial non-classical feature of quantum mechanics is the impossibility of embedding the non-Boolean logical structure of quantum mechanics into a Boolean algebra.

Influenced by Kochen and Specker, my new way of thinking about the foundations of quantum mechanics was quite at odds with Bohm’s approach, and as the correspondence continues one sees Bohm becoming increasingly frustrated, even

⁴[Bohm and Bub, 1966a, Bohm and Bub, 1966b, Bohm and Bub, 1968].

⁵[Ghirardi et. al., 1986].

⁶[Whitehead, 1978].

⁷[Kochen and Specker, 1967].

exasperated, with the lack of communication between us. In the postscript to the long letter of January 18, 1969 (p. 258, C136), he complains:

What struck me is that you tacitly dismissed all this work, which represents my deepest feelings on the subject, as v. Neumann and his fellow mathematicians tacitly dismissed Bohr as “inessential” when developing their formalisms which were aimed at capturing what was essential in the informal situation regarding quantum theory. If you had at least discussed the papers carefully and shown why you regard them as irrelevant, this would have made sense.... I understand that your whole background has been such as to make you believe that changes in the informal language can only be arbitrary. With this background, it was natural and inevitable that you would dismiss these papers as irrelevant. But how are we going to communicate if you regard my deepest thoughts and feelings as arbitrary? Is it not necessary to begin by entertaining these thoughts, if your intention is communication?

On January 20, 1969 (p. 249, C136) he laments:

I see again and again that our relevance judgements, are in certain ways, very different. It distresses me to see how far we are from communicating, even though we worked together and discussed so much together. How much further from communication I must be from others, with whom I never even had any common basis!

and a month later he ends the letter dated February 14, 1969 (p. 276, C136):

My advice to you with regard to the measurement problem is very simple. It is just: “Drop it”.

In the letters, there is a lot on measurement in quantum mechanics, the Daneri-Loinger-Prosperi theory I was working on, perception and the artist Charles Biederman (whom Bohm urged me to visit in Red Wing, Minnesota), Feyerabend and Popper, Bohr and the Copenhagen interpretation, Bohr’s insistence on classical language for communication, wholeness, the difference between Bohr and Heisenberg, between Bohr and von Neumann, and throughout it all Bohm’s attempt to get me to see the relevance of the informal as creative and my preoccupation with von Neumann and Kochen and Specker as a retreat inhibiting real creativity. For Bohm, Bohr’s ideas were relevant but flawed, and Bohr’s crucial insight was the ‘wholeness’ of the quantum description, while I had come to see ‘classical’ as ‘Boolean,’ and the quantum revolution as the transition to a non-Boolean theory. But quite apart from our intellectual differences, there was no way I could function in an academic environment as a Bohm clone.

The letters end in August 1969, when I moved to Yale. I got the job on the recommendation of Paul Feyerabend, who was at Yale briefly during the academic year 1969–1970 before returning to Berkeley. Feyerabend invited me to Berkeley after I sent him comments on a draft of his two-part critique of Popper on quantum mechanics ⁸ that Feigl showed me, and we continued to correspond afterwards. I saw Bohm occasionally and he visited me in Israel when I had a position at Tel Aviv University, but we never renewed our correspondence.

⁸[Feyerabend, 1968, Feyerabend, 1969].

Bohm's letters were handwritten and his handwriting is not easy to read. Chris Talbot has performed the Herculean task of transcribing and editing the correspondence. The letters should be a useful resource for philosophers of physics and historians of physics interested in Bohm's approach to the foundations of physics, but also for readers who simply want to understand what Bohm had to say on a range of topics, from physics to religion.

Jeffrey Bub
University of Maryland, USA

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My special gratitude goes to Prof. Jeffrey Bub of the University of Maryland, for writing the most valuable foreword to the book. It is not always easy to go over events in our distant past, but Jeff willingly provided material, information and advice which considerably enhanced the contents of this book. He also spotted a number of mistakes.

Thanks are also due to the historian of science Olival Freire Junior who, as he explains in his recent biography, has been working on Bohm history for three decades. Olival kindly read through my introduction, finding a serious omission and also noting mistakes. As usual though, the final responsibility for the introduction and the accuracy of the transcriptions and editing rests with the editor.

After moving to England, David Bohm developed an interest in art and his wife Saral was a keen amateur painter. It is therefore fitting that Australian artist Betty Davis of Alice Springs kindly allowed me to use a copy of her painting of David Bohm for the front cover.

Much of my time in recent years has been spent following up books and publications related to David Bohm and wider issues in physics and philosophy using the tremendous resources of the Bodleian Library, University of Oxford. Isabel McMann and the staff of the Radcliffe Science Library section of the Bodleian deserve particular thanks for their help and for providing an ideal environment in which to work. Sadly the RSL has recently been closed and moved to temporary premises. Hopefully it and the invaluable staff team will be soon be moved to more suitable accommodation.

I am fortunate in having the support of a loving family of three children, their partners and four grandchildren, to whom I express enormous gratitude. Many hours of sometimes tedious work in the preparation of the material in this book has only been possible through the support and encouragement of my wife, Ann. As a historian, she has a genuine appreciation of the importance of making these letters of David Bohm available.

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Chapter 1

Introduction



I was inspired to transcribe and edit this collection of letters from David Bohm to Jeffrey Bub after reading drafts of Olival Freire Junior's recent biography of David Bohm.¹ Freire's book is an excellent contribution to the history of 20th century science. In writing it he used some material from these letters, struggling with Bohm's handwriting, to comment on the late 1960s. I was also pleased that Freire was able to use the transcriptions I had made of the Bohm correspondence for the 1950s period in Brazil and Israel.² I made some suggestions on Bohm's ideas which Olival was kind enough to take seriously.

There is a small but continuing interest in Bohm-related physics, including the "Implicate Order" approach which has been championed by Basil Hiley, Bohm's colleague for more than 30 years. Since Bohm's death in 1992, Hiley has produced more than 100 publications developing theoretical physics from his and Bohm's standpoint, rebutting numerous attempts to refute or dismiss it from the position of quantum mechanical "orthodoxy". The recently published "Emergent Quantum Mechanics, David Bohm Centennial Perspectives"³ exemplifies this continuing interest.⁴

As well as Freire's biography, we already have the first biography of Bohm by his friend David Peat,⁵ written a little after Bohm's death, which, although containing less of the science, does give a colourful account of Bohm's life. In addition, giving some explanation of Bohm's involvement with the Indian thinker and teacher Jiddu Krishnamurti and his organisation, beginning in 1961 and continuing even after

¹Freire Jr. (2019).

²Talbot (2017).

³<https://www.mdpi.com/books/pdfview/book/1203>.

⁴Note that Basil Hiley, now in his eighties, is joint author of no less than four papers in this collection.

⁵Peat (1996).

Krishnamurti's death in 1986, we have the recent informative book by David Edmund Moody.⁶ Also most of the large number of books and articles written by Bohm, both scientific and philosophical as well as those relating to Krishnamurti are available in a good university library. Given all of this material, why is yet another book needed? This introduction is mainly an attempt to answer this question.

First me explain something about the material that is contained in this book. Bohm's correspondence with Jeffrey Bub is in the archives at Birkbeck College, University of London, in eight folders, numbered C130–C137, so I have kept them in the same order in the chapters here, headed with the same numbers. Altogether there are 594 pages in the original, mostly in Bohm's handwriting but a few are typewritten. Unfortunately Jeffrey Bub's replies were not preserved though Bohm often makes clear the point that Bub was making.

Since this correspondence the approach to physics followed by Jeffrey Bub and David Bohm diverged, as Bub explains in the foreword he has kindly written. I should give a brief outline of Jeffrey Bub's distinguished career for those not familiar with the world of modern physics. He received his PhD in mathematical physics from London University in 1966, where he studied physics with David Bohm at Birkbeck College and philosophy of science with Karl Popper and Imre Lakatos at the London School of Economics. He has published numerous articles in scientific and scholarly journals on the conceptual foundations of quantum mechanics and is the author of several books: *The Interpretation of Quantum Mechanics* (1974), *Interpreting the Quantum World* (1997), which won the prestigious Lakatos Award in 1998, *Bananaworld: Quantum Mechanics for Primates* (2016; revised paperback edition, 2018), and *Totally Random: Why Nobody Understands Quantum Mechanics* (2018; co-authored with Tanya Bub).⁷ The latter, "An eccentric comic about the central mystery of quantum mechanics" is particularly recommended for non experts. Bub is currently a Distinguished University Professor Emeritus at the University of Maryland, College Park.

Jeffrey Bub was most helpful in providing additional material from his personal archives which I have added in the first three appendices. The first contains correspondence with the Italian physicist Angelo Loinger which relates to folder C130, Chap. 2, of the Bohm–Bub correspondence; the second is a letter from Bub to David Bohm relating to a paper by well-known physicist Leon Rosenfeld, also relating to C130 and the third contains details of the "game" referred to at the beginning of C131, Chap. 3. In addition there are appendices containing two previously unpublished articles written jointly by David Bohm and his postdoc researcher Donald Schumacher. They have been added because of their relevance to much of the correspondence.⁸

Olival Freire, quite correctly for a historian of science, concentrates in his biography on Bohm's scientific work and his relationship to scientists of his day, putting this

⁶Moody (2016).

⁷Bub (1974, 1997, 2016), Bub and Bub (2018).

⁸They are in the Birkbeck archives as B44 and B88. For more on Schumacher, including his severe mental illness which prevented him from continuing to work with Bohm, see Freire Jr. (2019), pp. 141–148 and Peat (1996), pp. 246–253.

into the context of the times and against the background of his philosophical ideas. However Bohm saw himself primarily as a philosophical thinker, taking philosophy in the broadest sense and not in the limited tradition of academic philosophy. One can see Bohm giving primary importance to metaphysics in the correspondence.⁹ Referring to the Bellagio Conference in Theoretical Biology,¹⁰ Bohm observes the way in which contributors talked at cross-purposes because of differences in their “deep tacit assumptions”. “At a certain point, I plunged in and presented my own metaphysical notions. In doing this, I explained that each person inevitably has some kind of metaphysics, which is just a set of general and basic assumptions about reality as a whole.” (Bohm was apparently pleased at the response to his intervention, reporting, perhaps somewhat naively, that everyone at the conference came out into the open with their own metaphysics.) I think this does leave room for an alternative approach, attempting to find the origins of Bohm’s philosophical ideas, how he developed them, and how they relate to twentieth century thought. Perhaps one could say that this approach is more from the standpoint of the history of ideas, rather than from the history of science.

My first steps in this direction were an attempt to gain some understanding of “Causality and Chance in Modern Physics.”¹¹ David Peat gives some explanation and made some references to Bohm’s correspondence from Brazil and Israel, but it seemed to me that only by transcribing and editing all the correspondence could we gain a fuller understanding of the provenance of “Causality and Chance”. I attempted to summarise Bohm’s version of Marxist philosophy that the letters reveal.¹²

Moving into the 1960s we are presented with a far bigger task. Bohm’s publications in this period discussing philosophical issues throw up difficult questions concerning what influences are at work. We consider this further below. But as with the 1950s we can hope to get a clearer understanding if all the important unpublished material in the Birkbeck archives is made available. Much of this is in Bohm’s handwriting which is difficult and time-consuming to read. The publication here of the Bohm–Bub correspondence is a part of this project. A much bigger project, still ongoing, is the correspondence between Bohm and the artist Charles Biederman. Only the first letters of this correspondence, from March 6, 1960 to April 24, 1962 were edited and transcribed by the Finnish philosopher Paavo Pyykkänen.¹³ In the Birkbeck archives (Folders C67–C92) there are letters continuing up to October, 1969 (3202 pages of letters written by Bohm and 674 pages of replies by Biederman). Note that in seven of the Bohm–Bub letters there is a request that Bub sends them on

⁹C132, pp. 147–148.

¹⁰The 2nd Symposium on Theoretical Biology, held August 3–12, 1967. The proceedings are published in Waddington (1969). Bohm’s contributions were “Some remarks on the Notion of Order” (pp. 18–40), “Further Remarks on Order” (pp. 41–60), “Addendum on Order and Neo-Darwinism” (pp. 90–92), “Some Comments on Maynard-Smith’s Contribution” (pp. 98–105).

¹¹Bohm (1957). See also the important aftermath of this book in the Colston conference, Kožnjak (2017).

¹²Talbot (2017), especially pp. 23–37.

¹³Pykkänen (1999).

to Biederman.¹⁴ There are also a number of shorter collections of correspondence such as the letters to Bohm's brother-in-law, Yitzhak Woolfson. Some of these are published,¹⁵ including an important introduction by Woolfson.

The importance of Bohm's correspondence can perhaps be emphasised by making parallels with the case of Wolfgang Pauli and Carl Gustav Jung.¹⁶ I am not, of course, suggesting a direct comparison between Pauli, who belonged to an older generation of physicists, and Bohm and certainly not between the religion of Krishnamurti and the analytical psychology of Jung.¹⁷

Pauli is regarded as one of the foremost proponents of the "positivist" approach to orthodox quantum mechanics that was developed in the late 1920s. (The idea that there was a unified approach ignores the many differences between the leading figures, something which Bohm was well aware of as this correspondence with Bub shows, and which has been made clear in the work of Mara Beller Beller 1999.) It is remarkable that Pauli made a "metaphysical" turn from positivism to studying mysticism after 1930, to which his correspondence with Jung relates. Miller in his authoritative study comments that "Pauli told very few colleagues about his discussions with Jung. He feared their derision. Nevertheless his sessions with Jung convinced him that intuition rather than logical thought held the key to understanding the world around us."¹⁸ Thus little was known about Pauli's ideas, even after his death in 1953, though Bohm apparently knew something about Pauli's interest in Jung,¹⁹ (Bohm considers Jung's ideas in the correspondence²⁰ Ironically, considering that Pauli opposed Bohm's support for determinism in 1951, Bohm's essential objection to Jung is that his theory is "too crude and mechanical to account for perception, especially its more creative aspects.")

Without the Pauli–Jung correspondence and Pauli's correspondence as a whole becoming available to researchers, the studies of Miller and the others noted above, all of which are relatively recent, would not have been possible. Pauli's complete correspondence has been edited and published by Karl von Meyenn. It contains more

¹⁴These are C131: Jan 7, pp. 63–74, Feb 22, pp. 74–80, May 30, pp. 89–92, June 1, pp. 92–95 and June 2, pp. 95–98 and C133: Oct 20, pp. 172–177. All are in 1967. Note that there references to Biederman's ideas in C130 and especially C131.

¹⁵Nichol (2002), pp. 199–234.

¹⁶Lindorff (2004), Gieser (2005), Miller (2009) and Atmanspacher and Primas (2009).

¹⁷It was Pauli who, after Bohm replied to a series of objections, finally accepted that Bohm's two famous 1951 "hidden variable" papers were a valid alternative to standard quantum mechanics and sanctioned publication in *Physical Review*. Freire gives the details (Freire Jr. 2019, pp. 71–74) and also points out that Pauli, in a letter to Markus Fierz, "raised the stakes on philosophical grounds criticizing the expectations of recovery of determinism in physics. 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." (pp. 82–3).

¹⁸Miller (2009).

¹⁹C131, p. 80.

²⁰C131, pp. 80–89.

than 7000 pages in eight volumes, published between 1979 and 2005.²¹ As in Pauli's case, making available Bohm's correspondence can hopefully encourage a deeper understanding of his ideas, which, like those of Pauli, are unusual considering the widespread disdain shown towards "metaphysics" in the environment of modern science.

Consider now Bohm's published material in the 1960s relating to philosophy and the philosophy of science, as opposed to publications directly relating to physics. In the early 1960s there are a few of Bohm's papers within the "official" philosophy of science tradition.²² Note that in the first paper cited Bohm writes that "guided by different conceptions, one is led to seek different types of facts, some of which may be possible in a given field, and some not." He also stresses that "facts are made". These are ideas that can be seen in the Bohm–Bub letters.²³ The second, in a collection dedicated to Karl Popper, has a discussion of "understanding" which may be considered to be an earlier version of "creativity."

Some insight can be gained by comments from the philosopher of science Paul Feyerabend. In a letter to David Peat in 1993, answering questions on his experiences of working with Bohm at Bristol University in the late 1950s, Paul Feyerabend recalled that Bohm had discussed Gilbert Ryle, Ludwig Wittgenstein and Karl Popper:

Dave who knew about all of them remarked that Popper relied on logic which was OK, but too rigid to aid scientific research and that 'ordinary language philosophy' was not really using ordinary language but an artificial lingo which was also too rigid. He agreed that there could be 'category mistakes' in the sense that predicates could be applied to inappropriate subjects (such as calling the number two blue) but that regarding category mistakes as boundaries to talk as Ryle suggested, was going too far: scientific research often went right through such 'mistakes'. Dave's knowledge of Ryle and Wittgenstein was not very detailed but he had an amazing ability of getting the whole picture from a few hints. After that he criticized the whole picture, not just a few embroideries here and there as is the custom of many philosophers.²⁴

However, after moving to Birkbeck, University of London, Bohm begins to publish his own distinctive philosophical ideas. These are to be found in (1) the inaugural lecture given at Birkbeck (1963),²⁵ (2) the contribution given to a physics conference in Kyoto (1965),²⁶ (3) the Appendix to the Special Theory of Relativity (1965),²⁷ (4) contributions given to the Bellagio conference on theoretical biology (1967)²⁸ and (5) "On Creativity" (1968).²⁹ Of these (2), (3) and (5) are readily available.

²¹ Atmanspacher and Primas (2009), p. 3, n 3. An English translation of the Pauli–Jung letters is Meier (2001).

²² Such as Bohm (1961) and Bohm (1964).

²³ Especially in C132.

²⁴ Letter to Peat dated 07/09/93, Birkbeck archives folder A21.

²⁵ Bohm (1963).

²⁶ Bohm (1965), also <http://www5.bbk.ac.uk/lib/archive/bohm/BOHMB.149.pdf>.

²⁷ Bohm (2006).

²⁸ See p. 3, n 10 above.

²⁹ Bohm (1968) also Nichol (1998), pp. 1–18, or <http://classes.dma.ucla.edu/Fall07/9-1/pdfs/week1/OnCreativity.pdf>.

Between them the five publications present the concepts of *order*, *structure* and *process*, *perception* and *creativity* that characterise Bohm's philosophical views in this period.³⁰

Much of (1) is summarised in the first section of the more readily available paper (2). In the appendix to (1) we find an important comment on "process". Bohm wants to begin with "process", "the assumption that what is is movement itself." How does one then explain things which are at rest? "Such an explanation is carried out in terms of the notion of invariant repetitive, ordered and structured relationships that hold only relative to certain conditions, at certain levels, within specific contexts, and to limited degrees of approximation." Here "order" and "structure" are referring back to the discussion in the main text, on relativity, quantum theory and the latest developments in particle physics. The approach to process is reminiscent of "Causality and Chance", especially Chap. 5, but the latter is not referred to.

Reading (2) we see the basic definitions of "order", "structure" and a good deal on "function", especially "reflective function." Order is explained as being based on "similar differences" leading to "different similarities" with examples from geometrical curves and wave motion in physics. Structure is defined as "order of orders". Bohm clearly considers that these are concepts that are known to everyone from daily experience, so it is a matter of making them more precise. But note that he thinks they can be extended to "all of our perception, thinking, feeling and action", quite a wide generalisation. However in the long discussion on "function" he makes the qualification of "abstracting" from a "limited domain", and opposes ideas of "absolute and final truths"—again reminiscent of "Causality and Chance". The idea of "creative process" is briefly referred to but is expanded on in (4) and (5). "Reflective function" is clearly a key idea for Bohm, it is primarily "ontological" and is developed in the pansychism evident in the first half of the Bohm–Bub correspondence. This paper seems to have been central to the work of the research group at Birkbeck.³¹

In (3), another widely available publication from the 1960s, Bohm summarises his study of Piaget on child development and the latest work by psychologists such as James J. Gibson. Note that Bohm cites J. R. Platt, Professor of Physics and Biophysics at the University of Chicago with whom Bohm had over 100 pages of correspondence in 1963, also unpublished (Birkbeck archives C51–C54). It is clear that though the psychology of perception is the area of investigation, "perception" is also being regarded as a philosophical concept. Already in the preface, Bohm has stated that "science is mainly a way of extending our perceptual contact with the world, rather than of accumulating knowledge about it."³² Then extending the ideas of (2) we read that "in the process of perception we learn about the world mainly by being sensitive to what is invariant in the relationships between our own movements, activities,

³⁰I have omitted Bohm (1962) as this is concerned with a topological approach in physics, and also "On the Relationships of Science and Art" (1968) in Nichol (1998), pp. 19–28, which would take us too far afield.

³¹See, for example Bub (1969) and many later publications of Hiley, for example Hiley (2011), also <https://arxiv.org/abs/1211.2107>.

³²Bohm (2006), p. x.

probings, etc., and the resulting changes in what comes in through our sense organs,” and that “the invariant is finally understood with the aid of various hypotheses, expressed in terms of higher levels of abstraction, which serve as a kind of “map,” having an order, pattern, and structure similar to that of what is being observed.”³³ As with (2) we find these ideas being extended in the correspondence with Bub.

At the Bellagio conference (4) we have already referred to Bohm’s stress on the importance of metaphysics. Bohm goes over the material on order, structure and process,³⁴ stressing that “the notion of order is evidently more fundamental than other notions, such as, for example, that of relationships and classes, which is now generally regarded as basic in mathematics.” Everyone has some “tacit” knowledge of order so “with words we can ‘point to’ certain essential features of this tacit knowledge.” Which he does, repeating the exposition of “similar differences” and “different similarities”. Note that he makes an extension of the “difference” conception to “constitutive differences” (determining the essence of what we are talking about, such as chords in the curves example) and “distinctive differences” (defining the relation between one order and another, such as between the chords of different curves). This extended definition is noted in the correspondence.³⁵

Bohm stresses that order and structure will not be static but in a process. What is essential to process is “not merely that there is a change of order and structure, but that the differences are similar, so that the changes themselves are ordered”. Process is thus an order of change. In biology Bohm considers that there are evolutionary processes with “the coming into being of new orders, along with an ordering of the changes of order in the whole process”.³⁶ Previously he suggested that “the breaks or changes in order of a given process can themselves be the basis of a higher order of process.”

Bohm considers the mechanistic view in which the constitutive order of the universe is that of fundamental particles moving in some kind of mechanical motion. His position is that natural processes can “contain a really creative movement, in which there appear new orders and orders of orders.”³⁷ But while he considers there has been a tendency in physics to move away from a mechanistic view (he takes up several pages on statistical mechanics, quantum theory and quantum field theory) he is concerned that biology and psychology are moving closer to it.³⁸

Apart from a discussion with biologist John Maynard Smith on neo-Darwinism³⁹ which needn’t concern us here, note that in the section “Further remarks on order”, especially “On the self-regulating hierarchy of process” and “On the separation of

³³Bohm (2006), pp. 164 and 169.

³⁴Waddington (1969), pp. 19–25. See p. 3, n 10 above.

³⁵C132, pp. 115–120.

³⁶Waddington (1969), pp. 25–26.

³⁷Ibid, p. 28.

³⁸Ibid, p. 34.

³⁹Ibid, pp. 90–94.

the observer and the observed”⁴⁰ there is also material that occurs in the Bohm–Bub correspondence.

“On Creativity”⁴¹ (5) integrates, if in a briefer and more popular form, much of the material in (1)–(4). Although perhaps it could be considered as primarily a paper on psychology that is misleading. In fact as noted above, perception, even “creative perception”, is a philosophical concept for Bohm. The concepts of order and structure are explained again but note that a distinction is being made between perception which is mechanical where “the order, pattern and structure of what is perceived come from the record of past experiences and thinking” and creative perception. In the latter “one first becomes aware (generally non-verbally) of a new set of relevant differences, and one begins to feel out or otherwise to note a new set of similarities, which do not come *merely* from past knowledge, either in the same field or in a different field. This leads to a new order, which then gives rise to a hierarchy of new orders, that constitutes a set of new kinds of structure.” It is not difficult to see Krishnamurti’s idea of mechanical thought that he contrasts with “choiceless awareness.”⁴² Since “On Creativity” is readily available and relatively straightforward to read no more comment is needed.

The five publications I have considered could be supplemented by material from the Cambridge conference, held in July, 1968⁴³ and the Illinois symposium, held in March 1969.⁴⁴ However these two more recent publications are somewhat different in that Bohm has made a shift in his views to accommodate “communication”, following his discussions with Schumacher⁴⁵ Also Bohm’s ideas on Niels Bohr and quantum theory at Cambridge are part of a discussion that is ongoing throughout the correspondence, some of it quite technical, that would require much more consideration than we can give here.⁴⁶

For Bohm’s “Perception-Communication” view of the nature of science at Illinois, the material in the correspondence⁴⁷ taken together with the joint papers with Schumacher given in the Appendices should help give a clearer understanding of Bohm’s standpoint. The connection of Bohm’s ideas to Feyerabend’s “pluralism” can be understood further by noting Bohm’s sympathy with Feyerabend in earlier

⁴⁰Ibid, pp. 51–59.

⁴¹Noted by Bohm in C131, p. 83.

⁴²See Moody (2016), especially Chap. 5.

⁴³C133, p. 184, n 6 and C136, p. 250, n 1.

⁴⁴C136, p. 263, n 6 and C137, p. 301, n 1.

⁴⁵No records are kept at Birkbeck and the preprints given in Appendices D and E are undated, but discussions with Schumacher are referred to from November 1967 (C133, p. 191) to February 1968 (C134, pp. 213–219).

⁴⁶But note Freire Jr. (2019), p. 150.

⁴⁷See C136, pp. 287–289, 291–296 and 298, C137 pp. 301–306 and 319–320.

letters.⁴⁸ Such an exposition cannot be given in this brief introduction and must be left to later work.⁴⁹

Returning to consider the brief review of Bohm's philosophical material published in the 1960s, I have indicated that a number of key ideas also occur in the Bohm–Bub correspondence. I would suggest further that there is a wealth of material, especially in C130–C133 which expands on and, hopefully, helps to clarify the documents (1)–(5). Of course it is unfinished, not everything is clear, and there are ideas going in a highly speculative, Krishnamurtian direction. Bohm writes in a rather unstructured manner, sometimes repetitive, often using several words together to try to clarify or stress a point, even making up new words. Overall though, the correspondence should help to clarify the ideas of “structure-process” behind Bohm's (and Hiley's) view of the “Implicate Order” developed in the decades since. Also Bohm can be seen developing a type of panpsychism, “neither materialism nor idealism”,⁵⁰ “the observer is the observed”,⁵¹ based on perception but with a definite ontology of structure-process, which again sheds some light on his later work.

However, even if the Bohm–Bub correspondence is of considerable help, there are still questions remaining for the student of Bohm's philosophical views in the 1960s. Even allowing for Bohm's originality and creativity,⁵² the influences at work in his views remain something of a mystery. In (3) there are references to work on the psychology of perception and in (4) references to biologists and even a reference back to (2). But no other references and certainly no philosophical references are given in (1)–(5), not even to Bohm's own “Causality and Chance.”

It seems that Biederman played a major role in the formation of Bohm's ideas on order and structure as well as creativity. In the correspondence he discusses Biederman's views on art, especially in relation to “creating new orders”.⁵³ Surprisingly Bohm states that although he got some of his ideas from Whitehead,⁵⁴ his correspondence with Charles Biederman was a lot more important.⁵⁵ Some of this can be seen in the letters published by Pykkänen. Bohm introduces the Hegelian concepts of identity and difference⁵⁶ but Biederman objects to the term “identity”, preferring “similarity”. There is a discussion lasting over a year with Bohm eventually agreeing to replace “sameness” with “similarity”, or as Pykkänen puts it “to drop “identity” from their set of concepts, as long as certain conditions are accepted.”⁵⁷ But there is

⁴⁸C132, pp. 148–149, C133, pp. 160–161, pp. 167–168 and p. 191. Bohm is reading and referring to Feyerabend (1965).

⁴⁹Note however Freire Jr. (2019), pp. 148–150.

⁵⁰C130, pp. 36–41.

⁵¹C132, pp. 104–112.

⁵²In C131, p. 86, Bohm writes, “you cannot do valid work on creativity without yourself being in the creative state about which you wish to talk.”

⁵³C131, pp. 89–98.

⁵⁴To our knowledge Bohm does not comment on Whitehead in the 1950s and 60s period.

⁵⁵C131, p. 92.

⁵⁶Pykkänen (1999), p. 109.

⁵⁷Ibid, p. 244. Pykkänen's chapter summaries are very useful here.

no explicit mention of “order” or “structure”. Note however that by the end of 1962, Bohm is writing to Yitzhak Woolfson on the idea of “structure-process”. He writes: “Each structure has a kind of order, a set of sequences of elements that are naturally most immediately related, as well as breaks as variations in this order,” and “The problem of structure is basic to my work in physics. In essence, I am trying to find the general principle of the process-structure that can abstract as time-space.”⁵⁸

Krishnamurti features throughout the Bohm–Biederman letters and clearly is a major influence on Bohm’s thinking from 1962 onwards. Biederman derived ideas from the Polish-American philosopher, Alfred Korzybski,⁵⁹ of whom Bohm was quite critical. There are increasing differences of opinion regarding A.K. (Korzybski) and J.K. (Jiddu Krishnamurti) throughout the letters. Hopefully the Bohm–Biederman correspondence will help to explain Biederman’s influence but will also tell us much more about Bohm’s obvious enthusiasm for Krishnamurti in that period.

Another influence on Bohm is the 19th century philosopher G. W. F. Hegel. Bohm kept returning to a study of Hegel all his life, as reported in the 1986 interviews with his friend Maurice Wilkins where far more consideration is given to Hegel’s ideas than to Krishnamurti and Biederman (though Bohm had clearly become disillusioned with Krishnamurti by then). Hegelian views—or rather the Marxist interpretation of Hegel—are central to “Causality and Chance” as I attempted to show.⁶⁰ The interviews with Wilkins certainly show Bohm’s facility with Hegelian concepts.⁶¹ An important aspect of Hegel’s philosophy is the conception of the world and of thought as a process.⁶² We may assume that Bohm’s commitment to process philosophy comes from this background.

The dialectical opposites that are central to Hegelian philosophy were apparently frequently used by Bohm and appear in the Bohm–Bub correspondence (such as Necessity and Contingency, Form and Content, etc.). As if in a revelation that other people, especially physicists, do not have his familiarity with Hegel, Bohm notes that Bub regards “contingency and necessity as incredibly complex notions” compared to the von Neumann-style axiomatic approach to quantum theory, which Bub regards, to Bohm’s chagrin, as “manageably simple”.⁶³

It is presumably because Hegel, Biederman and Krishnamurti are such major influences that Bohm gives no references or discussion of what influenced his ideas. We could assume that Bohm does not refer to Hegel and Marx because he wanted to

⁵⁸Nichol (2002), p. 218 and p. 219.

⁵⁹Korzybski’s ideas, known as “general semantics”, published in “Science and Sanity”, Korzybski (1994) enjoyed some interest in the 1930s and 40s but are now rarely mentioned. A few references are given in the Bohm–Bub correspondence: C130, p. 48, C133, pp. 188–189, pp. 193–198.

⁶⁰Note that in the 1993 letter to Peat, Feyerabend writes that at Bristol Bohm either “read Hegel’s logic, or had just read it and like Lenin interpreted it materialistically.”

⁶¹Wilkins (1986), parts VII, IX, X, and XI.

⁶²See, for example, Beiser (2005).

⁶³C136, p. 274.

distance himself his pro-communist past.⁶⁴ But also one should not forget the general antipathy towards Hegel amongst philosophers and historians of science.⁶⁵

According to David Peat⁶⁶ Bohm's discussions with Krishnamurti "caused considerable consternation among his former colleagues in the United States," so that despite his enthusiasm for ideas that he thought held the key to understanding the world, Bohm became quite guarded on the subject. There was something of a change after 1980. As Freire notes, although the 1980 UK edition of "Wholeness and the Implicate Order"⁶⁷ contained no mention of Krishnamurti, later editions did.⁶⁸ Bohm then held a number of dialogues with Krishnamurti. The first, "The Ending of Time" was held in 1980, and published in 1985.⁶⁹ The reluctance to go public with the Bohm–Krishnamurti relationship seems to be not just Bohm's responsibility but also came from within the Krishnamurti organisation. As Moody explains, an earlier series of dialogues between Bohm and Krishnamurti held in 1975 was blocked from publication in 1977 by Mary Lutyens, Krishnamurti's official biographer.⁷⁰ There was something of a conflict between Bohm and Krishnamurti at that time, revealed in Bohm's correspondence with Fritz Willhelm. This correspondence, though in the Birkbeck archives, has also not been published. Willhelm was a physicist who worked for the Krishnamurti Foundation in the late 1970s. Conflicting interpretations of the seriousness of the conflict are given by David Peat⁷¹ (anti-Krishnamurti) and David Moody⁷² (pro-Krishnamurti).

Perhaps Bohm also thought that suggesting some of his views on the philosophy of physics were influenced by Biederman, an artist, would not be well received. If so he changed his mind by 1971, including a footnote, "This notion of order was first suggested to the author in a private communication by a well-known artist, C. Biederman," with a reference to Biederman's, "Art as the Evolution of Visual Knowledge"⁷³ in the first volume of the journal *Foundations in Physics*.⁷⁴

Having explained some of the remaining difficulties of understanding Bohm's philosophical ideas in the 1960s, I now turn to two recent developments that could perhaps help to revive some interest in Bohm's ideas today.

⁶⁴See Freire Jr. (2019), pp. 105–107.

⁶⁵This applies to all of the "Romantic" tradition in philosophy. It is to Basil Hiley's credit that he has pointed out the influence of such philosophy on mathematicians who played a key role in the development of theoretical physics, Hermann Grassmann and William Rowan Hamilton (see Hiley (2011), also <https://arxiv.org/abs/1211.2107>).

⁶⁶Peat (1996), p. 200.

⁶⁷Bohm (1980).

⁶⁸Freire Jr. (2019), pp. 175–6.

⁶⁹Krishnamurti and Bohm (1985).

⁷⁰Some were in fact published as the first part of Krishnamurti (1977).

⁷¹Peat (1996), Afterword.

⁷²Moody (2016), Chap. 11.

⁷³Biederman (1948).

⁷⁴Bohm (1971).

The first concerns panpsychism. It was quite a revelation to read an article by an analytic philosopher, William Seager, referring to Bohm's panpsychism, included in the above collection "Emergent Quantum Mechanics, David Bohm Centennial Perspectives."⁷⁵ Seager argues that "advances in science serve not to eliminate metaphysical questions, but illuminate them and sometimes to reawaken metaphysical options that had faded from view." Pointing to the rebirth of interest in panpsychism, especially relating to "the problem of consciousness", he notes that "mental features are a fundamental and ubiquitous feature of the world" in the panpsychic viewpoint. This is an approach that "integrates mind and the physical world, which leaves the physical world causally complete, avoiding outside influences distorting the laws of nature, but nonetheless provides a role for mind in the world. We can see Bohm as a kind of pioneer for this rebirth." He gives many useful references⁷⁶ including several from Bohm's later writings as well as Hiley's. However the correspondence published here shows that Bohm was developing a form of panpsychism as early as the 1960s.

Freire notes⁷⁷ that the ideas of panpsychism introduced in the last chapter of "the Undivided Universe",⁷⁸ the "most daring conjecture of the entire book" could be expected to "dismay some readers". The suggestion that "participation goes on to a greater collective mind, and perhaps ultimately to some yet more comprehensive mind in principle capable of going indefinitely beyond even the human species as a whole," does seem to take us in a quasi-theological direction. It is thus very interesting to read Seager on this issue, positioning it within the traditions of academic philosophy.⁷⁹ Note that Seager has also written on what is called the "dual-aspect approach to the mind-matter problem" in relation to Pauli's views.⁸⁰

All the recent works on panpsychism referred to by Seager are "Western" in orientation, even when giving a history of panpsychism.⁸¹ But clearly there is a rich tradition of Indian philosophy, which despite difficulties of interpretation is now becoming better known.⁸² Articles on panpsychism from an Indian standpoint are appearing in western journals.⁸³ In Bohm's writing there can be found the stereotypical view of the "Greek western tradition" on the one hand and the "Oriental mystical tradition" on the other, which was prevalent in the twentieth century. It did not help that Krishnamurti presented his ideas as his alone and gave no references. However it

⁷⁵Seager (2018), also <https://www.mdpi.com/1099-4300/20/7/493/htm>.

⁷⁶See also the entry in the Stanford Encyclopedia of Philosophy, of which Seager is a joint author, <https://plato.stanford.edu/entries/panpsychism/>.

⁷⁷Freire Jr. (2019), p. 197.

⁷⁸Bohm and Hiley (1993).

⁷⁹Though there is fierce opposition to panpsychism among neuroscientists, e.g. "Conscious spoons, really? Pushing back against panpsychism," by Anil Seth, <https://neurobanter.com/2018/02/01/conscious-spoons-really-pushing-back-against-panpsychism/>.

⁸⁰"A New Idea of Reality: Pauli on the Unity of Mind and Matter," Atmanspacher and Primas (2009), pp. 83–98.

⁸¹For example Skrbina (2005).

⁸²See, for example Hamilton (2001).

⁸³For example Vaidya and Bilimoria (2015).

is increasingly recognised that this rigid separation emerged from the Eurocentrism of colonial times⁸⁴ and has become unacceptable.

The second recent development concerns the various criticisms that have been made of the current situation in fundamental physics.⁸⁵ Without committing myself to support any of these critics, which I am hardly qualified to do anyway, there does seem to be something of an impasse, especially following the failure of the Large Hadron Collider at CERN, Geneva to confirm any of the predicted “supersymmetry” theories after the Higgs boson success in 2012. No doubt new theories, or variations on old theories will be developed. Even so it would seem not to be a bad idea to consider the critique that Bohm was making in the 1960s and which is reflected in the title of the present book.

Bohm’s arguments for “pluralism” and for the importance of what he calls the “informal” as opposed to the formal mathematical approach would still seem to have some validity. The problem is that the material in the correspondence is too much rooted in the issues he was dealing with at the time, and are not especially well argued. For example, although Jeffrey Bub was praised for his reply to objections to Bohm’s views at Illinois, it would seem to me that he was wise to concentrate on quantum theory. When Bohm used different theories of malaria⁸⁶ as an example he seemed to be on shaky ground, especially his “psycho-social” theory with its suggestion that (following Krishnamurti) “[t]he centrally relevant feature is that man has for thousands of years lived disharmoniously.”⁸⁷ This could easily be interpreted as anti-science. Also the argument for the “informal” does seem to be carried too far, perhaps in the heat of the argument with Bub. In an email discussing Bohm’s criticism of Basil Hiley for his addiction to formal deduction,⁸⁸ Hiley, who must surely be David Bohm’s greatest champion, explained that Bohm was not correct in suggesting that Hiley did not “understand the irrelevance of these equations.” Rather Hiley considered that exploring a topic did mean “putting it into some mathematical framework”. In the process “one has to think deeply about the proposal.” Note also that if the impression is given in the correspondence that Bohm was anti-mathematics this is not correct. He saw the possible importance of algebraic topology for physics in the 1960s and did his best to understand it.⁸⁹ Also he was one of the first to appreciate the importance of Clifford algebras.

So consider how Bohm presented the same core arguments but in the light of two decades of experience in “Science, Order and Creativity”,⁹⁰ co-authored with David Peat. Firstly on Pluralism:

⁸⁴See McEvelley (2002).

⁸⁵Woit (2006), Baggott (2012), Hossenfelder (2018). Note Jeremy Butterfield’s review of Hossenfelder at <https://arxiv.org/abs/1902.03480>.

⁸⁶C136, pp. 292–296, C137, pp. 302–304 and p. 319.

⁸⁷C137, p. 302.

⁸⁸C137, p. 307.

⁸⁹See the reference to the British mathematician W.V.D. Hodge in C136, p. 262.

⁹⁰Bohm and Peat (2010).

The development of science can be seen in a Kuhnian sense, “until today it is taken as perfectly normal for revolution to succeed revolution, interspersed by periods of relative stability.” But instead, would it not be possible that “creativity can operate at all times, not just during periods of scientific revolution? . . . this would imply that, at any given moment, there would be a number of alternative points of view and theories available in each particular area of science.” Bohm and Peat give arguments to show that this viewpoint does not necessarily lead to a lack of objectivity, i.e. that social considerations or subjective preferences would dominate.⁹¹

Secondly on the importance of “informal” considerations:

“Today the general atmosphere is such that a physicist can do little more than state, and restate, a particular point of view. Various approaches are generally taken to be rivals, with each participant attempting to convince the others of the truth of a particular position, or at least that it deserves serious attention. Yet at the same time, there is a general tendency to regard the whole question of interpretation and the role of informal language as not being particularly important, and instead to focus upon the mathematics about which everyone agrees.”⁹²

No doubt Bohm and Peat’s appeal for “the opening up of a free and creative communication in all areas of science” which “would constitute a tremendous extension of the scientific approach,” and their conclusion that “[i]ts consequences for humanity would, in the long run, be of incalculable benefit” could be seen as utopian in the current climate. But that does not mean that Bohm’s critique should not be taken seriously.

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⁹¹Ibid, pp. 44–49.

⁹²Ibid, p. 78.

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Chapter 2

Folder C130. Correspondence with Loinger, Rosenfeld, Schumacher and Bub. October–December 1966



Oct 3, 1966

Dear Jeff,

I was very glad to hear from you, and to learn that all is well. As for me, I am just getting back to work, with a number of new ideas.

I am having 25 reprints of your article sent to you.¹

I have already written to Biederman, and he is very willing to see you, if you will visit him in Red Wing.²

Everyone here sends you and Mead³ our best regards, also to your wife. Please let me hear what you are doing, and how you get on with Biederman.

Yours sincerely

David Bohm

This is my reply to a letter I received from Professor Loinger.⁴ He also sent one to you here and this has been forwarded.

¹Presumably (Bohm and Bub 1966b)—CT.

²In a letter from Biederman to Bohm dated Dec 1, 1966 (C91b in the Birkbeck archives) he reports that Bub and his wife visited on Thanksgiving Day, Nov 24, 1966. Biederman and his wife enjoyed their visit. In relation to Bohm, Bub “has a great admiration for what you are searching to do in science, in spite of those who oppose your intentions”—CT.

³Alden Mead, was a physical chemist at the University of Minnesota. He spent a sabbatical at Birkbeck College during Bub’s last year there as a graduate student and offered him a post-doc position in the Chemistry Department after graduation, which was his first job (information from Jeffrey Bub)—CT.

⁴Angelo Loinger was one of a trio of Italian physicists, Adriana Daneri, Angelo Loinger, and Giovanni Maria Prosperi referred to in these letters as DLP. Their main publications were Daneri et al. (1962) also at Wheeler and Zurek (1983), pp. 657–679 and Daneri et al. (1966). See Freire (2015), Chaps. 4 and 5 for more details. See Appendix A for Bub’s correspondence with Loinger—CT.

12th October, 1966.

Professor A. Loinger,
Universita Degli Studi di Pavia,
Istituto di Fisica Teorica,
Via Taramelli 4,
Pavia,
Italy.

Dear Professor Loinger,

Thank you very much for your recent letter. I agree with you that refinements of the conventional formation by hidden parameters is an unlikely way of discovering a new theory along these lines. The only purpose of our article⁵ was to clarify the question of what it would mean to have a hidden variable theory. I would be inclined to think that you are right in saying that a generalized field theory in the Einsteinian sense is the probably real alternative to the Copenhagen point of view, though it is my view also that this field should refer to some sort of discrete space time, rather than to a continuum. When I discussed the question with Einstein in 1951, he agreed that there was no intrinsic reason why the underlying reality should not be discrete. However, he said that he knew of no mathematics that could handle such a theory. I am at present working on such mathematics, and a very early version of this work is enclosed with this letter.⁶ Naturally, it has gone a lot further since then.

About your own paper, I am not sure that you have actually succeeded in definitively settling the measurement question in the present theory, at least not to my satisfaction. You have made an important step in clarifying the role of the measuring apparatus. In this connection, I would like to suggest that you have carried to its logical point of completion a notion that was implicit in what I wrote in Chapter 22 of my book, Quantum Theory (Prentice-Hall, N.Y., 1951).⁷ However, with regard to the status of the present physical theory, I feel that the significance of your work would be clarified, by pointing out that it leaves certain questions unanswered, which are reasonable to ask in any physical theory. Here, I want to stress not the questions that arise merely in implementing Bohr's philosophy, but rather, those that arise by careful inspection of the physical facts themselves.

Now, the essential point about a classical probability concept is that when new information is obtained, the domain of possibilities is always narrowed down. But in quantum mechanics, it may be narrowed down in certain ways and extended in others. Thus, if we have a state of spin $\frac{1}{2}$, with $\sigma_2 = 1$ then if we measure σ_2 , it is impossible to obtain $\sigma_2 = -1$. But after you measure the X component of the spin, it becomes possible to have $\sigma_2 = -1$. So measurement did not merely give information about σ_2 ; it created new possibilities for σ_2 . Thus, as everybody agrees, is something not contained in the classical concept of probability.

⁵Bohm and Bub (1966a)—CT.

⁶Not in the Birkbeck archives, but see Bohm (1965), also <http://www5.bbk.ac.uk/lib/archive/bohm/BOHMB.149.pdf>—CT.

⁷Bohm (1989)—CT.