Chris Talbot *Editor*

David Bohm's Critique of Modern Physics

Letters to Jeffrey Bub, 1966–1969

Foreword by Jeffrey Bub

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Editor
Chris Talbot
Abingdon, Oxfordshire, UK

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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

[1] Freire Jr. (2019).

[2] Talbot (2017).

[3] https://www.mdpi.com/books/pdfview/book/1203.

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

[5] Peat (1996).

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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

[6] Moody (2016).

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

[8] They 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 Pylkkänen.[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

[9] C132, pp. 147–148.

[10] The 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).

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

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

[13] Pylkkänen (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 correspondence[20] 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

[14]These 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.

[15]Nichol (2002), pp. 199–234.

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

[17]It 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).

[18]Miller (2009).

[19]C131, p. 80.

[20]C131, 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 critized 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.

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[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 panpsychism 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,

[30] I 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.

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

[32] Bohm (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-Darwinism[39] 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

[33] Bohm (2006), pp. 164 and 169.

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

[35] C132, pp. 115–120.

[36] Waddington (1969), pp. 25–26.

[37] Ibid, p. 28.

[38] Ibid, p. 34.

[39] Ibid, 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, 1968[43] 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 Schumacher[45] 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 correspondence[47] 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

[40] Ibid, pp. 51–59.

[41] Noted by Bohm in C131, p. 83.

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

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

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

[45] No 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).

[46] But note Freire Jr. (2019), p. 150.

[47] See 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 Pylkkänen. Bohm introduces the Hegelian concepts of identity and difference[56] 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 Pylkkänen puts it "to drop "identity" from their set of concepts, as long as certain conditions are accepted."[57] But there is

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

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

[50] C130, pp. 36–41.

[51] C132, pp. 104–112.

[52] In 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."

[53] C131, pp. 89–98.

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

[55] C131, p. 92.

[56] Pylkkänen (1999), p. 109.

[57] Ibid, p. 244. Pylkkänen'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

[58] Nichol (2002), p. 218 and p. 219.

[59] Korzybski'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.

[60] Note 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."

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

[62] See, for example, Beiser (2005).

[63] C136, 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 Peat[66] 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 Peat[71] (anti-Krishnamurti) and David Moody[72] (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.

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[64] See Freire Jr. (2019), pp. 105–107.

[65] This 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).

[66] Peat (1996), p. 200.

[67] Bohm (1980).

[68] Freire Jr. (2019), pp. 175–6.

[69] Krishnamurti and Bohm (1985).

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

[71] Peat (1996), Afterword.

[72] Moody (2016), Chap. 11.

[73] Biederman (1948).

[74] Bohm (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 references[76] 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 notes[77] 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

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

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

[77] Freire Jr. (2019), p. 197.

[78] Bohm and Hiley (1993).

[79] Though 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.

[81] For example Skrbina (2005).

[82] See, for example Hamilton (2001).

[83] For example Vaidya and Bilimoria (2015).

is increasingly recognised that this rigid separation emerged from the Eurocentrism of colonial times[84] 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 malaria[86] 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:

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[84] See McEvilley (2002).

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

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

[87] C137, p. 302.

[88] C137, p. 307.

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

[90] Bohm 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 Peats' 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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[91] Ibid, pp. 44–49.

[92] Ibid, 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 Mead[3] 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.

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[1] Presumably (Bohm and Bub 1966b)—CT.

[2] In 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.

[3] Alden 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.

[4] Angelo 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.

C. Talbot (ed.), *David Bohm's Critique of Modern Physics*,
https://doi.org/10.1007/978-3-030-45537-8_2

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 article[5] 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.

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[5] Bohm and Bub (1966a)—CT.

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

[7] Bohm (1989)—CT.

Now, you manage to deal with this question by considering the role of the observing apparatus. Generally speaking, after an interaction between a system I and its observing system II, but before anyone knows what the result of the interaction is, the wave function of I + II is

$$\Psi_{I+II} = \sum_i \varphi_i(y) c_i \psi_i(x) \tag{1}$$

where x is the coordinate of I and y of II, and where the initial wave function was

$$\Psi_{I+II} = \varphi_0(y) \sum_i c_i \psi_i(x) \tag{2}$$

Now, you show that because of the ergodicity of the apparatus system II, there is no interference between the terms of Eq. (1). Therefore, it is impossible by looking at system II to create new possibilities for I, because these latter depend on interference between the terms in Eq. (1). Therefore, the quantum mechanical rules for probability have now become more like classical rules.

Nevertheless, there is something left out of your account. This is that when a certain result is obtained in the measurement (e.g. $i = n$), then this result is reproducible. It is this behaviour which indicates the individual properties of a system, i.e., that certain properties of such a system are uniquely determined by the wave function. These include not only those operators for which the wave function is an eigenfunction, but also all properties that have probability unity. For example, one knows for certain that the electron will be found in the region in which its wave packet is appreciable, while its momentum will be found in the region where the fourier coefficient of this packet is appreciable.

This was all explained in our article, in connection with the diffraction experiment carried out with the cine camera, in which one electron entered the apparatus at a time. Each electron has its wave function, which is similar in shape to that of the others, but different in time of entering the system. Whether you say the wave function represents our knowledge or an objective reality beyond this, you must admit that when the wave function is given, then properties of this kind belong to the individual system, and are uniquely determined by the wave function. This is verified by the fact of reproducibility of a series of measurements of properties of this kind.

Now, as I take it, we both agree that the actual state of the system does not depend on our knowledge of the system. Therefore, after interaction of system I with the apparatus, II, but before anyone has looked at the apparatus, the wave function must be

$$\Psi_{I+II} = e^{i\phi} \varphi_n(y) \psi_n(x) \tag{3}$$

where ϕ is an arbitrary phase factor and n is the actual state of the system, which will be known later when someone looks at the apparatus.

Therefore, it is wrong to say that after interaction of I and II, but before anybody looks, the wave function is given by (1). If it were given by (1), then one would be forced to conclude that as soon as someone looked at the apparatus the wave function suddenly changed to III when he became conscious of the result of the measurement. For there is no doubt that if the result is to be reproducible, the wave function must be given by (3) after someone is conscious of the results of the measurement. So if we accept your claim that your treatment of the question is complete, we must either adopt the assumption that the system is influenced by our consciousness of it, or else we must say that both before and after an observer looks, the wave function is (1). But this latter assumption does not provide for the reproducibility of the measurement. Therefore, it is unacceptable. Since you also do not accept the influence of the observer's consciousness, you have not accounted for why the wave function is (3) and not (1), as soon as the interaction of I and II is complete, but even before anybody looks at the results of this interaction.

We see then that a simple inspection of the physical facts, along with an acceptance of the "realist" philosophy about the significance of measurements in physics, forces us to say that while the interaction between I and II is taking place, something more is happening than the destruction of interference between the terms of (1), because of ergodicity of the apparatus. This something more is that the wave function is not only undergoing a unitary transformation that turns it from (2) into (1). It is also undergoing a non unitary transformation that turns it from (1) into (3).

One is then immediately led to ask questions, such as: What is the equation of the process by which the wave function turns from (1) into (3)? Just when does this take place? (For until it does, the measurement is not complete.)

These are the sort of questions that our article shows how to answer, in terms of a theory that is admittedly not likely to prove to be on the correct lines. But we only wanted to make it clear what the nature of these questions is. Any theory of hidden variables would have to face similar questions, and I feel that your article would have been more clear and useful, if you could have brought this point out.

It may be that I have misunderstood your article. If so, please let me know in what way I am wrong.

With best regards, Yours sincerely,

D. Bohm.

Nov 15, 1966

Dear Jeff,

Thanks very much for your letter. I think your position is correct, as you will see from the enclosed copy of a letter to Loinger. By all means, write a detailed letter to him on the subject.[8]

[8] See Appendix A—CT.

I am glad to hear that you might get job in the philosophy dep't in Minnesota in Feigl's[9] dep't. I hope it comes through.

I shall be interested in hearing how your talk with Biederman goes.

It seems to me that the epistemological question raised by Bohr (see my letter to Loinger) is a crucial one. But his answer is on the wrong track. The key question is that of regarding physics as an extension of perception (as in the appendix to the Relativity book).[10] (But of course, it has little or nothing to do with Bertrand Russell). The question is: "What is really the immediate fact for physics? What does the physicist really perceive? Isn't classical physics largely a "descriptive fiction?" It is an exceptionally slippery question to discover just what the directly and immediately observed fact really is. Many philosophers have postulated fictions such as sense impressions. But these are as abstract as are the "atoms" that they are intended to replace. Somehow, man really perceives order, pattern, structure, potentiality along with actuality, totality and its partial aspects, etc., etc. Our mathematical terms are very far from these perceptions indeed. That is why physics seems to have degenerated into a purely formal one [of] practical manipulations of algorithms. Perhaps you can raise this question with Biederman, and ask him how art can help the physicist to be aware of how and what he actually perceives.

Best regards

D Bohm

Nov 24, 1966

Dear Jeff

I received your second letter. Meanwhile, I hope you received my letter to you, along with a copy of my answer to Loinger.

In my view, it is not possible to prove that Bohr's epistemology is compatible with "objective" probabilities in quantum theory, if only because the whole theory has been constructed in such a fashion that the interpretation of the "algorithms" depends essentially on what an observer is supposed to be able to communicate. As long as the equations are linear, you can't get out of the conclusion that the wave function spreads over a range of "quantum states", until another apparatus (III) observes (I+II). Any "objective" description would have to discuss the probability that the system I+II is in a certain state, S, (which we could call $P(S)$). But I+II is not in a state corresponding to eigenvalues of what is measured, unless the wave function is $\psi_i(I)\varphi_i(II)$. So there is no way to talk about the "objective" probabilities. We can only talk about probabilities of what III will find, when a further observation is made. I think that this is the essence of the argument against Loinger. No doubt,

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[9] Herbert Feigl, Austrian philosopher and member of the Vienna circle, was for many years Professor of Philosophy at Minnesota, establishing the Minnesota Center for Philosophy of Science in 1957—CT.

[10] Bohm (1996)—CT.

Rosenfeld[11] has confused both what DLP say and what Bohr says. It would be much better if he had said nothing at all.

Could you write to Loinger a letter[12] doing the following?:

(a) Explain where DLP go wrong.

(b) Explain where Rosenfeld confuses DLP.

(c) Explain where Rosenfeld confuses Bohr.

How did you get on with Biederman.

Best regards

D Bohm

10th November, 1966.

Professor A. Loinger,
Universita Degli Studi di Pavila,
Istituto de Fisica Teorica,
Via Taramel1i 4, Pavia,
Italy.

Dear Professor Loinger,

To some extent, we have been arguing at cross purposes and not meeting. I have always meant to say that you have made a contribution to the more nearly complete expression of Bohr's point of view. What I have been trying to emphasize is that I am not satisfied with this point of view, and that it has inherent inadequacies, which are not really changed by your work.

When I wrote my book, Quantum Theory, my main objective was to try to understand what the subject is all about, and in particular, what Bohr is actually saying. After writing it and thinking the whole question over, I began to feel that, after all, I did not understand. This was perhaps the main reason why I started then to inquire into hidden variables.

Since that time, I have had several opportunities to discuss with Niels Bohr and with his assistant, Aage Petersen.[13] Although we never brought up my own book in a direct way, I did gather (largely from Petersen) that Bohr did not really like my approach in this book, and felt that I had not understood what the point of complementarity actually is. I also gathered (though it was never stated explicitly) that Bohr and Rosenfeld did not really see eye to eye on these questions. Indeed, I

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[11] Léon Rosenfeld, long associated with the Niels Bohr Institute in Copenhagen, was then professor at Nordita (Nordic Institute for Theoretical Physics). For more on Rosenfeld including his earlier conflicts with Bohm see Freire (2015), Kožnjak (2017). For a letter from Bub to Bohm on Rosenfeld's paper, see Appendix B—CT.

[12] See Appendix A—CT.

[13] Petersen was Bohr's assistant from 1952 to 1962. His 1966 doctoral thesis was published as Petersen (1968)—CT.

had long suspected that Rosenfeld's belief that Bohr is a natural born dialectician was, to a considerable extent, merely Rosenfeld's own interpretation. Later, when I received a manuscript from Petersen (about a year ago) on Bohr's philosophy, this suspicion was confirmed.

Now as I see it, Bohr has been giving top priority to the role of language. As Petersen says, Bohr was fond of emphasizing that "Man lives suspended in language". Therefore, to Bohr, the principle of complementarity is primarily a linguistic question. As Kant raised the question of what conditions (such as space, time, causality, etc.) are the preconditions of all experience, so Bohr asked what is the precondition of the precise communicabi1ity of descriptive information about nature. He came to the conclusion that the concepts of classical physics (position and momentum) are what determines this precondition. This conclusion is not primarily physical, but rather, epistemological. What it amounts to is that Bohr believes that the language of classical physics is in some way a part of the "human condition" (or perhaps one could say that it is an intrinsic aspect of human nature, and of man's interactions with his environment). Another closely related aspect of the "human condition" is the language of subject and object. That is, we always say that there is an observer who is looking at some object, that is being observed. As long as the interaction of the observer with what is observed is not subject to restrictions, then there is no limit to the precise communicability of information about momentum and position. But because of the quantum, subject and object can no longer be separated, and therefore precise information about the object is no longer possible.

Bohr then emphasizes that precise classical descriptions are to be replaced by the mathematical algorithm of quantum mechanics, which contains just the required limitation on precision of information about the object.

It can be seen that the essential assumption of Bohr is in the field of epistemology, and more particularly, in the field of linguistics. Bohr's belief that the language of separation of subject and object, along with the description of the object by classical concepts such as position and momentum, is inherent in the "human condition", means, in effect, that he puts certain aspects of the human mind (i.e., language) in the first place, in man's relationships with nature. This approach is not really compatible with the dialectical materialist point of view adopted by Rosenfeld.

I think that Rosenfeld does a disservice both to Bohr and to his own dialectical materialist views, by failing to recognize that Bohr is not a materialist (though he may perhaps be, in certain ways, a dialectician). Indeed, I learned in conversations with Petersen that in his early life, Bohr as strongly influenced by Kierkegaard. One can in fact see the influence of existentialism in the principle of complementarity. Man is somehow an individual and yet he must be in indivisible union with God. This unresolvable contradiction must cause man to be in a state of torment. For every time he tries to determine some aspect of his individuality, this comes into contradiction with another equally significant aspect. Somehow, man exists as a totality, but in his perceptions and actions, he can always only define one fragment of himself at the expense of another. Similarly, in physics, Bohr takes subject and object as being indissolubly united by the quantum that "connects" them. Yet, this unity can never be defined. When one aspect (position) is defined, this comes into contradiction with

another aspect (momentum). Our language, according to Bohr, forces us to assert the separation of subject and object, while we know that they are united (as it also by implication forces us to assert the separation of God and man, though all religious people have said that they know the two are inseparably united).

Now, while I think it is useful to raise all these questions, I do not believe that Bohr's answers are pertinent. There is no evidence that the classical language of position and momentum is part of the "human condition". Rather, it evidently evolved historically for specific reasons. Indeed, in a typical cloud chamber, we never observe positions and momenta. We observe the order of a series of droplets (approximated as on a curved arc) and then we translate these into positions and momenta. So I would like to emphasize that man's direct perceptions are not limited by the need to be expressed in classical concepts of position and momentum.

To be sure, mathematics today is not able to say much about order, pattern, structure, etc. But this deficiency can ultimately be remedied. I am publishing an article on the subject, which I will send to you when reprints are available. In essence, I have reason to believe that both mathematics and physics can take the concepts of order, pattern and structure as basic. When this is done, quantum mechanics will be seen to fit naturally into this new language. The dichotomy of subject and object is thus removed. For order, pattern, and structure refer both to the order in the mind that perceives them and to the order in what is perceived. Indeed, the whole concept of an object is then merely an abstraction from an over-all structure. To each abstraction of an object, there must correspond an abstraction of a subject, which determines the "perspective" from which the object is "observed". But the basic reality is the total structure, in which subject and object are both contained as abstractible aspects.

So much for the Bohr point of view. Now, I understand that you are trying to base quantum theory on objective laws applied both to the measuring apparatus and to the observed system. Of course, you realise that this approach is diametrically opposed to Bohr's philosophy. I am sure that Bohr would have regarded it as being on a wrong track altogether, just as he probably thought of the approach in my Quantum Theory book. For this reason, I think that Rosenfeld is confusing the issue, by falling to distinguish clearly between his own point of view and Bohr's basically epistemological approach. In Bohr's point of view, it is absolutely essential that the ultimate observing apparatus be treated purely in terms of classical concepts, and that no attempt be made to represent an "objective" system in terms of the quantum mechanical algorithms.

The way Bohr would interpret your work is as follows. It is permissible to treat "object" (system I) and "observing apparatus" (system II) as a combined system, I+II, which is handled in terms of the quantum mechanical algorithms. But then, this presupposes an observing apparatus, III, which is "observing" system II. Apparatus III must be treated in terms of classical concepts only. The usual probability laws of quantum mechanics apply then to I+II. This means, of course, that the "reduction postulate" applies to the wave function, I+II, when the system, I+II, is "observed" by III.

As I indicated at the beginning of this letter, your treatment could be interpreted by Bohr as an extension and completion of his own notions. In effect, your theory pro-

vides a detailed explication of how it comes about that the "cut" between "observer" and "observed" can be moved around freely in the large scale domain. In your case, this "cut" is placed between apparatus II and apparatus III, which latter is assumed to belong to the purely classical domain, concerning the state of which human language allows the precise communication of detailed information. But of course, it is, according to Bohr, entirely inadmissible to discuss the quantum mechanical side (I+II) as if no human being were present. For the whole meaning of the quantum mechanical algorithm is that it refers to what a human being can communicate in precise terms about system III. To treat system I+II as if it existed objectively without III (which is taken to be, in essence, an extension of the observer's sense organs) is to deny the deep meaning and basic spirit of the principle of complementarity.

You ask my opinion of Tausk's paper[14] In my view, it is essentially right, though I think that Tausk does not pay enough attention to the significance of the ergodic properties of large scale systems in making possible a consistent theory, along Bohr's lines, in which the "cut" can be moved freely in the large scale domain. Dr. Bub has written to me that he also has analyzed Tausk's paper and found it to be essentially correct. I understand that he will be sending you some detailed comments on this subject.[15] Meanwhile, I would only emphasize that it is not enough to get the right probabilities for the properties of system I. If your treatment is to be "objective", it must also show that after I and II have interacted, but before system III has "observed" system II, the wave function is definitely on one of the "channels" of II, corresponding to non interfering results of possible measurements. As I see it, Tausk's treatment shows clearly that you have only provided for the "weak" reduction postulate – i.e., that after I+II have interacted, the "probabilities" are the same as if the system II were "objectively" in a certain channel. But where do these "probabilities" come from and to what do they refer? Basically, they are brought in only by tacitly assuming system III, which will "observe" I+II, and which can obtain certain well defined results with corresponding probabilities. But once you have presupposed system III, you have also brought in the reduction postulate. It is this postulate which provides for the fact that the wave function II is in one of the channels. As Tausk points out, if you don't assume the reduction postulate, then after I+II have interacted, the wave function of I+II still spreads out over all the channels. As long as this is the case, there is no way to provide for the reproducibility of a measurement of II. For after you obtain a certain result for II in any one measurement, the wave function still spreads out over all the channels, so that in the next measurement, you will in general get some other result. It is only because you have tacitly assumed the operation of system III, which is in essence an extension of the observer, that we can say that after the observation of II by III, the reduction postulate implies that II is in a part of its Hilbert space corresponding to a single one of its channels, and not in a part corresponding to its being in many channels at the same time.

[14] Tausk's paper is not available. See Freire (2015), Chap. 5 and Pessoa et al. (2008). See also Appendix A—CT.

[15] Appendix A—CT.

If you treat the system II in terms of the quantum mechanical algorithm, then Bohr would say that this algorithm refers to predictions of the behaviour of system III, which is used to "observe" II. I don't see how you can possibly get out of this, without giving up the very essence of Bohr's philosophy. (This point has been explained very clearly in a recent preprint by Aage Petersen, also by a man named Schumacher in Cornell University, who has been in correspondence with Rosenfeld.)

I am sure that Rosenfeld has, as you say, an extraordinary esprit de finesse. Nevertheless, I do not think that he has really understood the essence of Bohr's point of view in spite of having lived for a long time with Bohr. Bohr's ideas are very hard to grasp. Indeed, they are so unclear that I think it would be good if those of us who are interested in them (including you and Rosenfeld and myself, along with others) could some day get together for a full and informal discussion.

In von Neumann's point of view, it is never clear just what is meant by the term "making a measurement". Sometimes it seems that he refers to the registration of the event on the mind of an observer and at other times, he refers to its registration in what he calls "classical observables". Neither of these notions coincides with Bohr's position. Indeed, as Bohr has explicitly said to me, "Nothing is ever really measured at all in quantum theory". Rather one observes the state of the apparatus and describes this state in classical terms, including the whole set-up of the "experimental conditions". One then applies the algorithm of quantum mechanics, to make statements about what can or will be observable later in the apparatus. One can see that the experimental conditions needed to measure conjugate variables precisely are not compatible with each other. This incompatibility is expressed perfectly by the failure of the corresponding operators in the algorithm to commute. As a result, the statements that one can deduce from the algorithms about these sets of experimental conditions have exactly the right degree of ambiguity to match the ambiguity of the classically describable specification of the experimental conditions themselves. This is the meaning of the uncertainty relationships. Physical experiments have nothing really to do with measurements of "objective" conditions at the quantum mechanical level. Rather, they have to do with statements about the behaviour of classically describable aspects of the world (usually of a piece of laboratory apparatus). Therefore, your whole programme is directly contrary to what Bohr wants to do.

(3) You say that practically, a superposition of macroscopically distinguishable states is equivalent to a mixture. It is just this word "practically" that I cannot accept. If we were satisfied to regard quantum mechanics as nothing more than an elaborate formula for an engineer's handbook, then your reasoning would be adequate. But surely, Bohr and von Neumann were not satisfied to look at it in this way, and I doubt that you are either. Rather, there is the implication that quantum mechanics is a logically coherent structure of physical and mathematical ideas. Whenever, in a purely logical argument, you identify something with something else that merely approximates some of its qualities, you have a contradiction. It doesn't matter how "small" the error is, it is still a contradiction. For logically, there is no such thing as a "small contradiction". Either the theory is logically coherent, or it is self-contradictory.

No matter how "small" a contradiction is, there is a theorem in mathematics that from it, one can derive any statement whatsoever (this includes both true and false

Dec 8, 1966

Dear Jeff,

Just received your letter of Dec 4. Meanwhile, I have already sent you another reply to Loinger.

I rather doubt we are going to get a lot further with DLP. I would only add that their point (3) is unclear (See Loinger's letter to you).[16] If the probability of "finding" is a probability in the sense of q.m., it means that it is the probability what apparatus III would find, if it looked at I + II. So unless you begin with apparatus III, the rest of what DLP [say] makes no sense. Apparatus III will always "reduce" the wave packet of I+II. The only contribution of DLP is to show that it doesn't matter whether you regard apparatus II as being on the classical or q.m. side of the "cut". This is also what I did in my quantum theory book, Chap 22. But Loinger does not accept this, nor do I think that he will ever do so.

Also, about Loinger's point (4), von Neumann's work is not clear, as is shown by his false proof of the impossibility of hidden variables. It is only a structure of mathematics. Its physical notions are often confused.

Their conclusion that practically a superposition of macroscopically distinguishable states is equivalent to a mixture is hardly a surprisingly new discovery. Many people have shown this, in different ways. DLP do it in yet another way.

Their point (7) is totally wrong. Without apparatus III, there is first of all no meaning to "finding" I+II in a certain state. And secondly, without apparatus III, I+II is still always in a pure state. It is, as I pointed out, a logical contradiction to equate it with a mixed state, however similar its "practical" consequences may be.

I shall be interested in hearing your further questions about Biederman and related topics. I do not like the word "caricature" either. The question is whether theories are a direct reflection of things as they are. In a way, they have to be just this. But in another way, they cannot be this. The interesting question is: "What, if anything, do theories reflect?" This leads to the question "What is knowledge, and its relationship to what is known?" I shall be sending you something on this soon.

Sincerely

David Bohm

Dec 10, 1966

Dear Jeff

I enclose several copies of my recent correspondence with Dr Schumacher, which will give you some idea of my notions on questions of epistemology. I will write you in more detail later.

[16] In Appendix A—CT.

his total milieu. On the contrary, as man can learn the structure of his environment by sensitive and careful perception, he can also learn in a similar way how he is being limited by language, and thus develop new ways of expressing the new content that is disclosed in perception.

As I see it, Bohr is emphasizing certain epistemological conditions of communicability of precisely defined information, which, as it were, come before all questions of the actual content of this information. In this sense, he is using an argument similar in some ways to that of Kant, who emphasized the conditions of space, time, causality, etc., that are the necessary forms of experience, as distinct from the particular content of this experience. I regard it as a valid line of study to raise these questions. But before one answers them, one must also ask a similar question: "What are the conditions of communicability of knowledge about epistemology itself?" Bohr seems to accept tacitly the notion that whereas we must learn the actual content of our knowledge of nature, we are given some kind of direct and completely reliable intuitions about epistemology, that are free of epistemological confusion. Thus when he says certain things about the relationship of subject and object, the "cut" between them, etc., etc., he seems to accept all this as a self evident truth, which will be eternally valid, and is not open to serious questions. To me, however, it seems that we are infinitely more confused about just these epistemological questions than we are about the content of our scientific knowledge. As you yourself have remarked, the unity of subject and object is inseparable. What can it mean then to think a "cut" between them? This "cut" is an absolute contradiction of their unity. Of course, Bohr argues that language forces us into this cut. But I do not accept this limitation as inherent or necessary. Probably, it is only a result of certain habits of thought, expressed in terms of certain linguistic structures that have been common over the past few thousand years.

Whatever the truth may be about this point, I feel that we have hardly scratched the surface of the question, and that Bohr has therefore prematurely claimed to settle the issue definitively with the principle of complementarity.

In addition, there is the further unclear question of what it can mean to have knowledge about epistemology. Epistemology is by definition supposed to deal with the general structure of knowledge and the means by which we attain it and express it. But when we come to learn about epistemology, what we learn in this way is part of the total content of knowledge. If there are inherent structural limitations in the applicability or definability of the content of knowledge (e.g., inherent in man's mode of communication through language), then such limitations very probably exist on the content of knowledge about epistemology. Therefore, one cannot be sure that limitations deduced from the content of knowledge about epistemology are genuine.

On the other hand, one may suppose that precisely with regard to epistemology itself, man may have completely reliable knowledge which is not limited by epistemological considerations (such as the need to express it in language). But then, this admits the principle that some kinds of knowledge are not subject to epistemological or linguistic limitations of any kind. If this is true of some kinds of knowledge, where is the "cut" to be drawn between this kind of knowledge and the other kind that is thus limited?