The Spirit and the Letter of Copenhagen: 
A Response to Andrei Khrennikov

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The present communication addresses (by way of a response to Andrei Khrennikov’s recent argument), the epistemology of quantum mechanics and Bohr’s interpretation of quantum mechanics as complementarity.

1 Introduction

In his posting to the ArXiv (quant-ph/0202107 v1), Andrei Khrennikov offers what he calls the “Växjö Interpretation of Quantum Mechanics” (alluding perhaps to David Mermin’s “Ithaca Interpretation”) along with a critical commentary on “the Copenhagen Interpretation” of quantum mechanics, and, in Heisenberg’s famous phrase, the spirit of Copenhagen, and specifically Niels Bohr’s views. As his commentary refers, at arguably crucial points, to my own discussions of these subjects, I would like to address some of his points here. I also hope my remarks will help to clarify at least some of the aspects of Bohr’s interpretation of quantum mechanics, known as complementarity, about which, the spirit and the letter of which, there are still so many misconceptions and so much confusion. One might take advantage of the pun on the very word “letter” and see Bohr’s writings on complementarity as a letter or several letters, which were written to Einstein in particular, but also to all of us, and which are still awaiting their recipients at the post office of the history of quantum theory. In this case (or perhaps in any case), however, having received and even having read these letters by no means guarantees that they have reached their destination, including in the case of the present author. It is not entirely clear, and it did not appear to have been to Bohr, to what degree these letters reached their destination in Einstein’s case. But they have reached their destiny insofar as the Bohr-Einstein debate has fundamentally shaped the history of quantum theory.

Andrei Khrennikov, “Växjö Interpretation of Quantum Mechanics,” arXiv: quant-ph/0202107 v1, 19 Feb 2002; On Mermin’s “Ithaca Interpretation,” see N. David Mermin, “What Is Quantum Mechanics Trying To Tell Us?” American Journal of Physics 66 (9), 1998: 753-67.
It would be difficult for me to assess the merits of the Växjö interpretation itself, since, as an interpretation, it does not appear to me to be sufficiently explicated in Khrennikov’s communication as an interpretation of quantum mechanics as a physical theory. Accordingly, I shall not address it here. One certainly cannot object to pursuing this type of program; and I find, in particular, the p-adic approach to quantum theory intriguing, at least from a mathematical standpoint, even if only because the mathematics of p-adic numbers is very beautiful and quantum considerations may well contribute to this mathematics. On the other hand, I found Khrennikov’s argument concerning complementarity puzzling. The nature of my puzzlement stems primarily from my view of complementarity as an interpretation of quantum mechanics, and I shall restate this view first, in the first part, “The Spirit of Copenhagen,” of this paper. My position is, by and large, similar to that of Bohr or Heisenberg, although one might argue that they, especially Heisenberg, sometimes made stronger claims for complementarity as an interpretation of quantum mechanics. There are also those who see complementarity or other interpretations of quantum mechanics as actual descriptions of nature at the quantum level rather than as specific interpretations of a particular theory or experimental data it deals with and only in this, more limited, way telling us something about nature or about ourselves in our interaction with nature through our experimental technology. What these interpretations (there is no quantum mechanics apart from them) tell us is of course the question. Then, I shall address certain key aspects of complementarity that do not appear to me to be sufficiently taken into account by Khrennikov’s paper. In the second part, “The Letter of Copenhagen,” I shall comment on several specific points of the paper, in particular those dealing with Bohr’s actual statements.

2 The Spirit of Copenhagen

I see complementarity is an interpretation, a particular interpretation, of quantum mechanics, but only one among other possible interpretations, of which
there are now many (the assessment of their effectiveness is of course a different matter). Indeed, there is more than one version of complementarity, even in Bohr’s own work, let alone if one considers the work of others who appeal to complementarity, several founders of quantum mechanics among them. These interpretations are sometimes assembled under the rubric of the Copenhagen or, as it also called, orthodox interpretation, and often uncritically seen as a single interpretation. Instead, the phrase “the Copenhagen Interpretation” may be best seen as referring to a cluster of related interpretations of quantum mechanics, which do share certain physical and epistemological features, but which also, sometimes significantly and even fundamentally, differ, especially in their epistemology. One should also be careful as regards Bohr’s own versions of complementarity, of which there is more than one as well. Here I shall be specifically concerned with the post-EPR version of his interpretation, by and large finalized by Bohr in his “Discussion with Einstein on Epistemological Problems in Atomic Physics” (1949) and related later works. In my view, these works offer Bohr’s most consistent and most worked out exposition of his argument, in part because this exposition (re)defines what constitutes individual physical phenomena that are and, in his view, could only be rigorously considered by quantum mechanics as, or, again, interpreted as, complementarity. All relevant experimental data must be seen in accordance with this definition. There are also different interpretations of complementarity itself, as Bohr’s interpretation of quantum mechanics, whichever version of complementarity one considers. This is of course also true with respect to other interpretations of quantum mechanics, since whenever we consider such interpretations we also interpret them. Accordingly, the present argument represents only an interpretation of Bohr’s interpretation of quantum mechanics, even leaving aside those points of my argument whose attribution to Bohr’s view is uncertain, or of course those that expressly depart from Bohr. I shall indicate these points as I proceed.

Interpreted as complementarity, quantum mechanics is seen as a theory that a) employs a mathematically rigorous formalism; and b) predicts, in sta-

\[c\] It is worth further specifying that by quantum mechanics itself I only mean the theory covered by the standard mathematical formalism in whatever version, beginning with Heisenberg’s matrix formalism, rather than, say, any of the Bohmian hidden-variables theories. Technically, there are further epistemological and even physical, and hence also interpretive, complexities defined by particular versions of the mathematical formalism of quantum mechanics, such as that of Heisenberg vs. that of Schrödinger, but I shall put these complexities aside here. In any event, the formalism in question is seen as enabling the predictions of the data in question, such as the results of the double-slit experiments, the EPR correlations, and so forth.

\[d\]Niels Bohr, *Philosophical Writings of Niels Bohr*, 3 vols., Ox Bow Press, 1988, vol. 2, pp. 32-66. This work will hereafter referred to as PWNB.
tistical terms, the outcome of all relevant experiments within the scope of phenomena it considers. In addition, there is no conflict between quantum mechanics and other experimental data of physics, in particular (this becomes crucial) relativity. Quantum mechanics is a local theory.

In this sense, quantum mechanics is as complete as classical physics. Complementarity, however, makes quantum mechanics epistemologically different from classical physics, insofar as, in this interpretation, quantum mechanics only predicts the outcome of relevant experiments but does not describe, even in principle and even as an idealization, the physical behavior of the ultimate objects that it investigates, quantum objects, and that are responsible for the appearance of the data in question. (Classical physics, specifically classical mechanics, expressly does both, at least in idealized cases and, one might add, in most interpretations of it, including by Bohr. For one could in principle interpret classical physics differently.) Indeed complementarity sees such a description as, in principle, impossible. This view establishes the difference between Bohr’s and more traditionally positivist approaches, according to which, roughly speaking, one merely need not be concerned with providing such a description. This difference is epistemologically crucial, even though it may not be significant from the point of view of handling physics. Classical statistical mechanics also does not describe the behavior of the individual constituents, such as molecules, of the multiplicities it considers, but these constituents are assumed to behave classically, specifically, according to the laws and pictures of classical mechanics, which assumption is indeed crucial for the probability laws and counting procedures of classical statistical mechanics. This assumption, however, is not possible in complementarity in any circumstances, either in the case of individual objects or quantum collectivities, which also restrict quantum mechanics to statistical predictions concerning such behavior, or again, more accurately, to the impact of such behavior upon measuring instruments. Quantum mechanics can make some exact predictions, constrained by uncertainty relations, for example, in the EPR situations, but by virtue of the limitations imposed by uncertainty relations, such predictions are, from the perspective of complementarity, never sufficient to allow one to assume a classical-like behavior of quantum objects. Nor, in this view, can one rigorously speak of quantum objects themselves either as particles or as waves, or in terms of even partial properties of particles or waves, or indeed in terms of any conceivable physical objects or their attributes. The concept itself of an individual event or phenomenon needs to be redefined in terms of observable effects of a given interaction between quantum objects and the measuring instruments involved, an interaction that is accordingly, irreducible, in contrast to what obtains in classical physics. This redefinition also leads to a particular view of uncertainty
relations, which, as I shall also explain below, now rigorously apply only to the classical physical behavior of certain parts of measuring instruments.

These considerations do not of course imply that “quantum objects,” or rather what we infer as such, do not exist. Quite the contrary; it is their existence that, in this view, prevents us from conceiving of the way they exist or behave, or, to begin with, makes us infer the existence of such entities from the experimental data in question. In other words, we are compelled to infer this “inconceivable” from certain effects manifest in measuring instruments, such as those of the double-slit experiments, while the particular character of these effects, especially considered in their totality, prevents us from unambiguously speaking of any properties, even single properties, of quantum objects themselves or of their behavior.

Indeed, quantum mechanics as complementarity does not physically describe the behavior of any physical systems, including measuring instruments, which are, in this interpretation, seen as described by means of classical physics. More accurately (there is much misunderstanding on this point), classical physics describes those, and only those, parts of measuring instruments where the data in question is registered. By contrast, those components of measuring instruments that interact with quantum objects (it is this interaction that is responsible for the appearance of the effects in question) are quantum and, hence, are themselves indescribable, as is the interaction itself by virtue of its quantum character.

Of course, if one sees (as Einstein did) a physical description of the ultimate objects of the theory as a requirement of completeness, then quantum mechanics as complementarity would be incomplete. Accordingly, I call the very possibility, available in and usually defining classical physics, of such a description “classical.” (Bohmian theories, for example, would be seen as classical in this view, even though they should be rigorously distinguished from Newtonian mechanics.) In other words, quantum mechanics may be incomplete by classical epistemological criteria, established largely on the basis of classical physics or even a particular (causal and realist) interpretation of classical physics. This (i.e. classical) type of interpretation appears much more difficult to attain and is perhaps (according to complementarity, definitively) impossible in the case of quantum mechanics. In any event, short of this requirement, which is, again, epistemological or philosophical, quantum mechanics as complementarity is complete within its proper scope, as complete as classical physics is within its proper scope.

Is quantum mechanics as complementarity as complete as any theory of quantum data can possibly be? I would not make such a strong claim and I do not think that Bohr, or even Heisenberg, does either, although their position
may, at points, be somewhat stronger than mine. (Khrennikov appears to attribute a stronger position to my own argument as well.) It is, as I said, true that there have been some who believed or argued, and some still do, that this type of interpretation is inevitable, often without actually following the interpretation itself (be it Bohr’s complementarity or other) to its radical epistemological limits or even giving it a careful consideration at all.

It is in view of the considerations just given, especially the understanding that complementarity is an interpretation, a particular interpretation, of quantum mechanics (the standard quantum-mechanical formalism cum the data in question), that some of Khrennikov’s arguments concerning complementarity appear puzzling to me. For, it follows that, contrary to Khrennikov’s apparent view, complementarity neither prohibits nor aims to prohibit the possibility

a) that quantum mechanics could be interpreted otherwise, as it has been and continues to be, along all conceivable lines and, if anything, with increasing profusion; and

b) that other theories (such as those of the Bohmian type) of the same data could be offered.

Accordingly, Khrennikov’s apparent argument to the contrary appears to me misplaced. This argument appears to assume that complementarity prohibits certain epistemological alternatives, while I would argue that, as an interpretation of quantum mechanics, complementarity prohibits certain epistemological (or ontological) features within its own framework, which other interpretations or theories may allow. The assessment of such alternatives themselves is, again, a different question.

I might add the following point to my argument for complementarity as an interpretation, and only an interpretation (one among possible others), of quantum mechanics. On the one hand, this argument may be seen as entailing a weaker claim, again, possibly than that of Bohr himself or that of Heisenberg, upon complementarity or quantum mechanics itself as concerns their relations to nature at the ultimate level of its constitution (including whatever dynamics behavior of possible constituents might be involved). Upon this constitution complementarity makes no claim at all, and, by definition, it prohibits any such claims as part of its own conceptual structure. On the other hand, however, it follows that this impossibility of any description or conception concerning this constitution is itself an idealization pertaining only to complementarity and not to nature itself. Strictly speaking, one should, accordingly, put quotations marks around “nature” (at least at that level), or “quantum,” “constituents,” “dynamics,” “ultimate,” or indeed any term one might possibly use here. We do not know whether such terms are ultimately applicable or not to “nature” at that level. They are ultimately not applicable, anymore than “particles”
or “waves,” in complementarity as an interpretation of quantum mechanics, and function there only provisionally or metaphorically. They may be applicable to a greater degree in classical-like physical theories or interpretations, including those of quantum mechanics. The development of our theories (for example, quantum field theories) and of their interpretations may lead to more classical-like views of them and, possibly, of nature itself. It may, however, also reveal even great complexities and yet more radical forms of inaccessibility or inconceivability of “nature,” even though it appears difficult, if not impossible, to go beyond complementarity on that score.

As concerns complementarity itself and substantive problems it may have as an interpretation of quantum mechanics, it seems to me that Khrennikov’s paper does not sufficiently take into account several among the key aspects of this epistemology outlined above. In particular, Khrennikov’s argument concerning the statistical view of quantum mechanics (including uncertainty relations) vs. the view of it as a description of individual processes or events does appear to me to pay sufficient attention to one of the most crucial epistemological aspects of complementarity. It is a subtle point and is indeed easy to miss, in part since we tend to think of physical phenomena as pertaining to physical objects our theories are concerned with. Bohr, however, terminologically and substantively distinguishes observable phenomena, which manifest themselves only in measuring instruments and which are always classical, and quantum objects, which are never observable as such or, again, even conceivable in the sense of any possibility of giving them any conceivable specific characterization, be it conceptual, physical, or mathematical. Einstein, for example, did not adequately consider this aspect of Bohr’s view either. The question is how one understands or can possibly understand individual phenomena in dealing with the data of quantum mechanics. As I said, Bohr, in part in response to Einstein’s critique, was compelled to redefine the concept of the individual phenomenon as applied to quantum data. According to this view, quantum mechanics as complementarity only deals with individual phenomena in this sense. This is why Bohr’s post-EPR version of complementarity is so crucial and why it is perhaps the only fully developed version of it.

Now, if by individual processes and events one refers, as Khrennikov appears to do, to whatever happens at the level of quantum objects themselves, then, contrary to his claim, Bohr’s complementarity cannot be seen, including as regards uncertainty relations, as a theory of individual processes or events. This view of it is not possible for the simple reason that, as explained above, it does not describe any processes, individual or collective, at the quantum level, or, as I said, at any level, leaving the description (always partial) of the behavior of measuring instruments to classical physics. Indeed, as I stressed above,
complementarity sees such a description, or any analysis or reference to the properties of quantum objects or processes, as, in Bohr’s words, “in principle excluded” (PWNB 2:62). Complementarity is, however, a theory of individual phenomena, and indeed, in terms of physics, a theory only dealing individual phenomena, in a different sense. It provides rigorous predictions concerning occurrences of individual phenomena in Bohr’s sense and only concerning such occurrences. Each such phenomenon is defined in terms of a single classical effect, such as a “dot” on the screen in the double-slit experiment, of the interaction between the irreducibly indescribable quantum objects and indescribable quantum parts of measuring instruments upon the classically describable parts of measuring instruments. This, crucially, includes a rigorous specification of the conditions of a given experiment (PWNB 2:64). In Bohr’s interpretation, the influence of these conditions can never be eliminated in considering the outcomes of quantum-mechanical experiments in the way it can, at least in principle, be done in classical physics. Thus, a dot on the screen in the double-slit experiment would be seen as (a part of) a different type of phenomenon depending upon the possibility of knowing through which slit an “object” that left the dot (as a trace of its collision with the screen) has passed. (I underline “type” because, ultimately, each phenomenon is different and indeed unique in this interpretation.) If seen independently of the quantum-mechanical context of its appearance, each mark on the screen in the double-slit experiment would be perceived in the same way or as the same phenomena in the sense of the philosophical (say, Husserl’s) phenomenology of consciousness. Such a mark would appear as the same regardless of the difference in the conditions and, hence, outcome (“interference” or “no interference”) of the double-slit experiment. According to Bohr’s understanding, however, each mark is seen as a different individual phenomenon or as a part of a different phenomenon depending on and including these conditions, which are always mutually exclusive in the case, such as this, of complementary phenomena. In classical mechanics such conditions would of course be the same, assuming that we deal with particles (in the case of waves they would be the same but there would be no point-like traces and different equations, wave equations, would apply). Quantum-mechanical predictions, including numerical statistical predictions crucially depend on this distinction as well. (Mathematically, these predictions may be seen in terms of contextual probability in Khrennikov’s sense, although the physical content would be seen differently from the point of view of complementarity.) Thus, in the double-slit experiment, rather than dealing with two phenomena, each defined by a different multiplicity of spots on the screen, we deal with two distinct multiplicities of individual phenomena, defined by each spot. Each of the latter is indivisible—two sets of phenomenal
atoms or atomic phenomena in Bohr’s nonclassical sense—depending on two different sets of conditions of the experiment. One of these sets of conditions will lead to the emergence of the interference pattern, “built up by the accumulation of a large number of individual processes, each giving rise to a small spot on the photographic plate, and the distribution of these spots [following] a simple law derivable from the wave analysis” (PWNB 2: 45-46). Each single spot, however, must be, again, seen as a different individual phenomenon, which depends on the conditions in which the event occurs. Two different overall patterns, ”interference” and “no-interference,” pertain, thus, to two (very large) sets of different individual phenomena. Far from being a matter of convenience, this distinction between two multiple-spot phenomena and two multiplicities of spot-phenomena is essential for Bohr’s meaning and the consistency of his argumentation. First, no paradoxical properties, such as simultaneous possession of contradictory wave-like and particle-like attributes on the part of quantum objects themselves, are involved. Secondly, and perhaps most crucially, in our analysis we can never mix considerations that belong to complementary experimental set-ups in analyzing a given experimental outcome, even when dealing with a single spot on the screen, as we could, in principle, do in the case of classical physics. This is not an uncommon error (at least as Bohr’s interpretation and arguments are concerned), including in some of Einstein’s arguments, which could indeed lead to the appearance of paradoxes, on which point I shall comment in the next section in the context of the EPR argument and counterfactual logic. The latter, however, disappear once this rule of complementarity as mutual exclusivity of such considerations is followed. Throughout his arguments with Einstein, Bohr stresses in such situations, which are invoked in most of Einstein’s arguments, including of the EPR type, ”we must realize that . . . we are not dealing with a single specified experimental arrangement, but are referring to two different, mutually exclusive arrangements” (PWNB 2:57). These considerations clearly pertain to the question of counterfactual reasoning in quantum mechanics. They are also crucial in the context of contextual probabilities and, especially, their physical interpretation, and are, I would argue, not sufficiently addressed in Khrennikov’s paper.

Any further analysis of such phenomena is, again, “in principle excluded.” Accordingly, complementarity makes the unknowable—something that cannot in principle be known—part, indeed an irreducible part, of the knowledge that it provides, since this unknowable has fundamental effects upon what we can know. These effects are the data of quantum physics, according to complementarity. Given the particular character of the totality of such effects (e.g. in the double-slit or the EPR experiment), most such predictions, Bohr argues,
can only be statistical. As Bohr says, however, “It is most important to realize that the recourse to probability laws under such circumstances is essentially different in aim from the familiar application of statistical considerations as a practical means of accounting for the properties of mechanical systems of great structural complexity [as, for example, in classical statistical physics]. In fact, in quantum physics, we are presented not with intricacies of this kind, but with the inability of the classical frame of [physical] concepts to comprise the peculiar feature of indivisibility, or ‘individuality,’ characterizing the elementary processes [defined in terms of phenomena in Bohr’s sense, rather than in terms of quantum objects themselves and their behavior]” (PWNB 2:34). In other words, it is the irreducibly unknowable nature of quantum objects and their quantum interaction with the quantum parts of measuring instruments even in each individual case that are responsible for the irreducibly statistical character of quantum-mechanical predictions. By the same token, why a particular statistical counting works in quantum mechanics is in turn beyond the possible purview of the theory (interpreted as complementarity), in contrast to classical statistical mechanics, or elsewhere in classical physics where statistical predictions apply (as in predicting the probability in throwing dice. There the statistical behavior and particular ways of counting are explained from the classical, Newtonian, causal behavior of each individual constituent of a given multiplicity, and their interactions, although the large number and the structural complexity of the components (be they objects, forces, factors, or whatever) make it impossible to describe each such constituent. In quantum mechanics, as complementarity, this type of description is never possible even when the maximal information concerning any individual situation is available, and, again, no description of any kind concerning quantum objects themselves or their quantum interactions is ever possible. It is in this (physical) sense that quantum probabilities are irreducible, the sense, I would argue, not sufficiently considered in Khrennikov’s criticism of Bohr and complementarity, specifically as concerns the question of probability.

Some exact predictions are possible in quantum mechanics. They are, however, never sufficient to reconstitute a classical-like situation even at the level of measuring instruments, whose behavior in these experiments is, while classically described, subject to an only partial rather than complete classical treatment. The reason for this is that, in view of uncertainty relations, one can only establish at most half of the (conjugate) variables involved in and necessary for a complete classical description. It is worth stressing that quantum-mechanical statistical predictions involve particular (EPR) correlations and, hence, a certain form of order not found in classical physics. Accordingly, as indeed became apparent already in Planck’s Law, the statistical counting in
all quantum theories, including in quantum statistical theories, is not the same
as in classical statistical physics, even though mathematically one deals with
probabilities in both cases. (Whether quantum probability is Kolmogorovian
has been a matter of some debate, however. I leave aside the question of the
relationships between Kolmogorovian probability and contextual probability
in Khrennikov’s sense.) While each individual event, such as registering a
“dot” or a “click” (it actually has a complex structure), is a random event, the
collective accumulation (a temporal process!) exhibits a certain pattern, an "interference pattern" (using this term with caution, since there are no physical
waves involved), in a double-slit experiment. In this sense, again contrary to
Khrennikov’s argument, complementarity is a statistical interpretation. It is
an irreducibly statistical theory of both individual and collective (rather than
only collective) physical phenomena.

The uncertainty relations fit nicely into this scheme. Any measurement
could only involve a determination and indeed, according to Bohr, a definition
of one of the two conjugate variables for the relevant part of the measuring
instrument involved (once again, nothing else is subject to measurement or
prediction in complementarity), while Heisenberg’s formula would require sta-
tistical confirmation and may be also derived statistically. Accordingly, it does
not appear to me that there is any logical inconsistency here, as Khrennikov
appears to suggest. Let me note that to some degree, uncertainty relations are
a remnant of classical mechanics. Technically, one can see quantum mechanics
or, indeed even more so, quantum field theory, as a theory of statistical distri-
butions of certain individual events (dots, spots, traces, clicks, etc.), a theory
predicting such distributions, without any appeal whatsoever to uncertainty
relations or any mechanical variables of quantum objects.

I do see in Khrennikov’s paper an argument that the epistemology of com-
plementarity is unsatisfying (basically Einstein’s point) or at least that one
does not have to accept it and that alternatives could be offered, or, in view of
this dissatisfaction, should be (also Einstein’s point) offered. Indeed, Khren-
nikov’s main desiderata are very much those of Einstein. Quantum mechan-
ics was acceptable to Einstein as a statistical theory, while he hoped that a
classical-like, realist and preferably causal, alternative describing the individ-
ual behavior of individual quantum objects could eventually be found. Ein-
stein’s analysis of the situation itself was different from Khrennikov’s in that
Einstein did not see how quantum mechanics in its standard version could
account for individual events and processes at the quantum level, that is, in
terms of independent properties of quantum objects and processes, at least
without violating locality/relativity. (I put aside for the moment the role of
the EPR correlations in shaping his view, which I have considered in the works
cited above, and I, again, permit myself to refer to them.) Einstein saw quantum mechanics, including in Bohr’s interpretation, as irreducibly statistical in the sense of dealing only with ensembles (of quantum entities). Bohr tried to counter, along the lines here sketched, that it was, in his interpretation, also a theory of individual classical phenomena, that made any account of such phenomena (i.e. predictions concerning them, since nothing else is possible in Bohr’s interpretation) unavoidably statistical. He was not very successful in this argument (Einstein accepted only some of Bohr’s points). This is hardly surprising since Einstein was, at best, reluctant to see any theory not dealing with its ultimate objects, in this case quantum objects, as ultimately acceptable. He found Bohr’s view “so very contrary to his scientific instincts,” the statement that Bohr does not fail to cite (PWNB 2:61). Thus interpreted, quantum mechanics was almost not physics to Einstein (indeed I am not sure that “almost” is necessary here). In a way, he was not wrong, since (this is one of my main points in “Quantum Atomicity and Quantum Information”) Heisenberg’s “new kinematics” already renounced this type of description and dealt in fact or in effect with spectra as defined by the effects of the interaction between quantum objects and measuring instruments along the lines here outlined. This “kinematics” might have been better called “quantum informatics,” since it was not really kinematics, insofar as it did not deal with the motion of quantum objects. Einstein, however, also accepted that no such alternative was available, either in terms of a more acceptable interpretation of the quantum-mechanical formalism (which did not appear to him likely or even possible in view of locality considerations), or in terms of, at the time, alternative forms of formalism that would handle the data in question. He did not view Bohm’s 1952 theory as such an alternative, primarily, I think, in view of its inherently nonlocal character. Incidentally, Schrödinger’s view of the quantum-mechanical situation was pretty much the same. This apparent lack of rigorously developed alternatives also partially explains why Bohr and Heisenberg were sometimes less careful than perhaps they should have been as concerns the fundamentally interpretive character of complementarity, and sometimes dismissive of counterarguments concerning it, although Bohr patiently and painstakingly replied to all Einstein’s counterarguments.

Indeed, no such classical-like (realist and causal) and, this is again crucial (especially in the context of Bohmian mechanics), also local alternative may as yet be available. This is not to say that it is in principle impossible, although Bell’s and related theorems, such as the Kochen-Specker theorem, appear to make it nearly impossible. I would not go so far as to argue that there are no rigorously worked out or viable alternatives to complementarity, especially those dealing with the standard formalism, which would be difficult to do,
given the profusion of available interpretations, although I am tempted to think that this is the case. Indeed, it is not always clear how much of an alternative some of them really are, once they are rigorously considered or, to begin with, sufficiently worked out in terms of their physics, rather than hypothetical or semi-hypothetical proposals, which is indeed a crucial point. I comment on some among recent interpretations from this perspective in "Reading Bohr." Most of these interpretations are not causal or realist in the way Einstein wanted, or in the way some Bohmian theories are, although they, by and large, also want to dispense with the role of measuring instruments. It is, again, another question how successfully.

Let me say a few words concerning the Bohmian approach(es), which, I stress, are not quantum mechanics and deploy, in fact or in effect, a different mathematical formalism. I say “in fact or in effect” because certain versions of these theories, such as Bohm’s 1952 version, do in fact use the standard, say, Schrödinger’s, formalism, while explicitly assigning trajectories to particles (in these theories there indeed are particles as well as waves associated with individual particles), which the standard quantum mechanics does not do. This assignment, however, implies the presence of an additional differential equation not found in the standard theory. So, in effect, the formalism is different in this case as well. This equation introduced nonlocality, as well as causality (to the degree that one can speak of causality given nonlocality), into the theory, even though the theory, it is worth keeping in mind, is not classical either (i.e. is not a theory of the type of Newtonian mechanics and is indeed closer to quantum mechanics than to classical mechanics). This—nonlocality—is, in my view, the primary reason why the Bohmian approach is a minority, a small minority, view of quantum mechanics, although, as you observe, other factors play a role as well. There are arguments, such as those by Henry Stapp, that the standard quantum mechanics is nonlocal as well. These arguments, at least so far, have not been effective or, I would argue, sustainable. Accordingly, given the current state of physics, and specifically the extraordinary success of quantum mechanics and its extensions in quantum field theory in enabling theoretical predictions of the outcome of experiments, it does not appear to me that Bohmian theories are likely to have a greater impact in the current practice of theoretical physics. (Their mathematical aspects are obviously another matter). I might of course be wrong. I can hardly see Bohmian approaches as any less “romantic” in Khrennikov’s sense (I shall explain my emphasis presently) or, correlative, any more realist or realistic than Bohr’s—quite the contrary, in many respects it seems to me more romantic and less realistic, albeit realist. Bell used similar terms in his
criticism of Bohr et al., on which I commented elsewhere.

On the other hand, as a scholar of Romanticism (which is my literary field), I would argue that historically Romanticism, for example that of Hölderlin, Kleist, Blake, Shelley and Keats, is indeed epistemologically closer to Bohr than to most realisms. But this Romanticism may be more realistic or even more rigorously realistic, insofar as it is scrupulously attentive to the limits within which the concept of reality can and must apply, than most professed realisms. Both the dead nature and life, or the human mind and culture, may ultimately be better approached from this Romantic perspective. One cannot, however—this perspective also tells us—ever be fully certain.

3 The Letter of Copenhagen

In this section I shall comment on several specific points of Khrennikov’s paper, pertinent to the above argument, and most of my points here indeed follow more or less immediately from that argument. The page numbers below refer to those of Khrennikov’s paper.

Andrei Khrennikov (hereafter AK): “I do not think that understanding of . . . [the] contextual structure of physical theories . . . [i.e. that physical theories describe properties of pairs, physical system and measuring device] really was Bohr’s invention” (p. 2).

Arkady Plotnitsky (hereafter AP): It depends on what one means by contextual, more specifically, what is the epistemology of a given form of contextualism. Indeed, it seems to me that contextualism needs to be more sharply defined by Khrennikov, especially in physical terms and in relation to the dependence of the outcome of measurements on different experimental set-ups (such as those in the double-slit experiment). Insofar as contextualism is seen as the argument that the outcome of experiments, including statistical predictions (for example, in terms of contextual probabilities), fundamentally depends on a given experimental set-up, Bohr may well have invented it. He would of course question certain physical assumptions implicitly underlying Khrennikov’s argument for the role of contextual probabilities in quantum mechanics.

AK: “It was clear to everybody that physical observables are related to properties of physical systems as well as measuring devices” (p. 2).

AP: This statement (which needs to be more sharply formulated in any event) is true in one, more or less trivial, sense: it has indeed always been recognized that some measuring devices (even if only human organs) are nec-

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Arkady Plotnitsky, *Complementarity: Anti-Epistemology after Bohr and Derrida*, Duke University Press, 1994, pp. 182-85.
ecessary to establish and relate (observe, measure, etc.) such properties. On the other hand, in classical physics, this interaction between physical objects under investigation and measuring instruments could, in Bohr’s words, always be “neglected or compensated for,” at least in principle. In this sense the relevant properties of objects, such as, say, the position and the momentum of a given body, can be considered as independently existing and could, at least in principle, both be simultaneously determined within a single experimental arrangement, although in practice different arrangements are often used. By the same token, while classical physical theories must indeed rely on measuring instruments to confirm their descriptions and predictions, they describe, at least in principle, independent properties of physical systems under investigation, and predict the values of these properties. As I have explained above, this is not the case in quantum mechanics; and, this is, again, the crux of Bohr’s argument, where it is, at least in his interpretation, impossible to refer unambiguously to such properties. It is, in principle, impossible to dissociate quantum objects from their interaction with the measuring instruments involved and isolate them in the way it is possible, at least in principle, in classical physics. In Bohr’s interpretation, there are neither waves nor particles, nor indeed anything specifiable at the quantum level. One deals only with particular effects of the interactions between “quantum objects” and measuring instruments upon those instruments, if one can still, in all rigor, even use such terms as “quantum” and “objects,” although one can rigorously speak of measuring instruments, that is, their classically describable parts. The fundamental dependence of the outcomes of predictions on specific experimental set-ups is an immediate consequence. This, epistemologically radical or, as I like to call it, nonclassical, contextualism may well be Bohr’s (and Heisenberg’s) invention.

AK: “The main invention of N. Bohr was not contextuality, but complementarity. Bohr’s greatest contribution was the recognition of the fact that there exist complementary experimental arrangements and hence complementary, incompatible, pairs . . . . I think nobody can be against the recognition of such a possibility. Why not? Why must all contexts, complexes of physical conditions, be coexisting? Contextuality and complementarity are two well understandable principles (not only of quantum physics, but physics in general)” (p. 2).

AP: This statement does not seem to me to be altogether accurate. First of all, it, again, depends on what kind of contextuality one has in mind. As I said above, both contextuality (in the sense of the irreducible dependence of the outcome of an experiment on the experimental set-up and, as a result, the rigorous impossibility of ascribing anything to quantum objects or con-
sidering them apart from their interactions with measuring instruments) and complementarity (in Bohr’s rigorous sense of the irreducible mutual exclusivity of measuring procedures) are, according to Bohr’s interpretation, irreducible only in quantum, but not in classical, physics. In particular, as I said, in classical physics it is, at least in principle, possible to measure both the position and the momentum of a given body within the same experimental arrangement, although in practice different arrangements are often used. This is never possible in quantum mechanics in view of the uncertainty relations. It may be worth stating this point in more rigorously Bohrian terms, defined by the fact since no properties, such as those of position and momentum, could be attributed to quantum objects, not even each such property by itself. No quantum-mechanical measurement allows one both to fix both a space-time reference frame for any position determination and to measure a change of the momentum of the relevant parts of measuring instruments, in the way it could be done when we measure a classical object in classical physics. In quantum measurement (in this interpretation) we can do either one or the other, but never both together. Both this radical type of “contextuality” (this is of course not Bohr’s term, perhaps because in itself it does not convey the epistemologically radical nature of his idea) and complementarity may be seen as Bohr’s conceptions.

AK: “The real problem was that N. Bohr as well as W. Heisenberg (but merely further generations of their adherents) did not pay attention that quantum complementarity is the experimental fact concerning pairs

\[ \pi = (\text{elementary particle, macroscopic measuring device}) \]

and not elementary particles by their selves. It is a pity that the greatest promoters of contextualism forgot about contextual basis of complementarity” (p. 2).

AP: This is manifestly incorrect, since, as was shown in the preceding discussion, Bohr’s complementarity only concerns experimental arrangements in their interaction with “quantum objects.” (It can be shown that Khrennikov’s statement is not true about Heisenberg either.)

AK: “…complementarity of contexts in quantum physics does not imply complementarity of corresponding objective properties (of elementary particles) contributing into such observables” (p. 2).

AP: Bohr, as I explained above, never said that it would or, again, could, given that no such properties could be ascribed to quantum objects.

AK: “In particular, contextual complementarity does not imply that elementary particles do not have objective properties at all. In particular, there are no reasons to suppose that it is impossible to provide a kind of hidden variable, HV, description (ontic description . . . ) for these objective properties”
AP: As I said, for Bohr complementarity does in fact imply precisely this impossibility of speaking of independent objective properties of quantum objects, as Khrennikov indeed acknowledges later in the article. Of course, as I also said, this does not prevent one from making this type of presupposition, trying to pursue it, and connect it with, in Khrennikov’s terms, “contextual complementarity.” This is, for example, Bohm’s program. The question is how successful one is in this pursuit. Bohm’s theory may indeed be seen as achieving something of that type, but, again, at the expense of introducing nonlocality as a mathematical consequence of the theory. Hence, also the significance of Bell’s and related theorems in assessing such projects. And, as I said, it is not clear to me how $p$-adic hidden variable theories fare in this respect, or how they actually relate to experimental results, to begin with. Perhaps, they can reproduce quantum-mechanics predictions while avoiding nonlocality. Can they? Even if they can, it would still leave other problems as concerns relating this type of mathematics to anything physical. As Khrennikov acknowledges, this remains a hypothetical proposal: “the development of alternative (nonreal, noncontinuous) classical models, e.g. $p$-adic . . . , might play important role in clarification of foundations of quantum theory” (p. 3; emphasis added). They might, but then they also might not.

AK: “On the other hand, an adherent of N. Bohr [Plotnitsky] would argue that ‘Such a separation and, hence, the description of (properties of) quantum objects and processes themselves (as opposed to certain effects of their interaction with measuring instruments upon latter) are impossible in Bohr’s interpretation,’ ” (p. 3).

AP: Obviously, I agree, since this is indeed what I say; and I am grateful to Khrennikov for his choice of quotation here. I only direct one’s attention to the significance, a fundamental significance, of my qualification “in Bohr’s interpretation,” which must be viewed along the lines outlined above and which does not appear to me to be sufficiently taken into account by Khrennikov.

AK: “I think that the origin of such an interpretation of complementarity by N. Bohr was the individual interpretation of Heisenberg’s uncertainty [uncertainty relations?]: $\Delta q \Delta p \geq h/2$.”

AP: As I said, Khrennikov’s discussion of uncertainty relations appears to me to misconceive Bohr’s or (admittedly, this would require a separate discussion) Heisenberg’s view, as explained above and in more detail in my works cited earlier. In particular, Khrennikov says: “Unfortunately, Heisenberg’s uncertainty relation was interpreted as the relation for an individual elementary particle” (p. 3). First of all, one might ask: By whom, specifically? It was by some, to be sure, but certainly not by Bohr, at least not in this sense,
but only in the sense of individual phenomena, as explained above, that is, in
relation to certain parts of measuring instruments in two possible but always
mutually exclusive experimental arrangements. I leave aside Khrennikov’s his-
torical commentary, which is far too sketchy and incomplete, and is not really
germane to the argument.

AK: “The main problem was mixing by W. Heisenberg of individual and
statistical uncertainty. For example, in his famous book [The Physical Princi-
ples of Quantum Theory] he discussed the uncertainty principle as a relation for
an individual system, but derived this principle by using statistical methods!”
(p. 3).

AP: This sounds to me confused, at least as stated, including as con-
cerns the difference between the uncertainty principle and uncertainty relations, and,
in any event, this point is not sufficiently explained by Khrennikov. As I
said, one can, and in quantum mechanics perhaps must, rigorously combine
individual and statistical considerations. So it is not clear why this is a problem
or what the problem in fact is. This needs to be explained further as, in my
view, does most of Khrennikov’s commentary on uncertainty relations.

AK: “In any case the absence of continuous classical model for motion of
electron in Bohr’s atom does not imply impossibility to create other, noncon-
tinuous, classical (causal deterministic) models” (p. 4).

AP: Yes, but, again, the question is how effective such models are, and indeed
whether we really do have them or whether we can seem them as classical,
and in what sense. Indeed, it is quite clear that “mixing” or, I would say, con-
joining the classical and the continuous is not as misconceived as Khrennikov
appears to think (pp. 4-5).

AK: “Moreover, considerations of W. Heisenberg . . . [in his original matrix
mechanics paper] did not [even] imply [the] impossibility to create [a] continu-
ous classical model—as it was claimed by W. Heisenberg and then by N. Bohr.
The story is much simpler: first Bohr tried to create such a thing, but could
not; then Heisenberg, with the same result. After this it was claimed that
such a model did not exist. And what is the most interesting: not only for
Bohr’s model of atom (well it might be), but for any other model . . . I cannot
understand this kind of ‘quantum logic’ ” (p. 4).

AP: Again, claimed by whom, specifically? The nature of Bohr’s and
Heisenberg’s claims was, it seems to me, more complicated. But in any event,
my argument above concerning the interpretive nature of complementarity
obviously leaves space for a search for alternatives, even if, again, my view may
stress the interpretive nature of complementarity more than others (possibly
including Bohr and Heisenberg).

AK: “So Bohr’s complementarity was a kind of individual complementarity.
Complementary features were regarded [as belonging?] to individual physical systems. It is a pity that contextualists N. Bohr and W. Heisenberg related the uncertainty relation[s] not to some special class of measurement procedures of the position and momentum described by quantum formalism, but to the position and momentum of an individual elementary particle” (p. 5).

AP: As shown by the above argumentation, this is manifestly incorrect as stated, and appears to flatly disregard both Heisenberg’s and Bohr’s arguments, extending from Heisenberg’s new kinematics to Bohr’s post-EPR arguments for complementarity.

AK: “This imply the prejudice that the position and momentum even in principle could not be determined simultaneously and, moreover, that it is even in principle impossible to assign such a physical property, e.g. position or momentum, to e.g. electron: ‘electron does not have trajectory’” (p. 5).

AP: This is, in my view, not simply a prejudice (a view in part resulting perhaps from the preceding incorrect statement of Khrennikov’s paper, just cited), but, at least, an interpretation, and alternatives are not easy to offer. Bohmian theories, which have trajectories, are nonlocal, and indeed they are nonlocal because they have trajectories. And then, can one speak physically of p-adic, and hence discontinuous, trajectories? This may not be impossible, but it is not easy, assuming, again, that such theories could be properly developed, including as classical.

AK: “In fact, the only possible conscious [?] interpretation of Heisenberg’s uncertainty principle is the statistical contextual interpretation . . . . It is impossible to prepare such an ensemble of elementary particles that dispersions of both position and momentum observables would be arbitrary small. Everybody would agree that only this statement can be verified experimentally. Contextualism has to be statistical contextualism and, consequently, complementarity has to be statistical contextual complementarity. Such contextualism and complementarity do not contradict to the possibility of [a] finer description of reality than given by quantum theory” (pp. 5-6).

AP: It is true that they do not contradict this possibility. But, again, a possibility is not an actuality. It is Bohr’s interpretation of uncertainty relations that at any given point only one of the two conjugate variables involved (pertaining to certain parts of the measuring instruments used) could be defined rather than only measured with full precision within the capacity of our measuring instruments. This interpretation arises from his analysis of quantum measurement, including in the EPR case.

AK: “In particular, our contextual probabilistic investigations demonstrated that contextual complementarity, wave-particle dualism, is not rigidly coupled to microworld. Thus we can, in principle, perform experiments with
macro systems that would demonstrate ‘wave-particle duality,’ but not of macro objects, but contexts” (p. 7).

AP: Although one might want to see an example of such an experiment to be able to assess it, I merely observe here that there is no such duality in Bohr’s interpretation, and especially there are no (continuous) waves, as must be clear already in the double-slit experiment. I must add that this statement needs more lucidity in any event.

AK: “We have two theoretical descriptions of this experiment: 1) quantum-like statistical description; 2) Newtonian classical description. Both theories give the same statistical distribution of spots on the registration screen. Quantum-like theory operates with complex waves of probability; there is uncertainty, Heisenberg-like, relation for position and momentum. Of course, this relation is the statistical one. Suppose now that some observer could not provide the verification of Newtonian description, e.g. such an observer is a star-size observer and its measuring device produce nonnegligible perturbations of our macroscopic charged balls. Such an observer might speculate on [the] impossibility to find objective phase-space description and even about waves features of macroscopic balls” (p. 7).

AP: To repeat (this qualifications seems to be necessary at any point of Khrennikov’s paper), it is one thing to presuppose the possibility of a classical-like account and it is quite another to actually develop one. In addition, Khrennikov’s argument here seems to me either incorrect or insufficiently developed. I do not think that “both theories [quantum and classical] give the same statistical distribution of spots on the registration screen.” For, if one could, even in principle, count electrons, say, those passing through the slits in the double-slit experiments as Newtonian balls—as we indeed can, but only in one among the two possible complementary arrangements—there would not be an interference-like pattern of dots resulting from collisions between electrons and the screen. Thus, complementarity—the necessity of two different, mutually exclusive, arrangements for a comprehensive account of the situation we encounter in quantum physics—changes everything here or, rather, precisely reflects the difference, possibly irreducible, between the classical and the quantum. A Newtonian theory, at least any available Newtonian theory, does not appear to me to be able to account for this situation. Bohmian theories, which do account for it, but nonlocally, are not Newtonian.

There may also be an element of terminological and possible substantive confusion here, insofar as the “classical” configurations that Khrennikov refers to are in fact computer-generated models developed in discrete rather than continuous time and their status as classical models (if they indeed could be seen as classical) requires further explication. Let me, however, offer an argu-
ment, via Anthony J. Leggett’s elegant exposition, describing a different but equivalent experiment (in neutron interferometry), in which instead of slits we consider the initial state A, two intermediate states B and C, and then a final state E. (The latter is analogous to the state of a “particle” at the point of its interaction with the screen in the double-slit experiment.) First, we arrange to block the path via state C, but leave the path via state B open. (In this case we do not attempt to install any additional devices to check directly whether the object has in fact passed through state B.) In a large number (say, again, a million) of trials we record the number of particles reaching state E. Then we repeat the same number of runs of the experiment, this time blocking the path via B, and leaving the path via C open. Finally we repeat the experiment again with the same number of runs, now with both paths open. In Leggett’s words, “the striking feature of the experimentally observed results is, of course, summarized in the statement that . . . the number reaching E via ‘either B or C’ appears to be unequal to the sum of the numbers reaching E ‘via B’ or ‘via C’.” The probabilities of the outcomes of individual experiments will be affected accordingly. (This is of course also the situation that leads Khrennikov to his use of contextual probability.) The situation is equivalent to the emergence of the interference pattern when both slits are open in the double-slit experiment. In particular, in the absence of counters, or in any situation in which the interference pattern is found, one cannot assign probabilities to the two alternative “histories” of a “particle” passing through either B or C on its way to the screen. If we do, the above probability sum law (based in adding the so-called amplitudes, related to the wave-function, to which one applies Schrödinger’s equation, rather than, as in the classical case, probabilities themselves) would not be obeyed and the conflict with the interference pattern will inevitably emerge, as Bohr stressed on many occasions. This is also why the ways of counting probabilities are so different in classical and quantum physics, as Planck discovered. One may also put it as follows. We must take into account the possibility of a particle passing through both states B and C (and through both slits in the double-slit experiments), when both are open to it, in calculating the probabilities of the outcomes of such experiments. We cannot, however, at least in Bohr’s interpretation, assume either that such an event in space and time physically occurs for any single particle, anymore that we can assume that one can walk into a building simultaneously through two doors, when these doors are sufficiently far apart. The inherently quantum-mechanical nature of the EPR correlations may be linked to similar considerations as well. These considerations lead to considerable complica-

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\[\text{Anthony J. Leggett, “Experimental Approaches to the Quantum Measurement Paradox,”}\\ Foundations of Physics, 18, 9 (1988): 940-41.\]
tions in Bohmian theories, which indeed need to have both particles (having trajectories) and waves, associated with individual particles, to reproduce the predictions of the standard quantum mechanics, but, again, as a result introduce nonlocality into the theory. It seems to me that much of Khrennikov’s argument does not pay sufficient attention to these considerations or at least does not sufficiently address them.

It would be difficult to speculate how a possible “intelligent” observer with perceptual and reasoning capacities analogous to our own but of the size of a star would see the physical world in our universe, for example, an object of the size of a billiard ball. A more interesting question may be whether there could be “observers” (it is not inconceivable that they actually need to be very “large” rather than small) in our universe who would in fact ‘see’ anything on the quantum scale, that is, the scale defined by Planck’s constant $h$. I would think that, if they could, the picture would be quite fantastic by our standards. Perhaps, however, an irreducibly classical perception, such as our own (defined by the particular constitution of our bodies) is necessary in order to observe anything to all. My intuitive guess (which could of course be wrong) is that, given the ultimate physical nature of our universe, different possible “intelligent” beings in it might see different “classical-like” worlds, but could never see “the quantum world,” if, again, such terms as “quantum” and “world” could apply.

AK: “Finally, we remark that the possibility of $H3$-description implies that ‘quantum randomness’ does not differ essentially from ‘classical randomness.’ Of course, this contradict[s] to orthodox quantum views to randomness as fundamental or irreducible randomness. Unfortunately, I could not understand the latter ideas. Instead of fundamental irreducible quantum randomness, I prefer to consider well understandable theory of context (complex of experimental physical conditions) depending probabilities” (p. 7).

AP: This, at the very least, needs to be further explicated. It seems to me that Khrennikov’s argument is in essence of the Bohmian type, insofar as the statistical nature of quantum mechanics may only reflect our partial knowledge of a more classical-like configuration. This, again, may or may not be true, but so far there have been no effective arguments (at least for me and most physicists), in part in view of the nonlocality of Bohmian theories, that such is the case. As I said above, one would still need to account for the difference in counting, reflected already in Planck’s law. In Bohr’s interpretation, in any event, contextuality entails irreducible randomness in the physical sense explained above, and not really addressed by Khrennikov.

In his section, “Citation With Comments,” Khrennikov presents ‘some citations on orthodox quantum theory and our contextual statistical realist
comments. We use, in particular, collections of Bohr’s views presented in papers of H. Folse and A. Plotnitsky” (pp. 7-8). I think that part of the problem of this section is that the statements Khrennikov discusses are taken from different works, which are not always linked by Bohr and reflected very different stages of Bohr’s thinking. The purpose of this section of the article is unclear to me. I shall, however, comment on it in order to clear up some misunderstandings (some of them are not uncommon) that appear to me to affect Khrennikov’s commentary.

AK: (S1) “In contrast to ordinary mechanics, the new quantum mechanics does not deal with a space-time description of the motion of atomic particles. It operates with manifolds of quantities which replace the harmonic oscillating components of the motion and symbolize the possibilities of transition between stationary states in conformity with the correspondence principle,” N. Bohr.

This is simply the recognition of the restrictiveness of the domain of applications of quantum theory. I would like to interpret this as the recognition of incompleteness of quantum theory. However, it was not so for N. Bohr (p.8).

AP: Bohr specifically describes Heisenberg’s “new kinematics” here, which for the first time enabled correct predictions of the key experiments concerning electrons through a fully developed mathematical scheme, as opposed to the patchwork of the old atomic theory of Planck, Einstein, Bohr, and Sommerfeld.

AK: (S2) “...the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation,” N. Bohr.

The first part of this citation is the manifestation of contextuality. However, I cannot understand what kind of logic N. Bohr used to proceed to the second part. The second part can be interpreted as the declaration of the impossibility of objective, ontic description of reality (p.8).

AP: Bohr’s explains this more rigorously later in the same article, and better in his later works, along the lines explained above; this is cited from his Como lecture, where these ideas are as yet somewhat tentative. But even the Como lecture provides further explanation. It is very difficult to consider this and indeed most of Bohr’s statements apart from his overall argument.

AK: (S3) “...to reserve the word phenomenon for comprehension of effects observed under given experimental conditions ... These conditions, which include the account of the properties and manipulation of all measuring instruments essentially concerned, constitute in fact the only basis for the definition of the concepts by which the phenomenon is described,” N. Bohr.

I would agree if the last sentence would be continued as “is described in
quantum formalism” (p. 8).

AP: Bohr only deals with quantum mechanics here. Khrennikov appears to miss the context of Bohr’s statement. There was no other formalism at the time and it was this formalism that was used by EPR, to whom this argument replies. Indeed, rigorously speaking, such phenomena are, as I said, only predicted by using the quantum-mechanical formalism, and are physically described classically.

AK: (S4) “…by the very nature of the situation prevented from differentiating sharply between an independent behaviour of atomic objects and their interaction with the means of observation indispensable for the definition of the phenomena,” N. Bohr. I would agree if the last sentence would be continued as “of the phenomena described by quantum formalism” (p. 8).

AP: See my comment above, specifically my discussion of complementarity as only an interpretation of quantum mechanics. And, again, these phenomena are, in Bohr’s view, definitely not described by quantum formalism, which, in this view, describes nothing, nothing physical, at all.

AK: “Bohmian mechanics …—well, it has its disadvantages, but merely, mathematical” (p. 9).

AP: I would take issue with this assessment. The main (although indeed no the only) disadvantage of Bohmian theories is, in my view, their nonlocality, which is a physical feature, albeit a mathematical consequence, of the theory.

AK: “Finally, Bell’s inequality arguments were interpreted as they should be interpreted in the orthodox quantum framework, despite very strong counter-arguments. If all these counter-arguments be taken into account, Bell’s inequality activity would look very strange, as a kind of mystification.” AP: This is extremely unclear. The subject requires a great deal more explication in any event. It is indeed crucial to the problematic in question, and to the ongoing debate concerning quantum mechanics and the spirit and the letter of Copenhagen.

I would like to thank Andrei Khrennikov for this opportunity to discuss the spirit and the letter of Copenhagen, for other productive exchanges on quantum theory and related subjects, and for organizing two recent conferences, Quantum Theory: Reconsideration of Foundations (2001) and Foundations of Probability in Physics (2002), at Växjö University and for inviting me to participate in them. I am grateful to other participants of these conferences for many helpful discussions.