

# John Bell on ‘Subject and Object’: an Exchange

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## Abstract

This three-part paper comprises: (i) a critique by Halvorson of Bell’s (1973) paper ‘Subject and Object’; (ii) a comment by Butterfield; (iii) a reply by Halvorson. An Appendix gives the passage from Bell that is the focus of Halvorson’s critique.

## Part I: John Bell on Subject and Object; by H. Halvorson

It’s quite amazing that in the span of four short pages, John Bell can make the pioneers of quantum mechanics seem collectively like just so many addle-brains. I’m speaking here of Bell’s article “Subject and Object”.<sup>1</sup> I cannot deny the rhetorical effectiveness of this article. In fact, I consider it a model for how one can — with the effective application of insinuation and rhetorical question — render a view seemingly unworthy of serious consideration. Nonetheless, I cannot hold Bell’s paper up as a paradigm of philosophical inquiry, because he gives so little effort to understanding what others were saying. We can do better, and we must do better, if we’re ever going to make progress with the foundations of quantum physics.

Bell begins his article by claiming that:

1. Quantum mechanics is fundamentally about the results of “measurements”.
2. The subject-object distinction is needed for quantum mechanics, but
3. “Exactly *where* or *when* to make it [i.e. the subject-object distinction] is not prescribed.” (p 40)

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<sup>1</sup>The paper was presented at a symposium in honour of Dirac in September 1972, published in *The Physicist’s Conception of Nature* (ed. J. Mehra) in 1973, and reprinted in Bell’s collection (1987; second edition 2004, with the same pagination). Compare the Appendix.

Bell then says that (3) is a serious defect that makes quantum mechanics “vague” and “intrinsically ambiguous” and “only approximately self-consistent.” (We have included the complete text of the relevant passage in an appendix.)

Let me begin by saying that I simply deny (1), i.e. that quantum mechanics is fundamentally about the results of measurements. I’m afraid that Bell has himself made a logical leap from “the quantum mechanical formalism needs a user” to “quantum mechanics is fundamentally about the results of measurements.” There is a wide range of possibilities between these two extremes — e.g. that the quantum-mechanical formalism provides a means for translating facts about subatomic reality into a language that human beings can understand.

I will grant that Bell is correct about (2), that the subject-object distinction is needed for quantum mechanics, but unfortunately, Bell has misunderstood the sense in which it is needed. He seems to think that quantum mechanics must describe the world as bifurcated into two parts — subject and object. If that were correct, then I would completely understand Bell’s unease with the distinction. If the theory describes a world with two parts, then the theory should offer some guidance about what belongs to each part.

But if you think about the meaning the word “subject”, it quickly becomes obvious that it’s not supposed to play the role of a predicate in the theory (unlike, say, “electron”). Rather, the idea is that a subject uses the theory to describe objects — and in the case at hand, these objects fall under the laws of quantum mechanics. The theory sees no subjects, it sees only objects, and so it has no need for specifying *where* and *when* the subject-object split occurs. Such a split is a necessary prerequisite to physical theorizing, when a subject decides to use a theory to try to say something true about the world.

Now what about the complaint that quantum mechanics does not specify who the subject is, or when and where and how she decides to use the theory? But wait a minute. Is there any theory that does that? What an amazing theory it would be! Indeed, such a theory would fulfill Hegel’s aspiration of finally unifying the subject and object. In other words, such a theory would “theorize itself.” Is Bell suggesting that quantum mechanics is defective because it doesn’t yet achieve the Hegelian *Aufhebung* of the subject-object distinction?

So, in short, Bell is correct that quantum mechanics, as it stands, needs a subject. But that is true of every theory that has ever appeared in physics — i.e. these theories need subjects to decide when and where and how to describe things.

Bell’s subsequent rhetoric in the article is effective only against the backdrop of his false assumption that the subject must appear *in* the quantum-mechanical description. For example, Bell raises a question for which quantum mechanics doesn’t appear to have an answer.

Now must this subject include a person? Or was there already some such subject-object distinction before the appearance of life in the universe? (p 40)

But quantum mechanics is simply not interested in the question of what counts as a subject. If you ask me what counts as a subject, then my answer is that anyone who can use a theory to describe things is a subject — no other qualifications are necessary! If your dog can theorize, then he is a subject, and if an artificial intelligence could theorize, then it would also be a subject. And to Bell’s second question, I suspect that before the appearance of “life” in the universe, there were no things that could describe other things, and hence no

subjects. But that doesn't mean that we subjects, living today, cannot describe the universe as it was before the existence of any subjects. In fact, the entire point of the subject-object distinction is that when a subject  $S$  is treating some  $X$  as an object, then it is indifferent to  $S$  whether  $X$  is also a subject — because as far as  $S$  is concerned,  $X$  is merely an object.

If you now ask me, but is  $X$  *really* a subject or an object? Here I say that the question is misguided. Those two categories are not mutually exclusive. Without a doubt, each subject in our world can be an object of some subject's description. So perhaps what you want is a more comprehensive theory that answers the question of who or what can be a subject. But then who would be the subject who uses that theory, and must she wait for the theory to tell her that she is a subject before she can make use of it? I feel that we have now swum into deep metaphysical waters. For the business of physics, is it not enough that the subjects know who they are?

Due to misunderstanding the role of the subject in quantum mechanics, Bell also falsely accuses quantum mechanics of being “*intrinsically* ambiguous and approximate” (p 41, emphasis in original). If quantum mechanics does not describe a world split into subject and object, then where is the ambiguity supposed to appear? If Bell says that the ambiguity arises in what quantum mechanics is intended to describe — i.e. what counts as the object — then I would ask how that is different from any other physical theory. Take one of Bell's favorite theories: Bohmian mechanics. What is Bohmian mechanics supposed to describe? You might say: it describes particles following deterministic trajectories. But then I would ask: which particles, and which trajectories? You see, even in Bohmian mechanics, it's left to the discrimination of the theoretical physicist to decide how many particles, which Hamiltonian, when the interaction turns on and off, etc.<sup>2</sup> So, if standard quantum mechanics is “*intrinsically* ambiguous and approximate” how is that not also the case for Bohmian mechanics?

In “Subject and Object”, Bell slices and dices his opponent — a straw person of Bell's own making. The real problem, I think, is that Bell wants a theory that has no need for a subject.

## Part II: Comment on Halvorson; by J. Butterfield

### 1 Introduction

In this Comment, I will maintain that Halvorson's criticisms are unfair to Bell. As an introduction, let me begin with the charges in Halvorson's opening paragraph: that Bell made ‘the pioneers of quantum mechanics seem ... like ... addle-brains’, and that he gave ‘so little effort to understanding what others were saying’. I maintain that Bell is innocent of the charges. My reasons, in short, are that:

(i): Bell's discussion in this paper of ‘subject’ and ‘object’—meaning, as he explains, ‘measurer’ and ‘measured system’—is a brief statement of the measurement problem; as is

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<sup>2</sup>Consider, for example, the Bohmian description of a momentum measurement: According to Norsen, “one could ‘turn off’ the potential energy  $V(x)$  which confines the electron to the vicinity of the origin ...” (Norsen 2017, p 196). To echo Bell's question, exactly *where* and *when* is the potential energy turned off?

announced by Bell’s opening sentence: ‘The subject-object distinction is indeed at the very root of the unease that many people still feel in connection with quantum mechanics’ (1987, p. 40).

(ii): Bell does not shirk his duty to try to understand what the pioneers of quantum mechanics said about the measurement problem. On the contrary, he starts his third paragraph by saying that they were aware of these questions ...

... but quite rightly did not wait for agreed answers before developing the theory. They were entirely justified by results. The vagueness of the postulates in no way interferes with the miraculous accuracy of the calculations. Whenever necessary a little more of the world can be incorporated into the object. In extremis the subject-object division can be put somewhere at the ‘macroscopic’ level, where the practical adequacy of classical notions makes the precise location quantitatively unimportant.

(iii): Besides, one must of course read this paper—like any paper—in context. And Bell’s other papers contain longer statements of: both (a) the measurement problem, including in the terms used here, viz. the ‘ambiguity’ about where the ‘cut’ between object and subject, or ‘measured system’ and ‘measurer’, should be made (which he elsewhere calls ‘the shifty split’); and (b) the pioneers’ exertions over, and insights about, the measurement problem.<sup>3</sup>

But what about the substance of Halvorson’s criticism of Bell? Halvorson is very clear. His main criticism is that Bell has made a false assumption: that the subject, i.e. the measurer or measuring system, must appear *in* the quantum-mechanical description. I maintain that this criticism is wrong. Bell does not assume this. What he *does* do—here and in other papers (cf. footnote 3)—is to contrast:

(a) what he sees as the happy situation in classical physics: that there seems no obstacle, in principle, to a classical physical description of measurement processes that successfully describes getting definite measurement results;

(b) what he sees as the unhappy situation in quantum physics: that there seems to be an obstacle, in principle, to a quantum physical description of measurement processes that successfully describes getting definite measurement results—this is the measurement problem.

I do not mean to put Bell on a pedestal, or treat him as the fount of all wisdom about interpreting quantum mechanics. To be sure, his discussions are brilliant and his work on

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<sup>3</sup>Agreed, the papers with the best-known of these longer statements of (a) and (b) were written after ‘Subject and Object’: for example, ‘On the impossible pilot-wave’, ‘Speakable and unspeakable...’, ‘Six possible worlds...’, and ‘Against measurement’ (Chapters 17, 18, 20 and 23 of (1987/2004)). But I thank Chris Timpson for pointing out to me that one also finds earlier statements, in ‘The moral aspect of quantum mechanics’ (from 1966: Chapter 3), in ‘On the hypothesis that the Schrödinger equation is exact’ (from 1971; the revised 1981 version, called ‘Quantum mechanics for cosmologists’, being Chapter 15) and, more briefly, in Section 1 of ‘Introduction to the hidden variable question’ (1971, Chapter 4). Note also: (i) Bell’s 1989 Trieste Lecture, which talks in detail about ‘the shifty split’ and about Dirac (cf. Bassi and Ghirardi (2007)), and (ii) the quotes in Ghirardi’s touching memoir (2014).

non-locality was, obviously, epoch-making. (And speaking for myself: his realist philosophical outlook is music to my ears.) But a good case can be made for some views that he gave short shrift to. One main example is Bohr’s doctrine of the necessity of classical concepts: whose formulation and defence has been deepened in the intervening years, notably by Halvorson himself (e.g. Halvorson and Clifton 2002) and by Landsman (2006, 2017 especially Introduction).

So in the rest of this Comment, I will: reply to Halvorson’s criticism, by expanding on the contrast between (a) and (b) (Section 2); and conclude more positively (Section 3).

## 2 Reply to Halvorson’s criticism

Let us recall Halvorson’s opening trio of claims that he attributes to Bell. Halvorson writes:—

Bell begins his article by claiming that:

- (1): Quantum mechanics is fundamentally about the results of ‘measurements’.
- (2): The subject-object distinction is needed for quantum mechanics, but
- (3): “Exactly where or when to make it [i.e. the subject-object distinction] is not prescribed.” (p 40)

Bell then says that (3) is a serious defect that makes quantum mechanics “vague” and “intrinsically ambiguous” and “only approximately self-consistent.”

Let me begin by saying that I simply deny (1) . . .

I will focus on Halvorson’s discussion of his (2), and thereby (3). For I believe the apparent Bell-Halvorson disagreement over (1) need not detain us. For I think it is clear that for Bell, (1) has two roles: but the first prompts no dispute and the second is covered by the dispute over (2) and (3).

The first role of (1) is that Bell evidently intends it as a report of the orthodox ways of thinking about quantum theory, not as his own view. In this role, (1) just helps set up Bell’s discussion. In the second role, (1) serves to introduce measurement results as an undeniable focus of the enterprise of physics: physics is undeniably in the business of accounting for measurement results. Here, I say ‘account for’ to cover indifferently: (i) prediction (and retrodiction), definite or probabilistic, and-or (ii) explanation, and-or (iii) other relations of ‘meshing’ between the claims of a physical theory and empirical phenomena, such as confirmation.

Neither I nor Bell need to choose between these. For of course, in this second role, (1) is introducing the measurement problem. That is: it stresses the pervasive and detailed success of classical physics in attributing to all objects that it is applied to, definite values for all the quantities appropriate to them. This is often summed up in the slogan that ‘measurements have definite results’; or that according to classical physics, they do. But of course, all parties agree that the point at issue goes far beyond measurements, and encompasses all objects, measured and unmeasured, to which classical physics successfully applies. Accordingly, in view of classical physics’ supreme success in describing macroscopic objects as having definite values for all their quantities, the point is often summed up as: ‘the definiteness of the macro-realm’.

And *this point* yields the quantum measurement problem. For there is an argument—the familiar one: Schrödinger’s argument about a cat!—that this point is incompatible with quantum physics. More precisely: it is incompatible with the quantum dynamics of a strictly isolated system being unitary. And it is no escape from this quandary to point out that the cat (i.e. the pointer of an apparatus set to measure a quantity on a micro-system that is in a superposition for that quantity) is not strictly isolated, since it is interacting with, for example, air molecules, and indeed the CMB. For the official quantum state of the cat (or pointer), after the poisoning/measurement process, that is obtained by tracing out its environment—although it is mathematically a mixture—cannot be given the ignorance interpretation. In d’Espagnat’s terminology: it is an improper (not proper, i.e. ignorance-interpretable) mixture.<sup>4</sup>

Given all this; what about ‘subject’ and ‘object’, i.e. Halvorson’s (2) and (3)? As I announced in Section 1, and we read in Halvorson’s text: his main claim against Bell is that Bell falsely assumes that the subject must appear *in* the quantum-mechanical description. But I submit that Bell does not assume this. Rather, he emphasises a contrast between quantum and classical physics. As I put it at the end of Section 1: in quantum physics, there seems to be an obstacle, in principle, to a quantum physical description of measurement processes that successfully describes getting definite measurement results.

In other words: there is an argument (Schrödinger’s argument about a cat) that quantum physics cannot recover, or secure, the definiteness of the macro-realm. For a suitable ‘diabolical device’—a ‘ridiculous case’: both are Schrödinger’s phrases (1935, p. 328)—could propagate the *indefiniteness* of the micro-realm into the macro-realm. On the other hand, within classical physics, there seems to be no such obstacle, no such argument: measured systems can be coupled to apparatuses, in such a way that the definite values of their quantities can be registered by those apparatuses’ pointers.

Besides, this contrast can be ‘pushed inside the head’, if we so wish—and as the jargon of ‘subject’ and ‘object’ suggests it might be. So far, despite talk of ‘measurement’ with its connotations of human activity and cognition, it is the *inanimate* macro-realm, such as the definite positions of pointers, that I have emphasised. But (notoriously!) some authors suggest we should push ‘von Neumann’s chain’—the successive coupling of systems, correlating appropriate eigenstates, so as to get a many-component entangled state (cf. von Neumann 1932, Chapter VI.1 p. 418-420)—inside the head, and thereby consider the quantum mechanical description of the neural correlates of experience, e.g. seeing the black pointer inclined leftward against a white background vs. seeing the black pointer inclined rightward against a white background. If we concur with these authors, and countenance such a many-component entangled state as the complete physical description, we seem to face a looming threat of ‘indefinite, or superposed, experiences’. And we must choose between two broad options for avoiding the threat, i.e. for securing definite appearances of e.g. a pointer. That is: for securing an *apparently* definite macro-realm. Either we adopt an Everettian viewpoint (in a broadly ‘many-minds’, rather than ‘many-(inanimate)-worlds’, version); or

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<sup>4</sup>D’Espagnat suggested these terms in (1976: Chapter 6.2). Nowadays, the point is often made in the literature on decoherence (e.g. Zeh, Joos et al. (2003, p. 36, 43); Janssen (2008, Sections 1.2.2, 3.3.2)). But it is humbling to recall that the point was already clear, and beautifully expressed, in Schrödinger’s amazing 1935 papers: cf. especially the “cat paradox” paper’s analogy with a school examination (1935, Section 13, p. 335f.).

we say that ‘consciousness collapses the wave-function’.<sup>5</sup>

But in this paper, I of course do not need to choose between these options. For I come, not to solve the measurement problem, but only to praise it: or at least, to prevent it being buried ... Here, my point is—as it was in my discussion of the inanimate macro-realm—that in classical physics, there seems to be no such obstacle, no such argument, against maintaining both:

- (i) all experiences being definite, and
- (ii) there being a complete physical description of the neural correlate of any experience.

Just think of modern psychophysics, with its reliance on neurophysiology formulated wholly in classical-physical terms, e.g. with stick-and-ball models of the underlying biochemical molecules. Think in particular of Hubel and Wiesel’s 1950s investigations of vision. They found that in the visual cortex of a cat (*sic*), a single specifically-located neuron is dedicated to firing in response to an edge being aligned at a certain angle from the vertical (say, 20 degrees, as vs. 10 or 30) in a certain region of the visual field (say, the top-left region). Indeed, one can imagine the edge in question being a black pointer inclined leftward against a white background. Besides, the firing of the neuron is understood in classical neurophysiological terms as an electrical impulse, underpinned by sodium and potassium transport. So it fires—or it doesn’t. The cat detects the edge (the pointer) inclined leftward at 20 degrees from the vertical—or it doesn’t.

This completes my Comment on Halvorson. But there are three ancillary points that are worth making ...

## 2.1 Philosophy, history and the pilot-wave

The first two points are about the classical case. The first is about avoiding some philosophical commitments; the second is a historical point about the success of science supporting philosophical materialism. The third point is about the quantum case, and ‘extra variables’.

(1): Note that our ‘no worries’ attitude, for classical physics, about describing the ‘subject’, even experiences themselves, does not require: (a) the coherence of describing at once all the subjects in the cosmos; nor (b) philosophical materialism.

As to (a), I have gestured at how, in a world described by classical physics, ‘ordinary’ i.e. conceptually unproblematic empirical enquiry would be able in principle to discover the detailed physical description of any object or event, including measurement results; and even if one interprets ‘measurement’ in terms of experience, nothing in principle prevents classical physics from describing the neural correlates of experience. But this does not commit us to saying that in such a world, classical physics could describe it “all in one go”. It is of course a matter of the order of quantifiers: ‘for each object and event, there could be a classical physical description’ does not imply ‘there could be a single classical physical description, for all objects and events’.<sup>6</sup>

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<sup>5</sup>Famous examples of these two broad options include Zeh (1970) and Wigner (1962), respectively. Bell’s ‘Six possible worlds of quantum mechanics’ (1987/2004, Chapter 20) is a breezy introduction to both options, among others.

<sup>6</sup>As it happens, I do not have qualms about the sort of classical cosmic inventory envisaged by the last

As to (b): nothing I said about neural correlates of experience (in a world described by either classical or by quantum physics) requires philosophical materialism. I take materialism to be a thesis of supervenience or determination. It says, roughly speaking, that all the facts about the whole cosmos supervene on, or are determined by, the facts as described by the natural sciences. (I here take ‘natural sciences’ to encompass physics, chemistry and biology, on a par, i.e. with no special status accorded to physics: I will return to this in (2).) But I will not need to pursue a precise formulation of materialism. For me, the main point is that materialism is meant to *exclude* all non-natural-scientific properties and relations, even ones that are strictly nomologically correlated with some natural-scientific property or relation. Such properties and relations—non-natural-scientific but nomologically correlated with the natural-scientific—are invoked by some traditional *anti*-materialist views, like epiphenomenalism and property-dualism. So the point here is that what I said about the neural correlates of experience does not exclude such properties, or such views. This leads in to (2).

(2): Notwithstanding my liberal tolerance, in (1), of epiphenomenalism:— Consider the vast success since about 1850 of the natural sciences, i.e. physics, chemistry and biology, in describing and explaining phenomena, including mental phenomena. Think of the rise of physiology and psychophysics (in the mid-nineteenth century: figures like Bernard and Helmholtz), the decline of vitalism in biology, the rise of biochemistry and molecular biology. And think of how physics has provided an ever more detailed underpinning of chemical and biological phenomena (and so also, it seems: of mental phenomena).

These developments have undoubtedly prompted philosophers to formulate philosophical materialism; and also prompted many of them to defend the doctrine, thus formulated. (Of course, that is as it should be: positions debated in academic philosophy should reflect—make precise, and improve!—currents in the wider intellectual culture.) And hence, my unblushing statement a few paragraphs above of my main point. Namely: in classical physics, there seems to be no obstacle, no argument, against (i) all experiences being definite, and (ii) there being a complete physical description of the neural correlate of any experience.

I said it unblushingly, precisely because of the rampant success of classical neurophysiology. In 1850, or even in 1900, it could not have come so trippingly off the tongue.<sup>7</sup>

(3): In my Comment on Halvorson—my Bellian attempt to prevent the measurement

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sentence. I agree, of course, that it might well be infinite, and so ungraspable by human minds. But I do *not* take the propositions—the descriptions, the items in the inventory—to be a part of the cosmos described; and so there is no problem of the inventory itself having to be listed, or of self-reference or regress. But if you have such qualms: rest assured that nothing I, or Bell, have said commits one to such an inventory.

<sup>7</sup>I surmise that von Neumann was articulating the same confidence about the conceptually unproblematic status of classical psychophysics when in his (1932: Chapter VI.1 p. 419) discussion of measurement, he talked about the ‘psycho-physical parallelism’ being undercut by a *quantum* measurement: a confidence that, I say, was by 1932 well warranted. The point is also familiar in the history of analytical philosophy of mind. Recall McLaughlin’s much-cited account (1992) of how ‘British emergentism’, represented by e.g. C.D. Broad in the mid-1920s, declined not least because the rapid successes of quantum chemistry (e.g. London and Heitler’s work on covalent bonding) undercut Broad’s conjectured configurational forces.

Recall also Shimony’s vivid phrase, ‘*closing the circle*’, for the endeavour of recovering the ‘manifest image’ of the world—including the definite macro-realm, or at least definite appearances—from the ‘scientific image’ of it. In these terms, my point is that closing the circle seems easier in a world described by classical physics, rather than by quantum physics.

problem being buried—I assumed throughout that an appropriate many-component entangled state was the complete physical description to be considered. For example, when I ‘pushed the subject-object distinction inside the head’, it was this assumption that led to the looming threat of indefinite, or superposed, experiences. Of course, many advocates of proposed solutions to the measurement problem will deny this assumption, and announce this denial as their first step on the road towards their preferred solution. I of course say: ‘More power to you, and good luck, in developing your preferred solution. I have no brief to defend the assumption—I only made it, so as to better locate Bell’s dialectical position in 1973, and to defend him as innocent of the charges laid against him’.

But Halvorson’s mention of the pilot-wave (in the penultimate paragraph of Part I) prompts a final comment. Halvorson stresses that for the pilot-wave theory, as for any physical theory including orthodox quantum theory, an application of it focusses on a part of the world, leaving other features, such as the specification of the potential to which the quantum system is subjected, as an ‘external’ issue, ‘put in by hand’, or ‘up to the theorist or experimentalist’.

With which I agree: indeed so. But I—and the pilot-wave theorist—then add that this similarity between the pilot-wave theory and orthodox quantum theory (and indeed any physical theory) is neither here nor there. For a solution to the measurement problem—whether the pilot-wave solution or another—in no way needs to deny this innocuous role of an ‘external subject’. What matters is to have—which the pilot-wave theorist claims to have—a solution to the measurement problem: facts that secure a definite macro-realm—for example (in the simplest and most familiar version of the pilot-wave theory), the definite positions of point-particles.<sup>8</sup>

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<sup>8</sup>Two supplementary comments. (1) Having broached the topic of the neural correlates of definite experiences, I should note a misgiving about this solution. Even if one accepts that the definiteness of the inanimate macro-realm is a matter of point-particles’ positions being *here* rather than *there*, the pilot-wave theory, in order to secure our having definite experiences, presumably requires that an experience being definite—one way rather than another—involves point-particles being in one wave-packet rather than another. But that seems hard to line up with, for example, an edge-detector cell in a cat’s visual cortex either firing or not. For discussion, cf. e.g. Brown and Wallace (2005: Section 7, p. 533–537).

(2): I thank Ronnie Hermens for pointing out that Halvorson’s stressing that any theory ‘needs users’ (i.e. leaves features outside the described system as ‘external’ and ‘up to the physicist’) is echoed in the non-locality literature, especially in the wake of the Jarrett-Shimony distinction between parameter independence and outcome independence. In particular, Seevinck and Uffink make explicit the different theoretical roles of apparatus-settings and outcomes, when they write ‘to specify how probable it is that Alice will choose one setting [] rather than [another ...] would be a remarkable feat for any physical theory. Even quantum mechanics leaves the question what measurement is going to be performed on a system as one that is decided *outside* the theory, and does not specify how much more probable one measurement is than another’ (2011, Section III.B). I would add that besides, one can ‘shift the split’ i.e. ‘move the cut’. That is: one can instead model an apparatus-setting, and the act of choosing a setting, as a deterministic function of the state of the world, and then recover Bell’s theorem, and cousins like the Free Will theorem, by assuming these functions have suitable kinds of independence (Cator and Landsman 2014; especially pp. 784–786; Landsman 2017a, especially pp. 101–102). And for a recent judicious defence of the idea that setting *dependence* is tenable, I recommend Hermens (2019).

### 3 Conclusion

I rest my case: I urge that Bell is innocent of the charges laid against him. But let me end on a more positive and forward-looking note. There is a major part of Bell’s paper ‘Subject and Object’ that neither Halvorson nor I have touched on. But I should do so, since it answers Halvorson’s clarion-call in his first paragraph (quoted in Section 1) that ‘... we must do better, if we’re ever going to make progress with the foundations of quantum physics’.

After urging the measurement problem in the way that Halvorson has criticised and I have defended, Bell goes on to:

(i) state his distinction between ‘observable’ and *beable* (so far as I know, this is the first paper to advocate the jargon of ‘beables’);

(ii) sketch how one might formulate a Lorentz-invariant quantum theory, in which a select subset of observables (i.e. conventional physical quantities) are promoted to be beables.

Thus he writes

Many people must have thought along the following lines. Could one not just promote some of the ‘observables’ of the present quantum theory to the status of beables? The beables would then be represented by linear operators in the state space. [footnote suppressed] The values which they are allowed to *be* would be the eigenvalues of those operators. For the general state the probability of a beable *being* a particular value would be calculated just as was formerly calculated the probability of *observing* that value. The proposition about the jump of state consequent on measurement could be replaced by: when a particular value is attributed to a beable, the state of the system reduces to a corresponding eigenstate. It is the main object of this note to set down some remarks on this programme. Perhaps it is only because they are quite trivial that I have not seen them set down already.

... and so on!

Tempting though I find it to quote the page-long sketch that follows (and that concludes Bell’s paper), I will forebear. Suffice it to say that the sketch exemplifies precisely the line of thought that led to various later efforts to formulate a Lorentz-invariant, “no-collapse” but “one-world”, quantum theory. These efforts are many and varied. They include of course work by Bell himself; but also work since Bell’s death, for example on the modal interpretation—and by Halvorson himself, such as Halvorson and Clifton (1999). And the tradition continues: for example, in Kent’s recent proposals (2014, 2015, 2017).

This is not the place to report details of these efforts.<sup>9</sup> But I mention them (along with, of course, some of the other work cited above, e.g. in footnote 8) in order to convey a positive message to Halvorson, and to the reader: we *can* make progress with the foundations of quantum physics.

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<sup>9</sup>For example, Butterfield and Marsh (2018, 2019) discuss Kent’s proposals.

## 1 Epistemological Lesson $\geq$ Measurement Problem

There is much to thank John Bell for. He is responsible, to a great degree, for the rebirth of the foundations of physics that occurred in the 1960s, after the more pragmatic period around World War II. Nonetheless, historians and philosophers of science have an important responsibility to contextualize and assess Bell's contributions, and to point out cases, if there be any, where he got things wrong. It was to that end that I attempted to initiate a discussion with a brief critique of Bell's "Subject and Object." I'm grateful that Jeremy Butterfield joined the discussion, and I hope that it will further clarify Bell's role in physics and its philosophy.

Let me come straight to the point. I am in complete agreement with Butterfield about the value of clear, rigorous, and honest thinking about the foundations of physics. In particular, we should not turn a blind eye to genuine foundational problems. If Butterfield and I have any disagreement, then, it is primarily about where to focus our efforts. While Butterfield wants to keep the measurement problem alive, I want to resurrect the "epistemological lesson" of quantum mechanics that Niels Bohr spent forty years trying to articulate (see e.g. Bohr 1928), and John Bell spent thirty years trying to bury.

Bell's smear campaign against Bohr has been far more successful than he could have dreamed: these days it is all too easy to find articles and books — even bestsellers! — where Bohr is described as dogmatic, unphilosophical, obfuscating, uninterested in questions of reality, etc. And it is increasingly difficult to find literature where Bohr is treated as an important thinker (as Einstein treated him). I will make three claims about this sad state of affairs, where propaganda has prevailed over accuracy: First, these kind of slurs about a person's intellectual character have no place in serious philosophical discussion. Second, the objective historical evidence gives a rather different picture of Bohr than the one Bell painted, a picture of a person deeply engaged with questions about reality, but without being dogmatic that he had the answers.<sup>10</sup> Third, and most relevantly, this trash-talking of Bohr begins with the polemical works of John Bell — a man who never once spoke with Bohr, and who had little knowledge of Bohr's intellectual context.<sup>11</sup> The picture of Bohr as dogmatist, obscurantist, intellectual bully, etc. is a creative fiction of John Bell.

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<sup>10</sup>Just a couple of examples: Einstein in a 1954 letter: "[Bohr] utters his opinions like one perpetually groping and never like one who believes he is in possession of definite truth." Schrödinger in a 1926 letter: "There will hardly again be a man who has achieved such enormous external and internal success, who in his sphere of work is honored almost like a demigod by the whole world, and yet who remains — I would not say modest and free of conceit — but rather shy and diffident like a theology student. . . . this attitude works strongly sympathetically in comparison with the excessive self-confidence that one often finds in the medium-sized stars of our profession. . . . [Bohr] is so very considerate and is constantly held back by the fear that others could take an unreserved assertion of his (i.e. Bohr's) point of view as an insufficient recognition of others' (in this case, my) contributions." (translation by H.H.)

<sup>11</sup>I deduce that Bell never spoke with Bohr from his 1988 *Omni* interview, where he mentions that he was once in an elevator with Bohr, but did not work up the nerve to speak to him. For details about Bell's background, see (Whitaker 1998, 2016).

## 2 Bell in context

Let me now confess my own scholarly sins. First, as Butterfield correctly points out, I failed to contextualize “Subject and Object” in Bell’s larger corpus, and in particular, I failed to point out how it fits with the theme of “there is a quantum measurement problem.” The lack of contextualization was a result of the brevity of my piece. However, I maintain that a similar critique can be maintained and even strengthened against the backdrop of Bell’s entire corpus. In particular, Bell gets top marks for persuasive rhetoric, but low marks for historical accuracy (perhaps on purpose). What’s more, while Bell was good at drawing out the absurd implications of the views of some practicing physicists, he did not inquire into the presuppositions of his own views. In fact, I believe that if Bell were pushed to clarify his own philosophical commitments, then he would eventually have to admit that he demands a physical theory to describe things “as they are in themselves” (to use a phrase from Bernard Williams). This kind of commitment sounds really good, until one starts asking hard questions about the presupposed metaphysical and semantic picture. For example, does a theory describe, or is it a human subject who describes? And if a human subject is doing the describing, then how could he or she describe without employing arbitrary elements, such as specific linguistic conventions?<sup>12</sup> Such questions make it seem a little less obvious that a physical theory can and should describe reality as it is in itself.

Second, I admit that I should have explicitly noted that “Subject and Object” was addressed to a particular audience at a particular time — and that it was a time when physics was in a bad state, philosophically speaking. Bell mentions elsewhere that his university courses in quantum physics were philosophically frustrating (see Whitaker 1998); and surely he was acquainted with many physicists who did not care about foundational rigor. So, I grant that Bell did have a legitimate complaint.

Unfortunately, it did not occur to Bell that originally fruitful ideas might have become corrupted by less philosophically reflective physicists. Indeed, while Bell correctly identified a problem in how people were approaching physics, he incorrectly diagnosed the nature and the sources of the problem. The problem with physics was not Bohr’s influence, and especially not Bohr’s attempts to reflect on physics at a deep philosophical level. Rather, the problem was that physics was increasingly being pressed into the service of technological and military dominance, with the result that physics education came to focus more on technique and calculation than on conceptual understanding. The injunction to “shut up and calculate” did not emerge from pre-war Copenhagen, but from the rising technocratic superpower, the United States.<sup>13</sup>

It might seem like I am trying to direct attention away from the real problem, which is simply the measurement problem. In one sense, yes, I am intentionally trying to take in the bigger picture, and to loosen obsessive focus on analytically tight presentations of the measurement problem. On the face of it, the measurement problem says that a few

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<sup>12</sup>In footnote 7, Butterfield mentions Shimony’s project of “closing the circle”, i.e. deriving the manifest image from the scientific image. If the “scientific image” is things as they are in themselves, and the “manifest image” is things as they appear to us, then closing the circle would amount to achieving Bernard Williams’ “absolute conception of reality”. This noble aspiration was critiqued by, among others, Putnam (1992).

<sup>13</sup>To properly support these general claims, it would be useful to look at funding trends, or changes in physics curricula and textbooks.

propositions, say  $\phi_1, \phi_2, \phi_3$ , are inconsistent, and so one must reject at least one of these propositions. But of course,  $\phi_1, \phi_2, \phi_3$  are not formally inconsistent; they are inconsistent in the light of innumerable tacit background assumptions, e.g. about how we use mathematical objects to represent physical states of affairs. What this means is that one may legitimately refuse to play the game of saying whether one rejects  $\phi_1, \phi_2$  or  $\phi_3$ . For example, the typical way of trying to reduce the Everett interpretation to absurdity is by saying that it rejects the claim that measurements have outcomes. But Everettians rightly tell a more sophisticated story about their solution to the measurement problem (see Brown and Wallace 2005).

Butterfield suggests that criticizing Bell's approach threatens to bury the measurement problem. But the measurement problem had been on the front-burner of discussion already several decades before Bell wrote. In fact, in his 1946 article "On the measurement problem of atomic physics" (Om maalingsproblemet i atomfysikken), Bohr claims that the existence of the quantum of action entails that a subject can be entangled with the object of description, and that seems to destroy the idea of the object having definite properties. He then reminds the reader of the solution that he had articulated already in the 1920s, i.e. that the notion of a communicable "measurement result" presupposes a classical context.<sup>14</sup>

### 3 What is standard QM?

I tried to draw attention to the fact that Bell accuses QM of lacking several theoretical virtues, e.g. it is "inexact", "ambiguous", "only approximately self-consistent." However, Butterfield requests that we shift emphasis back to Bell's claim that QM fails to explain macroscopic definiteness. I grant that the latter kind of problem would make the former kind of problem rather insignificant. In particular, if a theory says something obviously false, then what does it matter if that theory has some vagueness? If QM is inconsistent with the determinacy of the macroworld, then why does Bell spend so much time insinuating that QM lacks theoretical virtues such as exactness?

The reason is simple: unitary QM may have the virtues that Bell extols, but he also believes that it makes false predictions. Thus, Bell thinks that physicists make various vicious modifications to QM in order to block its false predictions. In particular, physicists manually intervene into QM to get the right predictions, e.g. by invoking wave-function collapse, and the theory then becomes subjective, ambiguous, inconsistent, etc.

Again, there is a certain way of manually intervening in a theory's predictions that is definitely vicious. But I don't think that is what is going on with QM. [Here I agree with Wallace (2014) that standard QM does not include the eigenstate-eigenvalue link, nor the projection postulate.] The idea that QM makes predictions about which quantities are definite is, I think, a mistake. Instead, the "intervention" needed to extract predictions from QM is the innocuous intervention of specifying what question one is asking. E.g. is one

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<sup>14</sup>A precise notion of "classical context" emerges from Howard's (1979) explication of Bohr, and more recently it has been developed by Bub, Clifton, Dickson, Halvorson, and Landsman, among others. While Bohr does not use the exact phrase "classical context", and does not usually explicate such notions mathematically, he does say that classical concepts are needed to specify the "conditions of description" (betingelser for beskrivelse), and thereby establish a boundary between subject and object. For a clear and modern discussion of these issues, see (Landsman 2006).

asking about an object's position or about its momentum? In short, to get a description out of QM, one needs to specify a classical context.

Recall here that the pilot-wave theory itself specifies a fixed classical context, viz. the context where configuration variables are assumed to have definite values. Bohr thought that was an appropriate context to interpret the data in terms of "spacetime coordination." However, Bohr also thought that the data could be interpreted in terms of "causality", by establishing a context in which the law of momentum conservation holds.

## 4 Epistemology for entangled subjects

Butterfield disagrees with my claim that Bell assumes that QM must be read as a theory of two kinds of things: (classical) subjects and (quantum) objects. In particular, in Section 2 he says: "Bell does not assume this. What he *does* do — here and in other papers — is to contrast [the happy situation in classical physics with the unhappy situation in quantum physics]." I agree that Bell is trying to draw such a contrast, although I would describe it quite differently. Let me use an analogy. After Darwin, one might have contrasted the happy picture of humans as special creations of God with the unhappy situation of humans as the result of random evolutionary processes. Following Bohr, I think that the same kind of contrast is applicable to the shift from classical and quantum mechanics. In classical physics it was possible for humans to think of themselves as capable of achieving a god's eye view of reality. But quantum physics says: "Sorry, you are entangled with the things you are trying to describe. Therefore, it is impossible for you to describe things 'from the outside'."

But now back to my claim that Bell falsely assumes that the subject must appear in the quantum mechanical description. I grant that this point was not clearly stated. Let's look again, then, at what Bell says explicitly: QM is defective because "exactly where or when to make [the subject-object distinction] is not prescribed." What really is Bell's complaint here? I think the complaint, at root, is against the kind of contextualism that one finds in Bohr's way of thinking about QM, and in his epistemology in general.<sup>15</sup> According to Bohr, QM provides a function from quantum states and classical contexts to probability distributions. However, Bell seems to be troubled by the idea that the correct description could depend on the way that the subject-object distinction is drawn, and that the theory itself does not specify where to draw it. I would suggest that Bell presupposes that a fundamental physical theory should describe things in a contextless and subjectless fashion — i.e. it should give us a "god's eye view" or "absolute conception" or "view from nowhere". Of course, the desire for this kind of description of reality has an esteemed pedigree in the history of Western thought. Nonetheless, according to Bohr, the epistemological lesson of quantum theory is that this kind of description is not possible.

Bohr then suggests that an entangled subject can choose to ignore one part of reality (i.e. some quantities) in order to engage in the idealization that other quantities have exact values. This act, of choosing a subset of quantities, is the specification of a classical context;

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<sup>15</sup>A similar kind of contextualist view can be found in the work of Greta Hermann (see Crull 2017). This kind of contextualism has roots that go at least as far back as Hermann von Helmholtz, who stresses that knowledge of our physical constitution and situation should be used to interpret the data we receive from our senses (see Patton 2019).

and, thus, every quantitatively exact description in quantum physics is explicitly relativized to a context.

In this case, the situation in quantum physics is not “happy” like the situation in classical physics, if by “happy” one means that the context of description is essentially negligible. Classical physics would permit that descriptions can be essentially subject-free, or at least, that there could be lossless translation from one subject’s description to another’s. However, the reason for giving up on this kind of epistemic faith is because we now know that observers are entangled with the things they are trying to describe. That is the epistemological lesson of QM: humans cannot achieve the sort of perspective-free knowledge that Spinoza and Einstein hoped they could.<sup>16</sup> Of course, Bell would have said that this kind of claim — i.e. that QM has a significant epistemological lesson — is “romantic” or even “unprofessional”. Some of Bell’s admirers, e.g. Everettians, go for radical ontological revision, while others, e.g. Bohmians, maintain 17th century meta-theoretical standards for clear and distinct ontological description. In the latter case especially, it seems to be assumed that the meta-theory is immune from falsification, or that upholding old meta-theoretical presuppositions is a requirement of intellectual integrity! I would say, however, that physics has been especially successful, and philosophically interesting, in cases where it has led to revisions of meta-theory. Let us not forget that modern epistemology arose, with Descartes, in tandem with modern physics.

## 5 On reading charitably

In Section 1, Butterfield says that “Bell does not shirk his duty to try to understand what the pioneers of quantum mechanics said about the measurement problem.” However, the only evidence Butterfield brings forward for this claim is that Bell mentions the pioneers of quantum mechanics in the third paragraph of “Subject and Object.” *Pace* Butterfield, this paragraph provides little evidence that Bell tried to understand what the pioneers of quantum mechanics said and wrote. To the contrary, it shows Bell insinuating that they were uninterested in foundational questions. He says that “they did not wait for agreed answers before developing the theory”, and that “the vagueness of the postulate in no way interferes . . .” Being wise to Bell’s rhetorical tricks, I read these sentences as saying “the pioneers had a philosophical problem on their hands, but they ignored it and moved forward with deriving more empirical predictions” and “they tolerated vagueness in the postulates.” These claims are misleading at best, and they serve as the basis for many false claims in the literature today. Far from ignoring the foundational problems, Bohr and his colleagues discussed them, if not exhaustively, certainly exhaustingly. So, at the very least, I maintain that Bell is guilty of false insinuation.

More generally, Bell’s writings, while an exemplar of clarity and rigor, rarely provide careful or charitable reconstruction of the thought of those he criticizes or uses as a foil. To take one example, in the Appendix to “Bertlmann’s socks and the nature of reality”, Bell takes up Bohr’s reply to the EPR argument. After one paragraph of exposition, Bell says

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<sup>16</sup>Landsman (2006) establishes a useful analogy where Einstein stands to Spinoza as Bohr stands to Maimonides. In Bohr’s notes and correspondence it can be seen that he explicitly rejects the Spinozistic picture (see Favrholt 2009, Chap 5).

of Bohr’s claims: “I have very little idea what this means.” Interesting to know, but not exactly an exemplar of careful scholarly engagement, nor much help in moving a constructive dialogue forward. Imagine if you asked an undergraduate student to analyze Quine’s “Two dogmas of empiricism”, and he reported “I have very little idea what this means.” In that case, you might rightly assume that the student either was not in a position to appreciate the argument, or failed to put in the requisite effort.

If Bell did put a solid effort into trying to understand Bohr, Heisenberg, or other early interpreters of quantum theory, then he unfortunately did not document it. Whitaker’s (2016) detailed intellectual biography does not suggest that Bell consulted Bohr’s notes and correspondence, nor that he interviewed anybody from the inner circle in Copenhagen. What’s more, Bell had no training in metaphysics or epistemology, no experience reading and analyzing philosophical texts, and no knowledge of the languages (German and Danish) in which the discussions took place.<sup>17</sup> The narrow, technical focus of Bell’s education might explain why he failed to read Bohr’s text with a will to understand what was being said.

Similarly, in his *Omni* interview of 1988, Bell says: “There is this philosophy [i.e. Bohr’s], which was designed to reconcile people to the muddle; You shouldn’t strive for clarity — that’s naive. ‘Muddle is sophisticated’.” How could Bell, in so many ways an exemplar of clarity and rigor, allow himself to make such imprecise and groundless claims? First of all, Bell attributes intentions to Bohr without citing any evidence. Even if some physicists did in fact use Bohr’s intellectual authority as an excuse for their own intellectual laziness, why insinuate that Bohr intended that consequence? Does Bell know something about Bohr of which neither his contemporaries, nor his scholarly biographers, were aware? Second, Bell insinuates that Bohr explicitly discouraged the search for clarity, while the actual hard evidence supports the opposite conclusion. For example, the word “unambiguous” (entydig) appears hundreds of times in Bohr’s writings, always in the context of demanding that physicists give “unambiguous, i.e. clear, descriptions.”

To be fair, Bell said these things in an interview for a popular science magazine, not in a scholarly monograph. So, I am not accusing him of a moral shortcoming or of violating disciplinary standards. However, Bell’s attributions are ill-supported, and his intellectual stature gives us no reason to repeat them, or even to take them seriously.

## 6 The discretion of the theorist

In my original comment, I noted that even the pilot-wave theory leaves things to the discretion of a theorist. However, this point, says Butterfield (at the end of his Section 2): “is neither here nor there” because “what matters is to have a solution to the measurement problem.” I maintain that my point is relevant because among Bell’s charges against QM is that it leaves some things to the discretion of a theorist. For example,

... the theory should be fully formulated in mathematical terms, with *nothing* left to the discretion of the theoretical physicist ... until workable approximations are needed in practice. (Bell 1990, p 33; emphasis mine)

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<sup>17</sup>Bernstein (2011) points out that Bell could not read von Neumann’s text until it was translated to English in 1955.

Granted, Bell is thinking here of it being left to the discretion of a theorist when a collapse of the wave-function occurs; and I agree that this kind of discretion would be problematic, if it leads to inconsistent attributions of properties. Bell’s criticism is directed, then, to the deformed “theory” that he had been taught as an undergraduate.

But let’s think harder about whether Bell’s demand makes sense. Does it make sense to ask a theory to leave nothing to the discretion of the theoretical physicist? Would it be possible to replace QM with a theory like that?

In many other places, Bell suggests that the pilot-wave theory meets his stringent demands, and so also the demand that nothing be left to the discretion of the theoretical physicist. Butterfield clarifies by saying that the pilot-wave theory only leaves an “innocuous role” for an external subject. But how are we to distinguish the innocuous from the non-innocuous roles? I do not ask this question rhetorically, but in all seriousness. It would behoove philosophers of physics to raise the level of clarity about what we take to be virtues and defects of theories. In particular, all theories leave some things to the discretion of a theorist. Which such things are innocuous, and which are problematic?

My own view here is that Bohr himself was arguing that classical context belongs on the side of innocuous discretion — even if that destroys the illusion of a god’s eye view description. Indeed, much of Bohr’s work was devoted to exploring whether contextualism is consistent with objective description, even though it initially seems to raise the specter of subjectivity. In any case, I mention the pilot-wave theory to stress that every empirical theory, even the most exact, still leaves some things to the discretion of the theorist; and so Bell is unfair to criticize QM for having this feature.

## 7 Conclusion

*De gustibus non est disputandum.* The debate we are engaged in here is ultimately too vague to admit of any sharp resolution. It is true that we need more careful historical and conceptual research on Bohr, Bell, and on the history of quantum physics in general. Let’s not bury any of it, but let a hundred flowers bloom! However, even after all the historical research is in, there will still be disagreements of taste and style. While there are many aspects of Bell’s style that I find admirable, his philosophical heavy-handedness is not one of them.

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## Appendix: What Bell says

Here, for convenience and completeness, is the beginning of Bell’s paper, as reprinted in his (1987/2004). This is the passage on which Part I concentrates. The paper was for a symposium in honour of Dirac. But note that the CERN preprint from September 1972 (available at: <http://cds.cern.ch/record/610096/files/CM-P00058496.pdf?version=1>) has an opening sentence deleted from the reprint, namely: ‘I have been invited to contribute under this heading.’ So the form of the invitation to Bell might explain, at least in part, why he here cast the measurement problem in the language of ‘subject’ and ‘object’. It is possible that the symposium organizers were influenced in their choice by Bohr’s frequent use of the subject/object terminology, which traces back to more general epistemological discussions in the 19th century.

The subject-object distinction is indeed at the very root of the unease that many people still feel in connection with quantum mechanics. *Some* such distinction is dictated by the postulates of the theory, but exactly *where* or *when* to make it is not prescribed. Thus in the classic treatise of Dirac we learn the fundamental propositions:

... any result of a measurement of a real dynamical variable is one of its eigenvalues,

... if the measurement of the observable  $\xi$  for the system in the state corresponding to  $|x\rangle$  is made a large number of times, the average of all the results obtained will be  $\langle x|\xi|x\rangle$  ... ,

... a measurement always causes the system to jump into an eigenstate of the dynamical variable that is being measured ... .

So the theory is fundamentally about the results of ‘measurements’, and therefore presupposes in addition to the ‘system’ (or object) a ‘measurer’ (or subject). Now must this subject include a person? Or was there already some such subject-object distinction before the appearance of life in the universe? Were some of the natural processes then occurring, or occurring now in distant places, to be identified as ‘measurements’ and subjected to jumps rather than to the Schrödinger

equation? is ‘measurement’ something that occurs all at once? Are the jumps instantaneous? And so on.

The pioneers of quantum mechanics were not unaware of these questions, but quite rightly did not wait for agreed answers before developing the theory. They were entirely justified by results. The vagueness of the postulates in no way interferes with the miraculous accuracy of the calculations. Whenever necessary a little more of the world can be incorporated into the object. In extremis the subject-object division can be put somewhere at the ‘macroscopic’ level, where the practical adequacy of classical notions makes the precise location quantitatively unimportant. But although quantum mechanics can account for these classical features of the macroscopic world as very (very) good approximations, it cannot do more than that. [footnote omitted] The snake cannot completely swallow itself by the tail. This awkward fact remains: the theory is only *approximately* unambiguous, only *approximately* self-consistent.

It would be foolish to expect that the next basic development in theoretical physics will yield an accurate and final theory. But it is interesting to speculate on the possibility that a future theory will not be *intrinsically* ambiguous and approximate. Such a theory could not be fundamentally about ‘measurements’, for that would again imply incompleteness of the system and unanalyzed interventions from outside. Rather it should again become possible to say of a system not that such and such may be *observed* to be so but that such and such *be* so. The theory would not be about ‘observables’ but about ‘beables’. These beables need not of course resemble those of, say, classical electron theory; but at least they should, on the macroscopic level, yield an image of the everyday classical world ...