# Halvorson on Bell on 'Subject and Object'

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Abstract: Hans Halvorson has recently criticised Bell's (1973) paper 'Subject and Object'. I maintain that his criticism is unfair.

## 1 Introduction

In a recent three-page paper (2020), Halvorson criticises Bell's (1973) paper 'Subject and Object'.[1] Halvorson announces his criticism in trenchant and uncompromising terms. His opening paragraph reads as follows:

> 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" (1987). 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.

I will reply that both this, and the criticism that Halvorson goes on to develop, are unfair to Bell.

As to the charges above, 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 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

[1] 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.

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.[2]

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 2)—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 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).

In the rest of this paper, I will: report Halvorson's criticism (Section 2); reply to it, by expanding on the contrast between (a) and (b) (Section 3); and conclude more positively (Section 4). For convenience and completeness, I give in the Appendix the complete passage from Bell (1973) that Halvorson is criticising.

## 2 Halvorson's criticism

Halvorson's paper is short, and so I can present his criticism of Bell almost in its entirety. Here is an extended quotation. It starts from just after Halvorson's first paragraph. It omits two

[2] 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).

paragraphs in the middle of his paper (which I believe only fill out details I need not address), but otherwise it is complete. That is: it continues to the very end of Halvorson's paper.

> 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.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 back-drop of his false assumption that the subject must appear *in* the quantum-mechanical description.
>
> ... [here I omit two paragraphs]
>
> Due to misunderstanding the role of the subject in quantum mechanics, Bell also falsely accuses quantum mechanics of being "intrinsically ambiguous and approx-

imate" (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. [I omit a footnote giving an example.] 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.

## 3 Reply to the criticism

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.[3]

Given all this; what about 'subject' and 'object', i.e. Halvorson's (2) and (3)? As I announced in Section 1, and we saw in Section 2's quotation, Halvorson's main claim against Bell is that he 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 we say that 'consciousness collapses the wave-function'.[4]

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

[3] 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.).

[4] 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.

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 reply. But there are three ancillary points that are worth making ...

### 3.1 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'.[5]

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,

[5] As it happens, I have no have qualms about the sort of classical cosmic inventory envisaged by the last 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.

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.[6]

(3): In my reply to Halvorson—my Bellian attempt to prevent the measurement 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 (at the end of Section 2 above) 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.[7]

[6] 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.

[7] 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

## 4 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.[8] But I mention them (along with, of

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).

[8]For example, Butterfield and Marsh (2018, 2019) discuss Kent's proposals.

course, some of the other work cited above, e.g. in footnote 7) 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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## 6 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 Halvorson concentrates. As I said in footnote 1, 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 I surmise that 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'.

> 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 '*observ*ables' but about '*be*ables'. 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 ...