Another Philosopher Looks at Quantum Mechanics
or
What Quantum Theory is Not

Nancy Cartwright[1]
Department of Philosophy
London School of Economics and University of California San Diego

1) **Introduction**

In 1965 Hilary Putnam published the standard work on the interpretation of quantum mechanics, a piece that all philosophers of quantum mechanics of the time had to come to terms with. He laid out one-by-one in a clear non-technical way each of the available approaches to the problem and explained, again non-technically but very exactly, what is wrong with them. In his own final words Putnam took 'the modest but essential step of becoming clear on the nature and magnitude of the difficulties' (1965, 158).

In the almost forty years since there have been a variety of advances, technical developments and new points of view. But none of these is uncontroversial. In fact each suffers from some one or another of the very difficulties that Putnam summarised. Many of the newer approaches explain why the problem is not a problem after all; a few bury the problems in technical detail; and some make heroic assumptions, often metaphysical, that cause the problems to disappear. My conclusion surveying the contemporary literature is the same as Putnam's in 1965: 'no satisfactory interpretation of quantum mechanics exists today' (1965, 157).

The time scale is amazing. Putnam's important piece was written forty years after the first formulations and successes of quantum mechanics. I am writing forty years later and about a theory that has transformed our technology and our way of thinking about the world. Why does this theory still have 'no satisfactory interpretation'? Because, I shall argue, it does not need one; and that, I shall argue, is exactly the view that

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Putnam should have taken in 1965. For it is from Putnam himself writing just three years earlier in another context – in his 'What Theories Are Not' - that we learn: *successful theories do not need interpretation.*

Why then when it comes to quantum mechanics did Putnam join other philosophers in their hunt for an interpretation? I think that these philosophers were attempting to substitute an interpretive principle – in particular a single interpretive principle – for a very great deal of detailed physics that needs doing; and the attempt was unnecessary, since the physics was being done then and continues to be done now, very successfully. Theory needs to be attached to the world; and it gets this attachment through experiment, technology, explanation and prediction. It also needs to be rich and detailed with multiple connections among its own concepts and also with concepts from other branches of knowledge. All of this, Putnam argued, is what gives theoretical terms their meanings. Meaning depends then on doing more good physics, not on a principle of interpretation. The hunt for interpretation principles was fuelled, I suspect, by the idea that the proper axiomatic formulation of a theory should contain all the principles necessary to fix all the attachments that the theory makes to the world. Putnam taught us that this demand is both ill-conceived and impossible to fulfil.

The problem that probably drove Putnam to treat quantum mechanics differently from all other sciences was one long recognised. Much of the successful physics I referred to links quantum mechanics to the world via other more established branches of physics, such as classical mechanics, classical electromagnetic theory, fluid dynamics and classical statistical mechanics. This remains true even as we develop theories like quantum electrodynamics that cover aspects of the same domains that the classical theories treat. This may have seemed unacceptable to Putnam because he expected quantum physics to *replace* classical physics; and he expected replacement, I suppose, because he could see that quantum mechanics offered good treatments in a great many cases in which classical physics failed. Just think about the very first successes of the quantum ideas. Classical electromagnetic theory predicted that orbiting electrons would cycle down into the nucleus and thus the atom would be unstable. Bohr's quantum treatment forbade this. Putnam could have no doubt that Bohr's theory is *better*.

I too acknowledge that the quantum theory tells us far more accurately how an atom is held together than does classical electromagnetic theory. It tells us a great many other things as well, including – to mention an example I return to below – the change in time of the quantum state of an atomic dipole oscillator acted on by a classic electromagnetic field. But there are also myriads of things it is silent about, where the knowledge provided by classical theories continues to be highly reliable, such as the effects of the macroscopic polarisation induced in the field by the dipole oscillator.

Two things stand in the way of allowing our body of knowledge to include both quantum and classical theories. Once is the presupposition that there must be one single theory that covers everything in the domain of physics. I shall not comment here on this presupposition, which I have attacked at length in *The Dappled World* (Cartwright 1999). We know that Putnam has been inclined towards it, but I think it is a presupposition best avoided. The empirical evidence available at this time is too scanty to push us very far towards either unity or disunity; and a commitment to unity stands in the way of taking seriously our current heterogeneous body of knowledge, with all the successes it has provided us. Second is the assumption that quantum mechanics itself must tell us with one single interpretive formula how quantum concepts relate to classical concepts, or to the concepts with which we test and apply the quantum theory. Putnam's own teachings about theory, observation and meaning show that this is not so.

I should make explicit at the start one clear implication of my claims here. Contrary to standard accounts, the famous 'Born interpretation' of quantum mechanics can not serve as an *interpretation* of the quantum wave function, for all the same reasons that Putman gives against 'interpretations' of theoretical concepts in general. But this is not problematic, for two reasons. The first is Putman's own: if a theory is working correctly, its terms do not need special principles to interpret them.

The other reason is more peculiar to the case in hand. If we look at how quantum theory is tested and how it is applied, we find that the Born rule does not play the ubiquitous and irreplacable role attributed to it. The Born rule tells us, for every quantity Q represented by a quantum operator, what the probability is in a given quantum state for various values of Q to result if Q is measured.

This way of linking quantum theory to other concepts is sometimes of use, but often it is not. As we look through examples where quantum theory gets out of itself and connects with other matters, we see a great variety of different kinds of connections. There are myriad ways to link quantum and classical concepts as well as to connect quantum concepts directly with concepts that describe the materials out of which we build our instruments. Nor do these connections derive from any one central principle like the Born rule. They are part of the network of knowledge that we build up as we expand and fill in the details of quantum mechanics.

2) **The failure of quantum interpretations**

Putnam's concern in asking for an interpretation of quantum mechanics was specifically with the quantum state function, or 'Ψ-function':

> […] the state of a physical system can be represented by a set of waves…what is the significance of the 'waves'? Answers to this question are usually known as 'interpretations' of quantum mechanics […] (1965, 133).

He catalogued four standard answers: 1) the De Broglie interpretation: 'physical systems are sets of waves' (*ibid.*); 2) the original Born interpretation: quantum systems '*are* particles in the classical sense – point masses having at each instant both a definite position and a definite velocity […] The wave corresponding to a system of particles does *not* represent the state of the system (simultaneous position and velocity of each particle), but rather our *knowledge* of the state, which is always incomplete'. (1965, 135); 3) Hidden variable theories (like the Bohm theory) that assign 'a definite position and momentum to each system but avoid the difficulties of the original Born interpretation by adding "strange laws"' (1965, 139) to account for the strange phenomena connected with quantum interference. 4) The Copenhagen interpretation: '"observables" such as position, *exist* only when a suitable measurement is actually being made.' (1965, 140) The Ψ-function describes the probability distribution of the results that occur when an observable is measured.

As I said in the Introduction, Putnam then lays out what came to be the canonical catalogue of difficulties that undermine these interpretations. In the course of examining each in turn Putnam came to impose 'three *conditions of adequacy* upon proposed interpretations of quantum mechanics':

A. The principle ND [that a measurement does not disturb the observable measured] should not be assumed even for position measurement.

B. The symmetry of quantum mechanics, represented by the fact that one 'interpretation' [i.e. a representation in terms of one particular observable, such as position, rather than some other observable, such as momentum] has no more physical significance than any other, should not be broken. In particular, we should not treat the waves employed in one representation (the position representation in the case of the [Bohm-type] hidden variable theorists) as descriptions of physically real waves in ordinary space.

C. The phenomena of superposition of states […] must be explained in a unitary way. (1965, 146-147)

I present these three conditions explicitly because they can still bear on newer accounts developed since Putnam first wrote. It is, for instance, open to question whether Arthur Fine's prison models account (1982) in which some kinds of particles systematically get undercounted at measurement involves an unacceptable violation of condition A or not. Or consider condition B. Does it rule out theories like the Ghirardi-Rimini-Weber account in which the Ψ-function makes spontaneous reductions into states highly localised in *position*? If we demand, as Putnam does, that we do not privilege representations in terms of one observable over others it seems we may have to settle for a much more open-ended account of spontaneous reduction of the kind outlined in *How the Laws of Physics Lie* (Cartwright 1983).

Putnam showed then that at the time his paper was published in 1965, there were no unproblematic interpretations of the Ψ-function available. Many believe that his conclusion is still true today. But why is that a problem? For only three years before, in his famous paper 'What Theories Are Not', Putnam himself had shown that theoretical terms do not need interpretation.

At the time of writing this paper Putnam was well known for arguing that the meaning of a scientific concept is often given by all the laws that are taken to be true of it (cf. the section on law cluster concepts in his 1962b). Quantum mechanics is a rich theory with much to say involving the quantum state, both about its relations to other theoretical quantities inside and outside of quantum theory and about how this state relates to the world in a vast variety of concrete situations. The theory itself, the entire theory with all it diverse uses and implications, gives meaning to the quantum state. A concept from a theory like this comes already interpreted. This doctrine about meaning begins in 'What Theories Are Not', where Putnam attacks the idea that scientific concepts should ever be in need of interpretation.

3) **Putnam against interpretation**

I begin with a short pre-history to Putman's 'What Theories Are Not'. Logical Positivists were keen to ensure that genuine science should talk sense, unlike the Freudians, some Marxists and most Hegelians. The surest way to guarantee that theoretical claims have a clear and settled signification, it seems, is to demand that the theory itself provide for each theoretical term an operational procedure for ascertaining whether it applies or not. But this requirement for one-to-one operationalization proved too demanding.

In the first place operational procedures will not do the job. Many different procedures are equally appropriate for measuring the same theoretical quantity and different procedures are required in different circumstances. Moreover, the operational procedures associated with a theoretical term do not seem to contribute much to what that term signifies. In the second place, it seems that for a good many perfectly acceptable theoretical terms direct measurements are not available. The canonical example here was the *velocity* of an individual gas molecule in the kinetic theory of gases. We do not know how to measure that. But we can measure various quantities that depend on it, such as the mean kinetic energy, $<\frac{1}{2}mv^2>$, which kinetic theory teaches is equal to the temperature of the gas.

In the face of these difficulties, two concessions were made. First, the rules of interpretation need not be (indeed perhaps should not be) measurement procedures. New terms, rather, should be mapped onto ones 'antecedently understood'. Second, it is sufficient to map only some theoretical terms onto features that are observable or can be directly measured. This resulted in the view of theory that Putnam attacks in 'What theories are not': 'the view that theories are to be thought of as "partially interpreted calculi" in which only "the observation terms" are "directly interpreted" (the theoretical terms being only "partially interpreted", or, some people even say, "partially understood")'. (1962, 215)

Putnam had three objections to this view of theory.

1) 'The *problem* for which this dichotomy was invented ("how is it possible to interpret theoretical terms?") does not exist.' (1962, 216)
2) 'A basic reason […] for introducing the dichotomy is false: namely, justification in science does not proceed "down" in the direction of observation terms'. (1962, 216) In fact justification in science proceeds in any direction that may be handy – more observational assertions sometimes being justified with the aid of more theoretical ones and vice versa!
3) 'The distinction between theoretical and observational terms is "completely broken backed".' (1962, 216)

Claims (2) and (3) are by now well assimilated into the philosophy of science literature, in good part due to the arguments of Putnam in "What Theory Is Not" and elsewhere. Claim (2) denies the so-called "foundationalist" account of scientific knowledge, which supposes that there is some observational basis about which we can form relatively secure knowledge claims and that all of science is justified by its ties to this base. The fall from dominance of the foundationalist pictures and the catalogue of its vices and virtues is so well-known that I do not need to rehearse it here; moreover, it is not immediately relevant to my main points here about quantum mechanics and the need for interpretation.

Claim (3) is part of the attack, new at the time, on the so-called theory-observation distinction; that is, on the claim that scientific concepts can be divided into observational concepts, about which (following the foundationalist account) it was usually supposed we could have secure knowledge, and the theoretical, whose meanings and whose claims to knowledge must both derive from the observational base. Putnam argued – and many took up the battle in support – that such a distinction could not be drawn. The attack on the theory-observation, and the more recent defences of it are well-known.

There is however one feature of Putman's discussion that I should like to underline. Often the attacks on the theory-observation distinction are summarized in the slogan 'All observation terms are theory laden'. That is only half of the story. We must also remember that theoretical terms themselves play a direct role in observation in science, especially in those observations that test theory or help put it to use. Putman argues 'That observation statements may contain theoretical terms is easy to establish. For example, it is easy to imagine a situation in which the following sentence might occur: "We also *observed* the creation of two electron-positron pairs".' (1962, 219, ital. original)

This point is closely connected with Putman's assumption that we ought to accept new theories like quantum mechanics because they provide adequate treatments of phenomena that the older theories get wrong. Putman's claim echoes that of Wilfrid Sellars, who stresses that the chief reason for introducing new theories with new concepts is that the concepts of the older theories are not adequate for describing the world on their own. In Sellars's picture, where getting it right matters we should learn to respond to the world directly with our newer more accurate theoretical concepts (Sellars 1963).

My own conclusion from studying a large number of cases where theory is brought to bear on real phenomena is that neither quantum nor classical theories are sufficient on their own for providing accurate descriptions of the phenomena in their domain. Some situations require quantum descriptions, some classical and some a mix of both. But the practices in physics that support this eclectic view go no way to supporting the claim that the application of quantum concepts is always via classical ones. On the contrary, there are many situations that can only be correctly described by quantum concepts: we cannot patch together classical descriptions that will serve instead. Following Sellars, our best strategy is to learn to describe these situations directly in quantum terms. And we can learn to do so.

Putman mentions the observation of the creation of an electron-positron pair. For a strict quantum mechanical example we can consider superconductivity. The superconducting state in a metal is by now a well-known quantum mechanical state and there is no classical surrogate for it. In the experimental group in which I participated at Stanford University it was not unusual for the senior physicists to come into the laboratory with its familiar apparatuses (dewars for supercooling, complicated electrical circuitry, and so forth), note the characteristic intensity-voltage curve on the screen, and remark, 'I see you've finally got a superconducting state.' When the situation is set up correctly, physicists can tell by looking that a system is in this particular quantum state. Of course they do not do so infallibly and not without a very great deal of background knowledge. But that, we have learned from the attacks of Putman and others on the theory-observation distinction, is true of all observation.

Putman's argument for (1) begins with an attack on the whole idea of a partial interpretation. He reviews at some length two things that might have been meant by 'partial interpretation' by Rudolf Carnap, who championed the idea, and argues that they are both inadequate. The third meaning Putman considers is the one I gave at the beginning of this section, which by the time of his article was the standard reading: '… to partially interpret a formal *language* is to *interpret part* of the language.' (1962, 221, ital. original). This he dismisses with a single sentence: 'Finally, the third sense of 'partial interpretation' leads to the view that theoretical terms have *no meaning at all,* that they are mere computing devices, and is thus unacceptable.' (1962, 224, ital.

original). Partial interpretation turns theories into instruments. So it is no view for a realist to hold.

How then do we give meaning to theoretical terms? Putman turns the question on its head: 'Why should one not be able to give the meaning of a theoretical term?' (1962, 225) In a few sentences he undermines a whole philosophical tradition that had created a pseudoproblem.

> Something like this may be said: suppose we make a 'dictionary' of theoretical terms. If we allow theoretical terms to appear both as 'entries' and in the *definitions*, then there will be 'circles' in our dictionary. But there are circles in every dictionary! (1962, 226)

To finish off, Putman considers two other possible versions of the problem: how are theoretical terms learned and how are they first introduced. In both cases the answer is the same as before. Theoretical terms are learned and they are introduced in exactly the same way as any other terms. There is no special problem with theoretical terms:

> We perhaps come closer to the problem if we observe that, dictionaries are useful, they are useful only to speakers who already know a good deal of the language. One cannot learn one's native language to begin with from a dictionary. This suggests that the problem is really to give an account of how the use of theoretical terms is learned (in the life-history of an individual speaker); or perhaps, of how theoretical terms are 'introduced' (in the history of the language).
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> To take the first form of the problem (the language-learning of the individual speaker): it appears that theoretical terms are learned in essentially the way most words are learned. Sometimes we are given lexical definitions (e.g. 'a *tigon* is a cross between a tiger and a lion'); more often, we simply imitate other speakers; many times we combine these (e.g. we are given a lexical definition, from which we obtain a rough idea of thee use, and then we bring our linguistic behaviour more closely into line with that of other speakers via imitation).

> The story in connection with the introduction of a new technical term into the *language* is roughly similar. Usually, the scientist introduces the term via some kind of paraphrase. For example, one might explain 'mass' as 'that physical magnitude which determines how strongly a body resists accelerated, e.g. if a body has twice the mass it will be twice as hard to accelerate'. (Instead of 'physical magnitude' one might say, in ordinary language, 'that property of the body', of 'that *in* the body which …' such 'broad spectrum' notions occur in every natural language; and our present notion of a 'physical magnitude' is already an extreme refinement.) Frequently, as in the case of 'force' and 'mass', the term will be a common language term whose new technical use is in some respects quite continuous with the ordinary use. In such cases, a lexical definition is frequently omitted, and in its place one has merely a statement of some of the differences between the usual use and the technical use being introduced. Usually one gains only a rough idea of the use of a technical term from these explicit metalinguistic statements, and this rough idea is then refined by reading the theory or text in which the term is employed. (1962, 226)

We know that Putman's views about meaning have changed over the years and also that he no longer defends 'realism' without putting any qualifications in but instead insists on 'internal realism'. At some times Putnam has argued that the world itself plays a role in fixing the reference of our terms (cf. his 1975). At other times he has argued exactly the opposite. Our access to the world is always mediated by our experiences of it and our beliefs about it. So if we are ever to know what we are talking about, it must be the world as presented in our beliefs and experiences that fixes the reference of our terms, not the world as it is in itself. This leads naturally to "internal realism" (cf. his 1981). We can reasonably be realists about a good many things, but these must always be things in the world as presented in our beliefs and experiences since these are the only things that we can ever talk about; we cannot succeed in referring to anything else. The view that knowledge accumulates, which I believe Putnam supposed in his work on quantum mechanics, makes equal sense whether one is an internalist or not. One can suppose both that theory provides knowledge and that its terms are meaningful whether one thinks it describes the world "in itself" of thinks that it describes a world internal to our beliefs and experiences.

None of these changes in Putnam's views have any effect on the point here: there is nothing peculiar about meanings of theoretical terms. Their meanings are fixed in the same way as all other terms in the language. They do not need some special kind of interpretation. With respect to the particular project of interpreting them in observation terms, I repeat Putman's own question 'Why should we suppose that this is or ought to be possible?' (1962, 225) My claim here is that there is nothing peculiar about the quantum state function. It is just like any other theoretical term, and Putman's own conclusions apply to it. It does not need an interpretation, much less an interpretation in observational/classical terms; nor is such an interpretation likely to be possible.

4. Return to the quantum state

There may seem to be one advantage that attempts to interpret the quantum state have in the face of Putman's criticism of such enterprises in general. These attempts try to provide a direct observational or classical correlate for the quantum state. They thus avoid Putman's objection to *partial* interpretation: they aim to ensure that the quantum state does not have, as Putman describes, 'no meaning at all', that it does not function as a 'mere computing device', both results which, as we have seen, Putman regards as unacceptable to a realist. But the question remains: Can any of these attempts, should they prove problem free, serve as an *interpretation* at all? These attempts look like the same crude one-to-one operationalism that we long ago discarded. Just as with any other theoretical concept, the meaning of the quantum state cannot be given by one operational procedure, or one thing the quantum state does, or one set of consequences it implies. To understand what the quantum state means, we must know a great deal about how the quantum state behaves and what kinds of consequences it has in a vast variety of different situations.

Of course we must face questions not just of meaning but of verification and of use. Putman himself in defending the need for an interpretation of the quantum state argues, 'Any formalization of quantum mechanics must either leave the question of interpretation open – in which case no testable predictions whatsoever will be derivable within the formalization, and we will have formalized only the *mathematics* of quantum mechanics, and not the physical theory – or we must include in the formalization at least the minimal statistical interpretation, in which case the term "measurement" [and thereby the usual "measurement problem"] automatically enters.' (1965, 147) But I think Putman has made a mistake in this, as we can see by looking at the great variety of ways in which we do in fact test the quantum theory. I do not wish to say that the statistical interpretation never plays a role. It does, for instance, in cases like the ones for which Max Born originally introduced it – for scattering. But a detailed survey of tests and applications shows that it plays little role in other kinds of cases and certainly is not central to testing as Putman here maintains.

The story with testing in quantum mechanics is exactly the same as with any other theory. We verify the thousands of different claims in quantum theory by thousands of different connections in thousands of different circumstances. And as Putman himself argues (and Sellars's arguments support), there is no reason to think that the vocabulary in which we describe the test procedures or their outcomes can or should be stripped of quantum concepts.

In my own view what we see in looking at how quantum mechanics is tested and how we use it in modern technology – SQIDS, transistors or lasers for example – is that there are a great many situations, usually requiring a mix of quantum and classical concepts to pick out, in which quantum theory does have consequences appropriately described in purely classical terms. Perhaps it is this kind of fact that encouraged Putman to look for a classical interpretation of quantum mechanics, despite his own arguments against both the need for and the possibility of interpretation in general. But when we look at these cases, we see that there is no single formula that covers all the various connections we find. Discovering them is the stuff that ongoing physics is made from.

For example at the heart of the first quantum theory of the laser is the claim that a quantum dipole oscillator (i.e. a quantum state that changes shape in a certain way in time) produces a polarization in the electromagnetic field in which it is embedded. Here *polarization* is a purely classical concept that can then be used in all the usual ways to make calculations via classical electromagnetic theory. This is a link central to how we use quantum theory to construct and to understand lasers. But it is not a link built into quantum theory from the start, in one magic formula. It is something we learned as quantum theory expanded to cover new situations. Moreover it is facts like this that we keep learning as we keep doing quantum physics. What is important to the question of issue here is that we *discover* them: we do not deduce them from some single pre-given interpretation of the quantum state. Putman's own views about interpretation, it seems, apply just as much to quantum mechanics as to other theories. With respect to providing an interpretation of the quantum state, 'Why should we suppose that this is or ought to be possible?' (1962, 225)

5) Schroedinger's cat

What then, on this view, of Schroedinger's famous cat, whom Putman describes in his look at quantum mechanics? We may think of Schroedinger's cat in two ways, either as an exemplar of a micro-macro interaction or as a picturesque example of a measurement, in that odd abstract sense of 'measurement' that is special to the statistical 'interpretation' of quantum mechanics. In either case the story of Schroedinger's cat is a fantasy. In the fantasy we couple the life of a cat to the location of a single photon vis-à-vis a half-silvered mirror: reflected, the cat lives; transmitted it dies. But we have never yet succeeded in actualising this fantasy. It is, rather, a story invented to match an abstract piece of formalism and an abstract concept of measurement. It does not match any real physical process – and it is only real physical processes that our theories need to treat.

Viewed as a micro-macro interaction Schroendinger's cat can be treated either with or without the assumption of reduction of the wave packet. Consider first the story as it is supposed to proceed without reduction. By a series of quantum interactions a familiar quantum state in the photon is supposed to lead to a very unfamiliar quantum state in the cat: a superposition of a 'cat-alive' quantum state with a 'cat-dead' quantum state. But these are not real quantum interactions that we study in any branch of quantum physics; they are part of a 'just-so' story that has no physics to back it up. And of course there are no such quantum states as 'cat alive' and 'cat dead'. There are no such states described in any physics at all, let alone in quantum physics. The idea that there should be is sometimes defended by the slogan 'Every observable is represented by a quantum operator'. But this slogan is absurd. There is no such thing as the 'cat-alive/cat-dead' operator.

Even if we restrict the slogan less fancifully to what many seem to believe – that every classical dynamical quantity is represented by a quantum operator – it still won't do. First, it flies in the face of Putman's and Sellars's lesson that the most accurate terms with which we can respond to the world will be infected through and through with our best theories. Second the empirical support for this as a universal principle is strongly insufficient. There are applications in which such associations are made, but these are generally highly context dependent; they do not presuppose a once-and-for-all association between the given operator and the corresponding classical quantity to which it is matched. Moreover, they comprise only a small portion of the links between quantum and classical descriptions in successful application.

In rejecting the claim that the interaction with the photon casts the cat into an objectionable state, I do not mean to suggest that in reality quantum states in microsystems never interact with macro systems. To the contrary, it happens all the time, both between micro quantum states and macro quantum states and between quantum descriptions and classical descriptions. Understanding exactly how it happens is central to the many technologies we build today with the help of quantum theory. Consider my earlier example. Superconductors are macroscopic objects and, so far as our best theories can tell, the superconducting state is an irredeemably quantum state, with no classical surrogates. The same is true of causally important states in lasers and transistors as well. Real quantum physics and real quantum engineering teach us regularly about quantum states in macrosystems. But these states are not problematic. The quantum superconducting state is just the state we want in order to treat the behaviour of a superconductor.

Look next at the case of reduction: the photon and cat interact and the cat ends up genuinely alive or dead. As Putnam described, a conundrum is supposedly created by assigning to the cat either the putative quantum 'cat-alive' state or the putative 'cat-dead' state – we then 'back-read' that in this case the photon could no longer be in a state with significant components on both sides of the mirror, which is in contradiction with the original hypothesis.

I have already argued that the mapping between quantum states and the states of the cat is a mistake. I also said that 'cat-alive' and 'cat-dead' are not physics states at all, not even in classical physics. But by this I did not mean to imply that there are no irreducibly classical states nor that quantum and classical states never interact. To the contrary I have explained that in my view much of the physics of testing and application studies just such interactions. But then from this perspective, the whole idea that we could back-read anything about the interaction itself or about the state the photon from the just-so story of Schroedinger's cat is absurd.

If we look at the physics where our successes suggest we really know about quantum-classical interactions, we see, as I said before, that accounts of different processes work in different ways, and – what is important for the present discussion – *none of these quantum-classical interactions are subject to any special paradoxes*,[2] unlike the caricature of Schroedinger's cat.

The second way to view Schroedinger's cat is as a picturesque exhibition of the problems that beset the attempt to interpret the state function in terms of measurement. The cat is a measuring instrument for whether the photon is transmitted or reflected. We then argue, 'Ah, but the cat must be either alive or dead: the pointer must point to either "reflected" or "transmitted". But the photon itself, quantum theory teaches us, is neither reflected nor transmitted; it is in a superposition of both'. The obvious answer to this is that in this case we've got ourselves a very poorly designed measurement. Nothing about the apparatus we are trying to use can tell us the facts we wish to learn. If we want better information, we need a better measurement design, where effects are produced from which we can infer what the state of the photon really is. The second remark is the central theme of this paper. The apparition of measurement employed in the statistical 'interpretation' connects with real measurement in only a few special kinds of cases [3]. That's okay because the

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[2] I say "special" because of course *all* physics is messy, all theories are inaccurate and no treatments are ever final or entirely problem free.
[3] For more about real measurements, quantum and classical, see Chang (1997).

quantum state, deeply imbedded in a rich texture of real physics treating real problems, does not need the concept of measurement in its interpretation. And that is because —

6) Conclusion

— quantum mechanics is no exception to Putman's general views on theories and interpretation. An abstract calculus may require an interpretation. But a live working theory does not.

Bibliography

Cartwright, Nancy (1999): *The Dappled World. A Study in the Boundaries of Science.* Cambridge1999.

Chang, Hasok (1997): On the Applicability of the Quantum Measurement Formalism. Erkenntnis 46, 143-163.

Fine, Arthur (1982): Some Local Models for Correlation Experiments. Synthese 50, 279-294.

Putnam, Hilary (1962): 'What Theories Are Not.' In: Ernest Nagel, Patrick Suppes, and Alfred Tarski (eds.): *Logic, Methodology, and Science*. Stanford. Repr. in: Hilary Putnam: Mathematics, Matter, and Method. Philosophical Papers, Vol. I. Cambridge 1975, 215-27.

- (1962b): 'The Analytic and the Synthetic'. In: Herbert Feigl and Grover Maxwell: Minnesota Studies in the Philosophy of Science, Vol. III. Repr. in: Hilary Putnam: Mind, Language, and Reality. Philosophical Papers Vol. II, Cambridge 1975, 33-69.

- (1965): 'A Philosopher Looks at Quantum Mechanics.' In: Robert G, Colodny (ed.): Beyond the Edge of Certainty: *Essays in Contemporary Science and Philosophy.* Englewood Cliffs. Repr. in: Hilary Putnam: Mathematics, Matter, and Method. Philosophical Papers, Vol. I. Cambridge 1975, 130-58.

- (1975): 'The Meaning of Meaning.' In: K.Gunderson (ed.): Language, Mind, and Knowledge. Minnesota Studies in the Philosophy of Science, Vol. VII.Repr. in:

Mind, Language, and Reality. Philosophical Papers Vol. II, Cambridge 1975, 215-71.

- (1981) Reason, Truth, and History. Cambridge.

Sellars, Wilfrid (1963): Philosophy and the Scientific Image of Man. In: Science, Perception, and Reality. London: Routledge and Kegan Paul, 1-41.