John Bell and the Foundations of Quantum Mechanics

Summer School on

The Foundations of Quantum Mechanics

Dedicated to John Bell

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hidden variables

nonlocality

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typicality

… conventional formulations of quantum theory, and of quantum field theory in particular, are unprofessionally vague and ambiguous. Professional theoretical physicists ought to be able to do better. Bohm has shown us a way.

Hidden Variables

The realization that von Neumann's proof is of limited relevance has been gaining ground since the 1952 work of Bohm. However, it is far from universal. Moreover, the writer has not found in the literature any adequate analysis of what went wrong. Like all authors of noncommissioned reviews, he thinks that he can restate the position with such clarity and simplicity that all previous discussions will be eclipsed. (*On the problem of hidden variables in quantum mechanics,*, 1966, p.2)

Nonlocality

...in this theory an explicit causal mechanism exists whereby the disposition of one piece of apparatus affects the results obtained with a distant piece.

Bohm of course was well aware of these features of his scheme, and has given them much attention. However, it must be stressed that, to the present writer's knowledge, there is no *proof* that *any* hidden variable account of quantum mechanics*must* have this extraordinary character. It would therefore be interesting, perhaps, to pursue some further "impossibility proofs," replacing the arbitrary axioms objected to above by some condition of locality, or of separability of distant systems.

Quantum Theory Without Observers

The concept of 'measurement' becomes so fuzzy on reflection that it is quite surprising to have it appearing in physical theory at the most fundamental level. ... [D]oes not any analysis of measurement require concepts more fundamental than measurement? And should not the fundamental theory be about these more fundamental concepts? (John Stewart Bell, 1981)

It would seem that the theory is exclusively concerned about "results of measurement", and has nothing to say about anything else. What exactly qualifies some physical systems to play the role of "measurer"? Was the wavefunction of the world waiting to jump for thousands of millions of years until a single-celled living creature appeared? Or did it have to wait a little longer, for some better qualified system... with a Ph.D.? If the theory is to apply to anything but highly idealized laboratory operations, are we not obliged to admit that more or less "measurement-like" processes are going on more or less all the time, more or less everywhere. Do we not have jumping then all the time? (John Stewart Bell, 1990)

The Measurement Problem

Does the wave function of a system provide a complete description of that system?

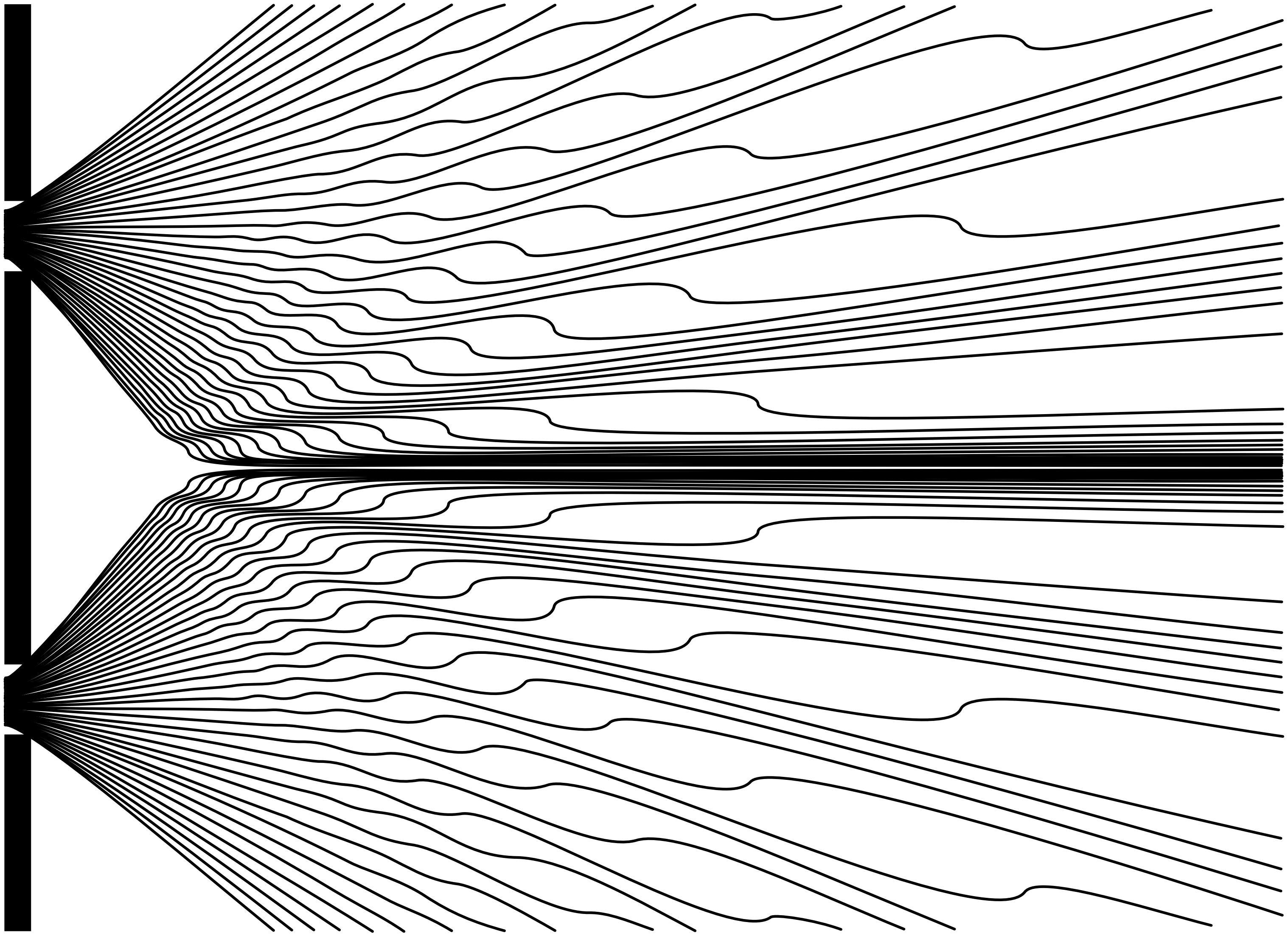
$$\Psi_{alive} + \Psi_{dead}$$

$$\Psi_{left} + \Psi_{right}$$

Local Beables

These are the mathematical counterparts in the theory to real events at definite places and times in the real world (as distinct from the many purely mathematical constructions that occur in the working out of physical theories, as distinct from things which may be real but not localized, and as distinct from the 'observables' of other formulations of quantum mechanics, for which we have no use here). (page 205)

The Two Slit Experiment



Is it not clear from the smallness of the scintillation on the screen that we have to do with a particle? And is it not clear, from the diffraction and interference patterns, that the motion of the particle is directed by a wave? De Broglie showed in detail how the motion of a particle, passing through just one of two holes in screen, could be influenced by waves propagating through both holes. And so influenced that the particle does not go where the waves cancel out, but is attracted to where they cooperate. This idea seems to me so natural and simple, to resolve the wave-particle dilemma in such a clear and ordinary way, that it is a great mystery to me that it was so generally ignored. (1986)

Naive Realism About Operators

the ‘observables’ of other formulations of quantum mechanics, for which we have no use here

A final moral concerns terminology. Why did such serious people take so seriously axioms which now seem so arbitrary? I suspect that they were misled by the pernicious misuse of the word 'measurement' in contemporary theory. This word very strongly suggests the ascertaining of some preexisting property of some thing, any instrument involved playing a purely passive role. Quantum experiments are just not like that, as we learned especially from Bohr. The results have to be regarded as the joint product of 'system' and 'apparatus,' the complete experimental set-up. But the misuse of the word 'measurement' makes it easy to forget this and then to expect that the 'results of measurements' should obey some simple logic in which the apparatus is not mentioned. The resulting difficulties soon show that any such logic is not ordinary logic. It is my impression that the whole vast subject of 'Quantum Logic' has arisen in this way from the misuse of a word. I am convinced that the word ‘measurement’ has now been so abused that the field would be significantly advanced by banning its use altogether, in favour for example of the word ‘experiment.’ (page 166)

[I]n physics the only observations we must consider are position observations, if only the positions of instrument pointers. It is a great merit of the de Broglie-Bohm picture to force us to consider this fact. If you make axioms, rather than definitions and theorems, about the ‘measurement’ of anything else, then you commit redundancy and risk inconsistency.

That X rather than Ψ is historically called a 'hidden' variable is a piece of historical silliness. (page 163)

From the 'microscopic' variables x can be constructed 'macroscopic' variables X

$$X_n = F_n(\mathbf{x}_1, ..., \mathbf{x}_N)$$

—including in particular instrument readings, image density on photographic plates, ink density on computer output, and so on. Of course, there is some ambiguity in defining such quantities—e.g., over precisely what volume should the discrete particle density be averaged to define the smooth macroscopic density? However, it is the merit of the theory that the ambiguity is not in the foundation, but only at the level of identifying objects of particular interest to macroscopic observers, and the ambiguity arises simply from the grossness of these creatures. It is thus from the xs, rather than from ψ, that in this theory we suppose 'observables' to be constructed. It is in terms of the xs that we would define a 'psycho-physical parallelism'— if we were pressed to go so far. Thus it would be appropriate to refer to the xs as 'exposed variables' and ψ as a 'hidden variable'. It is ironic that the traditional terminology is the reverse of this. (page 128)

Mind

A piece of matter then is a galaxy of such events. As a schematic psychophysical parallelism we can suppose that our personal experience is more or less directly of events in particular pieces of matter, our brains, which events are in turn correlated with events in our bodies as a whole, and they in turn with events in the outer world. (page 205)

As regards mind, I am fully convinced that it has a central place in the ultimate nature of reality. But I am very doubtful that contemporary physics has reached so deeply down that that idea will soon be professionally fruitful. For our generation I think we can more profitably seek Bohr's necessary 'classical terms' in ordinary macroscopic objects, rather than in the mind of the observer.

Typicality

Then for instantaneous macroscopic configurations the pilot-wave theory gives the same distribution as the orthodox theory, insofar as the latter is unambiguous. However, this question arises: what is the good of *either* theory, giving distributions over a hypothetical ensemble (of worlds!) when we have only one world. (Bell 1981)

. . . a single configuration of the world will show statistical distributions over its different parts. Suppose, for example, this world contains an actual ensemble of similar experimental set-ups. . . . it follows from the theory that the 'typical' world will approximately realize quantum mechanical distributions over such approximately independent components. The role of the hypothetical ensemble is precisely to permit definition of the word 'typical.' (Bell 1981)

Then there is the surprising contention of Everett and De Witt that the theory 'yields its own interpretation'. The hard core of this seems to be the assertion that the probability interpretation emerges without being assumed. In so far as this is true it is true also in the pilot-wave theory. In that theory our unique world is supposed to evolve in deterministic fashion from some definite initial state. However, to identify which features are details crucially dependent on the initial conditions (like whether the first scattering is up or down in an α track) and which features are more general (like the distribution of scattering angles over the track as a whole) it seems necessary to envisage a comparison class. This class we took to be a hypothetical ensemble of initial configurations with distribution $|\psi|^2$. In the same way Everett has to attach weights to the different branches of his multiple universe, and in the same way does so in proportion to the norms of the relevant parts of the wave function. Everett and De Witt seem to regard this choice as inevitable. I am unable to see why, although of course it is a perfectly reasonable choice with several nice properties. (Bell 1981)

Bell on Bohm

John Stewart Bell is best known for his discovery of the theorem that bears his name. This theorem establishes the impossibility of any explanation of quantum phenomena in terms of what are called local hidden variables. And since one might well imagine that any account in terms of *nonlocal* hidden variables would have to be artificial—cooked up just to do the job—and generally unacceptable, Bell's theorem is widely regarded as precluding any hidden variable account worthy of our consideration. (As far as the meaning of a "hidden variable account" is concerned, for now let me just say, somewhat imprecisely, that a hidden variable formulation of quantum theory would eliminate quantum craziness while retaining the quantum predictions.) In other words, Bell's theorem is widely used to support the proposition that quantum phenomena demand radical epistemological and metaphysical innovations—precisely what hidden variables promise to avoid.

Now Bell wrote much and lectured much about his theorem and its implications. But he wrote and lectured as much, if not more, concerning the virtues of what is the most famous of all hidden variable theories, that of David Bohm. The question thus naturally arises, why would Bell spend so much time and effort expounding upon a theory of just the sort that he himself had shown to be, if not impossible, unworthy of consideration?

Indeed, some physicists have spoken of two Bells, and have suggested that Bell must have been schizophrenic.

I wish to argue that there was, unfortunately for us, but one Bell, and he was the sanest and most rational of men.

There is something else that I would like to do: I would like to convey a small sense of Bell's wonderful style, wit, and clarity. So to the extent possible I shall allow Bell to speak for himself. I shall read excerpts from Bell's articles on the foundations of quantum mechanics which pertain to our question. These articles are all collected in a marvelous book, *Speakable and Unspeakable in Quantum Mechanics.* I would urge all of you, and, indeed, anyone with an interest in physics, to read this book, and then read it again. I shall also have occasion to read from an interview Bell gave several years ago, to the philosopher Renee Weber.

When I was a student I had much difficulty with quantum mechanics. It was comforting to find that even Einstein had had such difficulties for a long time. Indeed they had led him to the heretical conclusion that something was missing in the theory: "I am, in fact, rather firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this (theory) operates with an incomplete description of physical systems." (*On the impossible pilot wave*,page 160, 1982)

Einstein is expressing here the conviction that the supposedly novel quantum randomness will ultimately turn out to be of the same character as the familiar, normal, down-to-earth randomness exhibited, for example, in the behavior of a roulette wheel or a coin flip. The behavior appears random because there are too many relevant details to keep track of. If the quantum description could be completed by the incorporation of such details, the result would be called a hidden variable theory.

However, soon after the advent of quantum theory, any hidden variable account of quantum phenomena was mathematically "proven" to be impossible. Bell continues:

Einstein did not seem to know that this possibility, of peaceful coexistence between quantum statistical predictions and a more complete theoretical description, had been disposed of with great rigour by J. von Neumann. I myself did not know von Neumann's demonstration at first hand, for at that time it was available only in German, which I could not read. However I knew of it from the beautiful book by Born, *Natural Philosophy of Cause and Chance,* which was in fact one of the highlights of my physics education. Discussing how physics might develop Born wrote: "I expect...that we shall have to sacrifice some current ideas and to use still more abstract methods. However these are only opinions. A more concrete contribution to this question has been made by J. v. Neumann in his brilliant book, *Mathematische Grundlagen der Quantenmechanik*....The result is that...no concealed parameters can be introduced with the help of which the indeterministic description could be transformed into a deterministic one. Hence if a future theory should be deterministic, it cannot be a modification of the present one but must be essentially different. How this could be possible without sacrificing a whole treasure of well established results I leave to the determinists to worry about." (1982)

Having read this, I relegated the question to the back of my mind and got on with more practical things.

Bell continues :

But in 1952 I saw the impossible done. It was in papers by David Bohm. Bohm showed explicitly how parameters could indeed be introduced, into nonrelativistic wave mechanics, with the help of which the indeterministic description could be transformed into a deterministic one. More importantly, in my opinion, the subjectivity of the orthodox version, the necessary reference to the "observer," could be eliminated.

Moreover, the essential idea was one that had been advanced already by de Broglie in 1927, in his "pilot wave" picture.(1982)

Let me very briefly try to indicate the sort of thing Bell had in mind when objecting to the subjectivity of orthodox quantum theory, by means of a perhaps extreme example. Concerning the implications of quantum theory, in fact of Bell's theorem itself (about which more later), a very distinguished physicist once wrote that "the moon is demonstrably not there when nobody looks".

More Bell: Bohm's 1952 papers on quantum mechanics were for me a revelation. The elimination of indeterminism was very striking. But more important, it seemed to me, was the elimination of any need for a vague division of the world into "system" on the one hand, and "apparatus" or "observer" on the other. I have always felt since that people who have not grasped the ideas of those papers...and unfortunately they remain the majority...are handicapped in any discussion of the meaning of quantum mechanics. (*Beables for quantum field theory*,1984, page 173)

Interview: In my opinion the picture which Bohm proposed then completely disposes of all the arguments that you will find among the great founding fathers of the subject—that in some way, quantum mechanics was a new departure of human thought which necessitated the introduction of the observer, which necessitated speculation about the role of consciousness and so on.

All those are simply refuted by Bohm's 1952 theory. In that theory you find a scheme of equations which completely reproduces all the experimental predictions of quantum mechanics and it simply does not need an observer....So I think that it is somewhat scandalous that this theory is so largely ignored in textbooks and is simply ignored by most physicists. They don't know about it.

What does Bohm add to the standard quantum description? In a word, the particles themselves: For Bohm the so-called hidden variables are simply the positions of the particles of the quantum system, say the electrons of an atom. These particles move in a manner which is naturally choreographed by the wave function of the system. From the perspective of Bohm's theory, orthodox quantum mechanics leaves out the guts of the description, the very particles which combine to form everything we see around us.

Thus as applied to Bohm's theory, the terminology "hidden variables" seems rather inappropriate, suggesting as it does something exotic, artificial, and ad hoc.

Absurdly, such theories are known as “hidden variable” theories. Absurdly, for there it is not in the wavefunction that one finds an image of the visible world, and the results of experiments, but in the complementary “hidden” (!) variables. Of course the extra variables are not confined to the visible “macroscopic” scale. For no sharp definition of such a scale could be made. The “microscopic” aspect of the complementary variables is indeed hidden from us. (*Are there quantum jumps?* , 1987, p.201)

Here Bell refers to the fact that in Bohm's theory the detailed trajectories of the microscopic particles are not observable. While this unobservability is a *consequence* of the very structure of Bohm's theory, many physicists quickly objected: After all, physics is about prediction, about observations, not about things which cannot be observed.

But to admit things not visible to the gross creatures that we are is, in my opinion, to show a decent humility, and not just a lamentable addiction to metaphysics. (1987)

The very existence of Bohm's theory, agreeing as it did in its predictions with those of orthodox quantum theory, quite naturally, under the circumstances, raised many questions for Bell:

But why then had Born not told me of this "pilot wave"? If only to point out what was wrong with it? Why did von Neumann not consider it? More extraordinarily, why did people go on producing "impossibility" proofs, after 1952, and as recently as 1978? When even Pauli, Rosenfeld, and Heisenberg, could produce no more devastating criticism of Bohm's version than to brand it as "metaphysical" and "ideological"? Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show us that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice?(1982)

Of course, the most immediate question raised was, or should have been, What went wrong with the “proof”?

The realization that von Neumann's proof is of limited relevance has been gaining ground since the 1952 work of Bohm. However, it is far from universal. Moreover, the writer has not found in the literature any adequate analysis of what went wrong. Like all authors of noncommissioned reviews, he thinks that he can restate the position with such clarity and simplicity that all previous discussions will be eclipsed. (*On the problem of hidden variables in quantum mechanics,*, 1966, p.2)

And Bell proceeded to do just that!

Bell analyzed von Neumann's proof as well as other proofs, found that they were based upon rather arbitrary assumptions or axioms, and focused on the the manner in which Bohm's theory violates these assumptions. In so doing he noticed that [Bell 1966, p. 11]:

...in this theory an explicit causal mechanism exists whereby the disposition of one piece of apparatus affects the results obtained with a distant piece.

Bohm of course was well aware of these features of his scheme, and has given them much attention. However, it must be stressed that, to the present writer's knowledge, there is no *proof* that *any* hidden variable account of quantum mechanics*must* have this extraordinary character. It would therefore be interesting, perhaps, to pursue some further "impossibility proofs," replacing the arbitrary axioms objected to above by some condition of locality, or of separability of distant systems.

No sooner said than done! In fact, if we follow the publication dates, done before said—the EPR-Bell's theorem paper(1964) in which it was done appeared almost two years before the paper from which I was just quoting. Publication delay!

Bell interview: ...as a professional theoretical physicist I like the Bohm theory because it is sharp mathematics. I have there a model of the world in sharp mathematical terms that has this non-local feature. So when I first realized that, I asked: "Is that inevitable or could somebody smarter than Bohm have done it differently and avoided this non-locality?" That is the problem that the theorem is addressed to. The theorem says: "No! Even if you are smarter than Bohm, you will not get rid of non-locality," that any sharp mathematical formulation of what is going on will have that non-locality.

Moreover, the nonlocality of Bohm's theory derives solely from the nonlocality built into the structure of standard quantum theory, as provided by a wave function on configuration space, an abstraction which, roughly speaking, combines—or binds—distant particles into a single irreducible reality.

That the guiding wave, in the general case, propagates not in ordinary three-space but in a multidimensional-configuration space is the origin of the notorious ‘nonlocality’ of quantum mechanics. It is a merit of the de Broglie-Bohm version to bring this out so explicitly that it cannot be ignored. (1980, p.115)

Now the relevant experiments have been done (Aspect, 1982), confirming the strange predictions to which Bell was led by his analysis of Bohm's theory. Where does this now leave us?

There is a basic problem: Bohm's theory violates Lorentz invariance, a central principle of physics. Nor can Bohm's theory be easily modified so that it becomes Lorentz invariant. The difficulty here arises from the fundamental tension, the *apparent* incompatibility, between nonlocality and Lorentz invariance. Bell interview:

Now what is wrong with this theory, with David's theory? What is wrong with this theory is that it is not Lorentz-invariant. That's a very technical thing and most philosophers don't bother with Lorentz-invariance and in elementary quantum mechanics books the paradoxes that are presented have nothing to do with Lorentz-invariance.

Those paradoxes are simply disposed of by the 1952 theory of Bohm, leaving as *the* [my emphasis] question, the question of Lorentz-invariance. So one of my missions in life is to get people to see that if they want to talk about the problems of quantum mechanics—the real problems of quantum mechanics—they must be talking about Lorentz-invariance.

And from the last sentence of (to my knowledge) Bell's last publication—the LAST WORD, as it were:

Referring to Bohm's theory and to GRW theory—a modification of quantum theory in which he became interested in his last years, Bell said

The big question, in my opinion, is which, if either, of these two precise pictures can be redeveloped in a Lorentz invariant way.(*Against "measurement"*, Physics World **3**, 33–40, 1990)

I believe that this really is the big question. And I urge it upon you. But I am afraid that in trying to answer this question, we shall miss Bell's help and inspiration very much indeed! And we shall miss Bell's marvelous style, his penetrating wit, and his brilliant clarity!

....If philosophy or religion prompts a person to deny or doubt that humans, or that kangaroos, are land-mammals, the only rational thing to do is to ignore him; and the same holds for science, too, whether past, present, or future.

I may be reminded that some respected physicists have said in recent years that something like Berkeleian idealism is actually a logical consequence of their best fundamental theories. (One of them wrote, for example: 'We now know that the moon is demonstrably not there when nobody looks'.) It would be irrational to believe this logical claim, but if it is true then it would be irrational to believe these physicists' best theories. Fundamental physical theories never say anything about a particular macroscopic physical object, such as the moon; but if they did say something about the moon, then they would say the same thing about all macroscopic physical objects, hence about all land-mammals, and hence about the particular land-mammal, Professor N. D. Mermin, who wrote the sentence I have just quoted. Now it may perhaps be true that Professor Mermin depends for his ease of mind on being an object of attention. This would not even be especially surprising, in view of the powerful emotional root which idealism has in common with religion. But that he depends for his very existence on being an object of attention, is entirely out of the question: it is much more likely (to say the least) that one or more of his scientific theories is wrong. Mammals are very complex, of course, and depend for their existence on a great many things; but somebody's looking at them is not among those things, and everybody knows this.