Probabilities and Quantum Reality: Are There Correlata?

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Abstract

Any attempt to introduce probabilities into quantum mechanics faces difficulties due to the mathematical structure of Hilbert space, as reflected in Birkhoff and von Neumann’s proposal for a quantum logic. The (consistent or decoherent) histories solution is provided by its single framework rule, an approach that includes conventional (Copenhagen) quantum theory as a special case. Mermin’s Ithaca interpretation addresses the same problem by defining probabilities which make no reference to a sample space or event algebra (“correlations without correlata”). But this leads to severe conceptual difficulties, which almost inevitably couple quantum theory to unresolved problems of human consciousness. Using histories allows a sharper quantum description than is possible with a density matrix, suggesting that the latter provides an ensemble rather than an irreducible single-system description as claimed by Mermin. The histories approach satisfies the first five of Mermin’s desiderata for a good interpretation of quantum mechanics, including Einstein locality, but the Ithaca interpretation seems to have difficulty with the first (independence of observers) and the third (describing individual systems).

1 Five Desiderata Plus One

David Mermin is widely acknowledged to be the prince of expositors in the field of quantum interpretation. It is a pleasure to read what he writes, even when you don’t agree with it. When it comes to his Ithaca interpretation of quantum mechanics, I actually agree with five out of six of Mermin’s desiderata for a satisfactory interpretation (Sec. 2 of Ref. 1), and before discussing some points of serious disagreement, let me indicate the areas of overlap. Here are the five desiderata in Mermin’s own words, followed in each case by a selection from his explanatory comments:

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1. The theory should describe an objective reality independent of observers and their knowledge

... A satisfactory interpretation should be unambiguous about what has objective reality and what does not, and what is objectively real should be cleanly separated from what is "known". Indeed, knowledge should not enter at a fundamental level at all.

2. The concept of measurement should play no fundamental role

There is a world out there, whether or not we choose to poke at it, and it ought to be possible to make unambiguous statements about the character of that world that make no reference to such probes. A satisfactory interpretation of quantum mechanics ought to make it clear why "measurement" keeps getting in the way of straight talk about the natural world; "measurement" ought not to be a part of that straight talk. Measurement should acquire meaning from the theory — not vice versa... Physics ought to describe the unobserved unprepared world. "We" shouldn’t have to be there at all.

3. The theory should describe individual systems — not just ensembles

The theory should describe individual systems because the world contains individual systems... and the theory ought to describe the world and its subsystems... In a nondeterministic world probability has nothing to do with incomplete knowledge, and ought not to require an ensemble of systems for its interpretation... The fact that physics cannot make deterministic predictions about individual systems does not excuse us from pursuing the goal of being able to describe them as they currently are.

4. The theory should describe small isolated systems without having to invoke interactions with anything external

... I would like to have a quantum mechanics that does not require the existence of a "classical domain". Nor should it rely on quantum gravity, or radiation escaping to infinity, or interactions with an external environment for its conceptual validity... It ought to be possible to deal with high precision and no conceptual murkiness with small parts of the universe if they are to high precision isolated from the rest.

5. Objectively real internal properties of an isolated individual system should not change when something is done to another non-interacting system

... Einstein used [this] supposition, together with his intuitions about what constituted a real factual situation, to conclude that quantum mechanics offers an incomplete description of physical reality. I propose to explore the converse approach: assume that quantum mechanics does provide a complete description of physical reality, insist on generalized Einstein-locality, and see how this constrains what can be considered physically real.
On these five desiderata I agree two hundred percent with Mermin. In some cases I may be giving his words a slightly different interpretation from what he intended, but at least in broad outline and probably in most of the details, I could not agree with him more, even though I could not possibly have expressed it with such clarity and enthusiasm. But now we come to the sixth desideratum:

6. It suffices (for now) to base the interpretation of quantum mechanics on the (yet to be supplied) interpretation of objective probability

I am willing at least provisionally to base an interpretation of quantum mechanics on primitive intuitions about the meaning of probability in individual systems. Quantum mechanics has taught us that probability is more than just a way of dealing systematically with our own ignorance, but a fundamental feature of the physical world. But we do not yet understand objective probability... I maintain that if we can make sense of quantum mechanics conditional upon making sense of probability as an objective property of an individual system, then we will have got somewhere...

The tone is different, and the bold confidence which characterized desiderata 1 to 5 has changed into something more tentative. Mermin is not sure that this is the right way to go, and I have even greater misgivings. Part of the problem is that I have had a great deal of difficulty making sense of what he means by probability as an objective property of an individual system. While I enjoyed reading the prose, distilling the essential idea out of Refs. 1 and 2 was a chore. The main sticking point was the notion that there can be statistical correlations among the properties of subsystems even if those properties have no objective physical reality: “correlations without correlata”. When repeated reading did not make things clear — something which for me is a common experience, though quite exceptional for papers written by Mermin — I adopted the alternative approach of examining some of the same problems from my own point of view, in hopes that by getting somewhere using my own methods I could better make sense of Mermin’s. The remainder of this paper is the fruit of reflections of this sort.

There is a fundamental difficulty when one attempts to introduce probabilities into quantum theory by a route other than appealing to measurement outcomes, something which both Mermin and I consider unsatisfactory. As discussed in Sec. 2.1, the problem arises in trying to relate the quantum Hilbert space, as interpreted by von Neumann, to the sample space structure required by ordinary probability theory. I then discuss some ways of handling this problem, beginning with the quantum logic approach of Birkhoff and von Neumann in Sec. 2.2, followed in Sec. 3.1 by consistent or decoherent histories — which I simply refer to as “histories”, since this will be unambiguous — as developed by Omnès and me, with major assistance from Gell-Mann and Hartle. Next, in Sec. 3.2 I comment on the successes and limitations of the measurement approach found in standard textbook (“Copenhagen”) quantum mechanics, as seen from a histories perspective. Following this in Sec. 4 I return to the Ithaca interpretation, examining in turn each of its two pillars, as defined by Mermin in Sec. 4 of Ref. 1: the absence of correlata despite the...
existence of correlations, which I take up in Sec. 4.1, and the density matrix as a fundamental objective and irreducible property of a subsystem, the subject of Sec. 4.2. In Sec. 5 I respond to some comments by Mermin on the histories approach, and then summarize my conclusions in Sec. 6.

2 Probabilities and Logic

2.1 The problem with quantum probability

According to Jammer (p. 38 of Ref. 15), Born proposed his probabilistic interpretation of the Schrödinger wave function at about the same time that Schrödinger published the time-dependent version of his equation. The fact that two of the most important principles of modern quantum theory did not originate in the mind of a single genius may be one reason why they have been so hard to combine. However, there is a much more fundamental difficulty connected with the different mathematical structures used for quantum mechanics and probability theory. Quantum theory employs a complex vector space with an inner product, a Hilbert space. Probability theory is founded on the notion of a sample space (e.g., Ref. 16) of mutually exclusive possibilities, one and only one of which occurs, or is true, at any given time, or in any given experimental run. Appropriate subsets of the sample space make up an event algebra whose members are assigned probabilities. For example, if one rolls a die the sample space consists of the six possible outcomes, and if one of these, say 5 spots, occurs, the others do not occur. The event that the number of spots is even belongs to the event algebra, and is assigned a probability of 1/2 for an honest die.

Figure 1: The set of points $\mathcal{P}$ inside the circle correspond to the proposition $P$, the set $\mathcal{Q}$ inside the square correspond to $Q$, the totality of points in the two regions corresponds to $P \lor Q$, and the region of overlap to $P \land Q$.

In classical statistical mechanics the sample space is the phase space of classical Hamiltonian dynamics, spanned by the coordinates and momenta of the different particles which make up the system. If the mechanical system is correctly represented by one of these points it is not represented by any of the others, so one has a set of mutually-exclusive possibilities. Probabilities are then assigned to those subsets of points in the phase space that make up the event algebra, in a way which satisfies the rules given in texts on probability theory. It will be convenient to refer to such subsets as properties. For example the property $P$ that the energy is between 1 and 2 mJ corresponds to the subset of points $\mathcal{P}$ in the phase space.
for which $P$ is true. Its negation $\tilde{P}$, NOT $P$, corresponds to the complement $\sim P$ of the set $\mathcal{P}$ in the phase space, the points for which $P$ is false (i.e., the energy lies outside the specified range). Given another property $Q$ corresponding to a different set $\mathcal{Q}$ of points in the phase space, the combined properties $P$ AND $Q$, written $P \land Q$, and $P$ OR $Q$, written $P \lor Q$, correspond to sets of points in the phase space which can be represented schematically in a Venn diagram, Fig. 1. Thus in classical statistical mechanics there is a natural correspondence between propositions used to describe the system and sets of points in the phase space, and between logical operations on the propositions and set-theoretical operations on the corresponding sets.

Figure 2: Two-dimensional Hilbert space represented schematically by the real plane, with origin at the point where the rays intersect.

The reason for mentioning such elementary matters is that in quantum mechanics the situation is very different if we interpret the quantum Hilbert space in the manner indicated by von Neumann in Sec. 5 of Ch. III of Ref. [4]. He associates propositions not with arbitrary collections of points, but instead with linear subspaces of the Hilbert space. (To be precise, with closed linear subspaces; hereafter all such qualifications are left to the reader sophisticated enough to know they are needed.) The simplest nontrivial linear subspace is a ray consisting of all kets of the form $\{\alpha |\psi\rangle\}$, where $|\psi\rangle$ is some fixed vector in the space, and $\alpha$ is any complex number. One can think of this geometrically as an infinite line through the origin, as in Fig. 2 where a two-dimensional complex Hilbert space is represented schematically by its real counterpart. The point is that $\alpha |\psi\rangle$ for $\alpha \neq 0$ has precisely the same physical significance as $|\psi\rangle$; the customary normalization of kets so that $\langle \psi |\psi\rangle = 1$ is convenient but not essential, and the arbitrariness of the phase is simply the arbitrariness of $\alpha$ when $|\alpha| = 1$. The logical proposition $P$ associated with a ray $\alpha |\psi\rangle$ is just the assertion that “the system is in the state $|\psi\rangle$”, whatever that may mean. For example, if $|\psi\rangle$ is a nondegenerate energy eigenstate with energy $E$, we can interpret $P$ as “the energy has the value $E$”. As an example of a larger subspace, consider the proposition that the energy of a harmonic oscillator is less than $3\hbar \omega$. This corresponds to a three-dimensional subspace of
the Hilbert space consisting of all linear combinations of the kets $|0\rangle$, $|1\rangle$, and $|2\rangle$, where $|n\rangle$ is the eigenstate with energy $(n + 1/2)\hbar\omega$.

The differences between a quantum Hilbert space and a classical phase space begin to appear when one considers the negation $\tilde{P}$ of a proposition $P$ associated with a subspace $\mathcal{P}$. If we follow von Neumann, $\tilde{P}$ does not correspond to the complement $\sim \mathcal{P}$ of $\mathcal{P}$, the set of all vectors in the Hilbert space which do not belong to the subspace $\mathcal{P}$, but rather to the orthogonal complement $\mathcal{P}^\perp$ of $\mathcal{P}$, the collection of all vectors $|\phi\rangle$ with the property that $|\phi\rangle$ is orthogonal to every $|\psi\rangle$ in $\mathcal{P}$ in the sense that $\langle \phi | \psi \rangle = 0$. There are various reasons why this is a sensible proposal. To begin with, $\mathcal{P}^\perp$ is a subspace of the Hilbert space, whereas $\sim \mathcal{P}$ is not, so that if we want the negation of a property to itself be a property associated with a subspace, we cannot use $\sim \mathcal{P}$. Second, what distinguishes a Hilbert space from any old (complex) vector space is the existence of the inner product $\langle \phi | \psi \rangle$, and since orthogonality is a rather natural and fruitful concept from the mathematical point of view, one anticipates that it ought to play a significant role in the physical interpretation. Third, if $P$ is the projector (orthogonal projection operator) onto $\mathcal{P}$, then $\tilde{P} = I - P$ (with $I$ the identity) is the projector onto $\mathcal{P}^\perp$, and this way there is a nice analogy between quantum projectors and indicator functions on the classical phase space; for details, see Sec. 4.4 of Ref. [12]. (Using the same symbol $P$ for a proposition and the corresponding projector will cause no confusion.) The reasons given thus far appeal to mathematical elegance, and I am sure this had something to do with von Neumann’s choice. But is it physically reasonable? This can only be discussed by considering physical examples. For instance, if $P$ is the quantum proposition that the energy of the oscillator is less than $3\hbar\omega$, then its negation $\tilde{P}$ corresponds to the subspace where the energy is greater than (or equal to, but it cannot be equal to) $3\hbar\omega$. Is that reasonable? I think so, but (following a strategy I learned from Mermin) I invite any reader who disagrees to come up with something better.

Why all this discussion of negation? Because it leads to a certain oddity visible in Fig. 2, which shows one ray $\mathcal{P}$ as a solid line and its orthogonal complement $\mathcal{P}^\perp$ as a dashed line. But there are many other rays, such as the dotted one labeled $\mathcal{Q}$, each associated with a quantum proposition. Both $\mathcal{P}$ and $\mathcal{Q}$ are rays, the smallest possible subspaces of the Hilbert space (aside from the origin, which does not represent a physical property), and therefore they are the quantum counterparts of points in a classical phase space. But in a classical phase space, either two points coincide, or each lies in the complement of the other, so that either they mean the same thing, or else the truth of one implies the falsity of the other. Now $\mathcal{P}$ and $\mathcal{Q}$ in Fig. 2 clearly do not mean the same thing, but $\mathcal{Q}$ does not lie in the subspace $\mathcal{P}^\perp$ corresponding to the negation of $\mathcal{P}$, nor $\mathcal{P}$ in the subspace corresponding to the negation of $\mathcal{Q}$, so the truth of one does not imply that the other is false. Thus if we follow von Neumann in associating subspaces and their orthogonal complements with quantum propositions and their negations, an oddity emerges in terms of nonorthogonal rays, which have a nonclassical relationship to each other. To put the matter another way, it is only orthogonal quantum states which are distinct in the same sense that two different classical states, corresponding to different points in the classical phase space, are distinct.

It helps to have a term to describe the relationship between rays such as $\mathcal{P}$ and $\mathcal{Q}$ in Fig. 2, which are neither identical nor orthogonal, and we shall call them incompatible. Two
rays are incompatible if and only if the corresponding projectors do not commute with each other (which means that incompatibility is very much a quantum notion). Similarly, two arbitrary subspaces, or their projectors, or the corresponding propositions, are incompatible if and only if the projectors do not commute. Otherwise they are compatible. Quantum incompatibility in this technical sense should be clearly distinguished from the relationship of being mutually exclusive. Two classical propositions which are mutually exclusive stand in a relationship such that the truth of one implies the falsity of the other. In the quantum case two subspaces or propositions or projectors are mutually exclusive if the projectors are orthogonal to each other in the sense that their product (in either order; it makes no difference) is zero.

As mentioned previously, ordinary (classical) probability theory is based upon the notion of a sample space of mutually-exclusive events. It is quite clear what this means in terms of subsets of the phase space: they are mutually exclusive if they do not overlap, if they have no points in common. But what should one do in the quantum case, where, as we have seen, there are rays in the Hilbert space which are not identical but also not distinct, at least in the same sense that nonidentical classical things are distinct? Choosing a sample space is a fairly trivial matter in classical physics. (Well, not exactly, but by now the rules have been worked out and the experts can explain Borel sets to you.) But in quantum physics it is far from trivial, because you have to decide what to do with incompatible subspaces. This is a fundamental problem facing anyone who wants to introduce probabilities into quantum mechanics in a consistent fashion, at least if these probabilities are to obey the usual rules of probability theory. To be sure, that may be too restrictive. Perhaps what is needed in quantum theory is a new theory of probability, with different rules. In that case the fundamental problem is to find new rules which are mathematically consistent and make physical sense. In any event, there is a nontrivial problem.

### 2.2 Quantum logic

Since von Neumann was a skilled mathematician, it would be surprising had he overlooked the odd effects of his scheme for associating quantum propositions and their negations with subspaces of the Hilbert space. He didn’t, and in a 1936 paper[5] — one much less cited than the 1935 paper of Einstein, Podolsky, and Rosen[17], though in my opinion it is equally important — he and Garrett Birkhoff tackled the problem of the relationship between incompatible propositions by suggesting that their conjunction \( P \land Q \), \( P \land Q \), be associated with the intersection \( P \cap Q \) of the corresponding subspaces of the Hilbert space. This is a natural choice in that the same operation, the intersection of two sets, is appropriate for a classical phase space, and because the intersection of two (closed) subspaces of the Hilbert space is another (closed) subspace. In the case of compatible \( (PQ = QP) \) propositions this leads to no problems, but if one also adopts it for incompatible propositions, the result is a logical peculiarity.

Rather than an abstract discussion, let us consider a two-dimensional Hilbert space representing the spin angular momentum of a spin-half particle. Let \( P \) project on the ray \( \mathcal{P} \) in Fig. 2 passing through the ket \( |z^+\rangle \), corresponding to the property that the \( z \) component
of angular momentum $S_z$ is equal to $+1/2$ (in units of $\hbar$). Its negation $\bar{P}$ is the property $S_z = -1/2$. Let $Q$ project on the ray $Q$ passing through $|x^+\rangle$, corresponding to the proposition $S_x = +1/2$ for the $x$ component of angular momentum, with negation $S_x = -1/2$. In Birkhoff and von Neumann’s quantum logic $P \land Q$ corresponds to the intersection of the rays $P$ and $Q$, which is the origin or zero vector of the Hilbert space. This represents the proposition that is always false; it is the quantum counterpart of the empty subset of the points of a classical phase space.

One’s initial reaction is that calling $S_z = +1/2$ and $S_x = +1/2$ always false represents good physics. We know, or at least we were taught, that there is no way in which one can simultaneously measure both $S_z$ and $S_x$ for a spin-half particle, and if $S_z = +1/2$ and $S_x = +1/2$ could ever be true, then surely some clever experimentalist would have figured out a way to check it, and by now would have received a Nobel prize. However, there is a problem. Logic tells us that the negation of a proposition that is always false is a proposition that is always true. The negation of $S_z = +1/2$ and $S_x = +1/2$, following the usual rules, is $S_z = -1/2$ or $S_x = -1/2$. Is it always true that either $S_z = -1/2$ or $S_x = -1/2$ or both? Let’s consider a case in which $S_z = +1/2$, so its negation $S_z = -1/2$ is not true. Are we entitled to conclude that if $S_z = +1/2$, then it is always the case that $S_x = -1/2$? But that contradicts the fact (at least in the Birkhoff and von Neumann scheme) that $S_z = +1/2$ and $S_x = -1/2$ is always false, for precisely the same reason that $S_z = +1/2$ and $S_x = +1/2$ is always false.

When you reach a contradiction in this fashion, the first thing to do is to go back and check that you have not made some silly mistake, and if that doesn’t solve the difficulty, you suspect that one of your assumptions is wrong. But there is a third way, and this is what Birkhoff and von Neumann proposed: change the rules of logic! Their paper made the quite specific proposal that one abandon, or at least modify, the distributive identities, that is

$$P \land (Q \lor R) = (P \land Q) \lor (P \land R)$$

and its counterpart with $\land$ and $\lor$ interchanged. This proposal and variations on the same theme have given rise to a considerable body of literature. A quarter of a century ago Jammer (Ch. 8 of Ref. 15) said that the Birkhoff and von Neumann proposal had given rise to a lot of discussion among the philosophers, but very little interest among the physicists. That situation has not changed, and I think it regrettable. Not because the Birkhoff and von Neumann scheme solves the conceptual problems of quantum mechanics, but rather because — and it is in this respect that their work has a parallel with Einstein, Podolsky and Rosen — it raises a significant issue, a problem that needs to be thought about. There are important logical issues lurking in the foundations of quantum mechanics, and ignoring them, or treating them with contempt, does not make them go away. Indeed, it might just be the case — let me be honest, I think it is the case — that paying more attention to logical issues would speed up the resolution of the difficulties raised by Einstein and his colleagues.

To be sure, our immediate concern is with probabilities, not logic. But the structure of ordinary probability theory, in particular the sample space and event algebra, is closely linked with propositional logic, which is in some sense a limiting case of probability theory, with probabilities 1 and 0 the counterparts of TRUE and FALSE. Thus any attempt to base
quantum probabilities on the quantum logic of Birkhoff and von Neumann, or the various
variants that have appeared since, will require a new set of rules.

3 Histories and Measurements

3.1 Quantum histories

One might also think of the (consistent or decoherent) histories approach to quantum inter-
pretation as constituting a revised logic, but if so it is a much more conservative revision
than that proposed by Birkhoff and von Neumann. For it requires no modification of the
rules of propositional logic, or of ordinary probability theory; provided — and here is where
conservatism comes in — one strictly limits the domain of discourse, the set of things which
can sensibly be said about a quantum system, in an appropriate way.

Again, the example of a spin-half particle is useful for illustrating what this does and
does not mean. At least under some circumstances it is reasonable to say of such a particle
that \(S_z = +1/2\), because there is a corresponding ray in the quantum Hilbert space, so
a statement of this sort is consistent with quantum mathematics as interpreted by von
Neumann. Similarly, it is (sometimes) reasonable to say that \(S_x = +1/2\), or that \(S_w = +1/2\)
where \(w\) denotes any direction in space. On the other hand, \(S_z = +1/2\) AND \(S_x = +1/2\)
is a meaningless statement, for there is nothing in the Hilbert space that could conceivably
correspond to it. Every ray signifies \(S_w = +1/2\) for some direction \(w\), and there are none left
over to represent a proposition with AND in it of the sort we are considering. At first it might
seem as as if calling \(S_z = +1/2\) AND \(S_x = +1/2\) meaningless is the same as saying that it
is always false, and in ordinary language there is, indeed, not much difference between the
two. However, the negation of a false but meaningful statement is a true statement, whereas
the negation of a meaningless statement is equally meaningless, neither true nor false. The
sense of “meaningless” employed here is the same as when in ordinary logic one combines
two meaningful propositions \(P\) and \(Q\) in the form \(P \land \lor Q\). Such an expression does not
conform to the rules given in books on logic for forming meaningful sentences, and hence it
is a waste of time discussing what it means, whether it is true or false, or what its negation
might be.

What the histories approach does is specify that a proper domain of logical discourse in
quantum theory is limited to a set of compatible propositions corresponding to subspaces with
projectors that commute with each other. Within such a framework (my terminology) or
logic (Omnès) all the usual rules of (classical!) propositional logic work just fine, without the
slightest modification. Logicians know, and the books on logic tell us, that logical reasoning
requires a well-defined domain of discourse or collection of propositions (the well-formed
sentences, or whatever), and that specifying this collection is a task, sometimes a nontrivial
one, which needs to be done before you draw your conclusions. We physicists, amateurs
in this as in every other field outside our specialty, are inclined to plunge ahead and leave
the mathematical or logical niceties to some future paper or (more often) to someone else.
Frequently we get away with it, but not always, and if Omnès and I are right, quantum
mechanics is one case in which it pays to pay attention to what you are doing.
In classical physics we do not have to be specific about the domain of discourse, because in most instances only one reasonable possibility will come to the mind of a trained physicist, so there is no point spending time at the beginning of a conversation or a paper stating what it is. As soon as I mention the angular momentum of a spinning top, you know precisely what the phase space is, so you know what constitutes a meaningful description. On the other hand, in quantum mechanics there are always many different frameworks (logics) to choose from. You might be interested in talking about the $z$ component of angular momentum of a spin-half particle, in which case you use the $S_z$ framework corresponding to the orthonormal basis $|z^+\rangle, |z^-\rangle$. These represent two mutually-exclusive possibilities, because the product of the corresponding projectors $[z^+] [z^-]$ is zero, where $|\psi\rangle$ is a convenient shorthand for $|\psi\rangle\langle\psi|$, and so they constitute a sample space suitable for a probabilistic description of the system. On the other hand, if you are interested in the $x$ component of the particle’s spin you use the $S_x$ framework with projectors $[x^+]$ and $[x^-]$. The $S_z$ and $S_x$ frameworks are incompatible: they are logically disjoint, you cannot combine propositions from one in a meaningful way with propositions from another.

More generally, a possible framework for discussing a quantum system at a single time always corresponds to a decomposition of the identity, a collection of projectors that commute with one another and sum to the identity $I$. They represent mutually exclusive possibilities, since the product of any two (distinct) projectors belonging to the collection is zero. These projectors together with sums of two or more of them constitute the collection of propositions that make sense in this framework. Two frameworks are compatible if the projectors in one commute with the projectors in the other; otherwise they are incompatible. To describe a quantum system developing in time you use a framework consisting of histories: each history is a sequence of projectors representing properties of the system at a succession of times. The rules for constructing frameworks of histories are more complicated than those for describing a quantum system at a single time, and in fact there has been some diversity in the proposed rules, although in the end most of us seem to have settled on a formulation due to Gell-Mann and Hartle. A given framework, either for the properties of a quantum system at a single time or for a collection of histories, is a collection of commuting projectors, and the corresponding logical propositions relate to one another in precisely the same way as in familiar, classical logic. For this reason, introducing probabilities is no problem: one simply follows the usual rules of probability theory, and all the usual rules are satisfied, as long as the discussion is confined to a single framework.

But then, how are we to relate the propositions or the probabilities that occur in different, incompatible frameworks? To this Omnès and I have a simple answer: don’t! This is stated more formally in the single framework rule. It says that propositions belonging to incompatible frameworks cannot be combined in any way, shape, or form when constructing a meaningful description of the quantum world. At one level the single framework rule agrees pretty well with the quantum physicist’s intuition. Talking about a particle which is in an energy eigenstate of a harmonic oscillator AND has a position associated with it, or has a position at the same time as it has a momentum, or a spin half particle with values for both $S_x$ AND $S_z$ — these things make us uncomfortable. So it is nice to have a rule which gives a reason — the projectors for the two items don’t commute with each other — for not doing
what would in any case make us nervous. But there is another level at which the rule is much less intuitive. This is when a projector $P$ for some property that interests us belongs to two different, incompatible (because some other projectors do not commute) frameworks $\mathcal{F}_1$ and $\mathcal{F}_2$. We have just used $\mathcal{F}_1$ to compute a probability for $P$, and what could be more reasonable than to suppose that $P$ must have the same probability in $\mathcal{F}_2$? Indeed, how could it possibly be otherwise? This sort of reasoning leads to all manner of paradoxes, and it is by insisting on the single framework rule that the histories approach avoids or, as I like to put it, tames these paradoxes (see Chs. 20 to 25 of Ref. [12]).

The single framework rule has been frequently misunderstood, so it may be worth making a couple of brief comments about what it does and does not mean. First, it is not a prohibition on creating quantum descriptions using any and every framework which strikes your fancy. You can talk about, and it makes sense to talk about, $S_z$ or $S_x$ or whatever you please. What is prohibited is combining incompatible frameworks, thinking of them as somehow both referring to the same system at the same time. Second, the relationship of two incompatible frameworks is not that they are mutually exclusive, that if one is true the other must be false. We have already seen that this is the wrong way (at least if you accept von Neumann’s notion of negation) of viewing two incompatible propositions, and it is equally wrong when it comes to incompatible frameworks. It is useless to search for some “law of nature” which identifies the “right” framework for describing a quantum situation, because alternative frameworks are not related in this fashion. I shall have more to say about this later.

In summary, the histories approach requires reasoning in a different way about quantum systems than we are accustomed to doing in the case of classical systems, and in this sense requires a “new logic”. However, what is new is not modus ponens or the rule of the excluded middle. Instead it is a syntactical rule that specifies which quantum descriptions make sense, and imposes the painful discipline (slightly less painful when you get used to it) of paying attention to what constitutes a meaningful domain of discourse. There are some significant benefits from exercising discipline. One is that you can think clearly about what is going on in a quantum system without running into logical paradoxes. Next, quantum probabilities can be manipulated using exactly the same rules as for their classical counterparts. Third, all those mysterious nonlocal influences are eliminated from quantum theory: they owe their existence to logical errors, and correcting these errors banishes them from the scene.

### 3.2 Quantum measurements

I am in complete agreement with Mermin’s desideratum 2, that the concept of measurements should play no fundamental role in quantum interpretation, and consequently I share his dissatisfaction with the “Copenhagen” interpretation, understood to mean the ideas of Bohr and the other founding fathers as these have come down to us in the textbooks and lectures from which we first learned the subject. Nonetheless, one must admit that the Copenhagen has been remarkably successful in introducing at least some probabilities into quantum theory, namely those referring to the outcomes of measurements, and it seems worth investigating, using various frameworks of the sort introduced in Sec. [3.1], the reasons
for its success, and what are its limitations.

One of the many possible frameworks that can be used to analyze the time development of a quantum system is what I shall call a unitary framework: let $|\Psi_t\rangle$ be a solution of Schrödinger’s equation, and at any time let the projector $[\Psi_t]$ onto $|\Psi_t\rangle$ be part of the decomposition of the identity used to describe what is going on. Consider a situation in which a measurement is about to take place, and at the initial time $t_0$ let $|\Psi_0\rangle$ represent the “ready” state of the apparatus and that of the system it will soon be measuring. Let $|\Psi_1\rangle$ be the resulting state at $t_1$ when the measurement interaction has taken place. As Schrödinger\(^{(18)}\) himself taught us, one can easily imagine an initial $|\Psi_0\rangle$ which is physically reasonable, but develops into a monstrosity $|\Psi_1\rangle$ that I like to call a macroscopic quantum superposition (MQS), but everyone else refers to as “Schrödinger’s cat”, because it is a superposition of states in which the apparatus pointer indicating the outcomes has various different positions. The quantum historian will admit he does not understand $|\Psi_1\rangle$ any better than you do, but will add that if you insist that this represents the physical state of the total system at time $t_1$ — that is, if you are using the unitary framework — then anything else you want to say about the system at this time must be expressed in terms of projectors which commute with the projector $[\Psi_1]$ onto $|\Psi_1\rangle$. In particular, you certainly cannot speak about a pointer pointing in a particular direction (or a cat which is dead or alive), because whatever projector could be employed for this purpose will not commute with $[\Psi_1]$. The situation is analogous to asserting that $S_z = \pm \frac{1}{2}$ and then trying to ascribe values to $S_x$. It makes no sense.

As seen from a histories perspective, what theorists trained in Copenhagen actually do when they calculate measurement outcomes is to use what I call the dragon framework, after Wheeler’s striking image of the great smoky dragon\(^{(20)}\) which at some instant bites the detector, at which point the dragon collapses, to be replaced by a well-defined pointer position. In histories language, unitary time development is employed up to just before $t_1$, but then one introduces a physically reasonable (or at least interpretable) pointer decomposition of the Hilbert space identity into subspaces (projectors) such that in each of then the pointer has a well-defined position. Constructing such a decomposition is in principle not too difficulty, since any two states of affairs which are distinguishable at a macroscopic level will be represented by quantum projectors whose product is (essentially) zero, and thus we have a good sample space of mutually exclusive possibilities. This framework and the unitary framework constitute equally valid but mutually incompatible ways of describing quantum time development, so it makes no sense to combine them, and you will only confuse yourself if you think of them as equivalent for all practical purposes or, indeed, for any purposes whatsoever. (In saying this I am siding with Bell\(^{(21)}\) in his critique of an earlier Ithaca interpretation, Sec. 20 of Ref.\(^{(22)}\).) Note that just as the unitary framework excludes any discussion of the probabilities of measurement outcomes, in the same way using the dragon framework excludes any discussion of the MQS states of different pointer positions once you have introduced the pointer decomposition of the identity. The precise time at which the dragon collapses is a choice that is up to the quantum physicist; indeed, there are many different (incompatible) dragon frameworks, and since there is no “right” framework, there is also no point in discussing when it is that the collapse occurs. To put it another way, collapse
is something that occurs in the theorist’s notebook, not the experimentalist’s laboratory.

But if collapse can be pushed around in time, is there any reason that it cannot be pushed back to shortly before the measurement takes place? No there is not, and if you do this you arrive at what I shall call the measurement framework in which just before the measurement takes place the soon-to-be-measured system actually has the property corresponding to the after-the-measurement-occurs pointer position. That means the measurement is actually a measurement! One of the few idiocies to escape Bell’s scathing criticisms (21) of the Copenhagen measurement approach is that it is not a theory of measurement in any reasonable sense of the word. Ordinary macroscopic measurements, such as the height of a table, tell one a property of the measured system both before and after the measurement takes place. Typical measurements of properties of quantum entities in the laboratory, such as the energy of an alpha particle, tell one what the property was before it was, often quite severely, altered by its interaction with the measuring apparatus. A quantum theory of measurement that can tell us the probability of a pointer position, but is incapable of relating that position to some earlier state of affairs is of no use for analyzing the experimental data that accumulates so rapidly at particle accelerators! Despite the prominence it receives in textbooks, the von Neumann theory of measurement, Ch. VI of Ref. [4], in which a property of the measured system after the measurement takes place is correlated with the position of a pointer indicating the outcome of the measurement, is actually of very limited utility for discussing what actually goes on in the laboratory. However, in those circumstances in which it can be (properly) applied there is also a framework which justifies the usual conclusions in terms of conditional probabilities; for details, see Sec. 18.2 of Ref. [12].

The measurement framework is one step in the right direction, but what particle experimentalists actually use when designing their equipment and analyzing their data is what I shall call the practical framework in which a particle described quantum mechanically moves along what on macroscopic length scales is a classical trajectory from the point where it was produced (or scattered) towards whatever detector eventually detects it. And if it is detected at some point, you know that a short time earlier it was headed toward that point and not in some other direction. The practical framework, let me emphasize, provides a fully quantum-mechanical description of what is going on, and it justifies the usual phenomenological talk in which particles possess certain properties before a measurement takes place, or in the complete absence of any measurement. It also indicates the limits of validity of such talk: why, for example, you may (but will not always) be in trouble if your description says that the neutron passed through a definite arm of the interferometer. The histories approach justifies (at least most of the time) the intuitive belief of experimental physicists that they are dealing with real particles moving along in the manner they have been accustomed to talking about. For more on this topic see Ref. [23] and pp. 123ff of Ref. [7].

In summary, the main defect of Copenhagen, viewed from the histories perspective, is not what it says about measurement outcomes and their probabilities, which can be readily justified using the fully-consistent histories interpretation, but rather its inability to push further and relate measurement outcomes to the microscopic properties which are being measured. In practice physicists who deal with real measurements tend to believe that they are really measuring something: they talk as if the “measurata”, or whatever term Mermin
approves, really exist. (For a few examples of situations where this sort of talk is reasonable, see Sec. 3 of Ref. \[1\], Sec. 4.1 of Ref. \[7\], and Sec. IV of Ref. \[23\].) The histories approach, unlike Copenhagen, tells us why quantum mechanics, properly interpreted, justifies this belief.

One can also view the matter in a different way. Restricting the discussion to macroscopic measurement outcomes, which is really what the great smoky dragon amounts to, is an example of limiting the domain of logical discourse in a manner consistent with the single framework rule. Sticking to macroscopic events as they occur in the world around us is a safe thing to do because, as seen quantum-mechanically, the macroscopic “classical” limit can always be described using a single quasi-classical framework — more on this at the end of Sec. \[4.1\]. It is safe, but it is inadequate, for physicists need to be able to discuss the microscopic world in a physically meaningful way; we need to talk about particles and spins and such things. Quantum mechanics interpreted using the single framework rule allows us to do this without getting into contradictions.

### 4 The Ithaca Pillars

#### 4.1 Correlations without correlata

The first pillar of the Ithaca interpretation is *objective probability*, the notion that correlations (more generally, probabilities) are the only fundamental and objective properties of the world. The term “objective” could mean various things. One is that quantum mechanics provides specific rules for calculating probabilities, e.g., the probability that at the end of the measurement the apparatus pointer will be directed at 2 rather than 1, and that two physicists who accept the principles of the theory and start with the same initial state will assign the same values. It is not a matter of subjective judgments, of saying “my hunch is the chance of its happening is less than 1/2.” If “objective” is understood in this sense, most quantum physicists would probably say our probabilities are objective. An alternative interpretation could be that quantum dynamics is intrinsically stochastic rather than deterministic, and this is an objective fact about the world. Many, though not all, quantum physicists would agree.

But Mermin seems to have something different in mind, since he refers to objective probability as an *irreducible* feature of the world, not linked in any way with human ignorance. Consider an example. Just before a coin is tossed I assign a probability of 1/2 to each outcome, but after it has been tossed and I have seen the outcome, I assign a definite value, \(H\) or \(T\). In this case the earlier uncertainty has been reduced, the earlier probability replaced by a new one conditioned on new data. You can also imagine a quantum coin toss in which \(H\) and \(T\) correspond to which of two photomultipliers detects a photon which has just passed through a beam splitter. In any case, after the toss he who knows what happened knows more than he who does not know. I think Mermin intends his objective probability to be something that cannot be reduced in this way, perhaps like a coin which is still spinning, so it really does not make sense to say \(H\) or \(T\).

In particular, Mermin asserts that *correlations* between subsystems of a quantum system are real and objective. By this he means joint probabilities of the form \(\Pr(A, B)\), where \(A\)
is some property (projector) associated with subsystem $A$ and $B$ a property of subsystem $B$, when the total system has a Hilbert space $A \otimes B$, and their generalizations to three or more subsystems. If $B$ is equal to the identity, $\text{Pr}(A, B)$ becomes the probability $\text{Pr}(A)$ of $A$ by itself without reference to the other subsystem $B$, so this is included in what Mermin refers to as a “correlation”. And since any system to which anyone except a cosmologist applies quantum mechanics can be thought of as part of a larger universe, it seems to me that Mermin is asserting the objective reality of all quantum probabilities of the type

$$\text{Pr}(E) = \langle \Psi | E | \Psi \rangle,$$

where $E$ is any projector on the Hilbert space, and $|\Psi\rangle$ is the quantum state of the total system. There are, to be sure, passages where he stresses the notion of a subsystem, but I think most if not all of what he wants say about objective probabilities can be formulated without reference to subsystems, and is a bit clearer in this more general framework. In further support of this idea is the fact that Mermin does not care how one splits the Hilbert space up into subsystems, which if it can be done at all can be done in a large (uncountably infinite) number of ways. Adding a bit of casuistry, any Hilbert space is the tensor product of itself with the one-dimensional Hilbert space of complex numbers, and in this sense it is its own subsystem. Anyway, I shall proceed on the assumption that the subsystem structure as such is not central to “correlations without correlata”, which is basically equivalent to “probabilities without an event algebra”.

In everyday life we do not normally think of probabilities as things that exist, or at least their existence is less real than their referents, the states of affairs of which they are probabilities. Saying the probability is $1/2$ that the coin toss will result in $H$ is an abstract notion whose significance has long been debated. But the referent, the actual outcome of the toss, is part of the real world, and can have significant consequences, as when it determines which team has the ball when the game begins. So Mermin’s including correlations, which is to say probabilities, among real things, among what Bell would have called quantum beables, is surprising. But what is truly astounding is his claim that while (at least some) quantum probabilities are real, their referents are not part of physical reality. While $\text{Pr}(A)$ (or $\text{Pr}(A, B)$ if one considers subsystems as essential) is an objective, real property, $A$ (or $A$ and $B$) does not exist. This is a truly radical approach to quantum interpretation, and one’s first inclination is to dismiss it as unintelligible nonsense.

But Mermin is not some antiscientific crank, and other reputable scholars have suggested that quantum theory requires a radical revision of our view of reality, even to the point of abandoning conventional logic (Sec. 2.2). So let us try and understand what in the (quantum) world has driven Mermin to this (seemingly) crazy conclusion. The key passage is Sec. IX of Ref. 2 where, as in App. B of Ref. 1, Mermin discusses Hardy’s paradox. Elsewhere Mermin gives us his own lights-and-switches version, and I strongly recommend it to anyone who wants to get a feeling for what is genuinely paradoxical about Hardy’s paradox. There is no need to repeat it here, for one can get to the crucial logical and probabilistic issues without making reference to a particular physical setup. In a four-dimensional Hilbert space, $A$, $B$, $C$, and $D$ are four appropriately chosen properties, each a projector onto a 2-
The pairs
\[ AC \neq CA, \quad BD \neq DB \]  
(3)
do not commute, whereas \( A \) commutes with \( B \) and \( D \), as does \( C \). The negations are \( \tilde{A} = I - A \), \( \tilde{B} \), \( \tilde{C} \), and \( \tilde{D} \).

For a particular, carefully chosen \( |\Psi\rangle \), probabilities are assigned using (2), and one finds that
\[
\begin{align*}
\Pr(A, \tilde{B}) &= 0, \\
\Pr(B, \tilde{C}) &= 0, \\
\Pr(C, \tilde{D}) &= 0, \\
\Pr(A, \tilde{D}) &> 0.
\end{align*}
\]
(4)
Here \( \Pr(A, \tilde{B}) \) is the probability of \( A \) AND \( \tilde{B} \), obtained by setting \( E = A\tilde{B} \) in (2); \( A\tilde{B} \) is a projector because \( A \) and \( \tilde{B} \) commute, and the same is true for the other products needed in (4).

Figure 3: Venn diagram to illustrate (4).

There is something odd about this collection of probabilities as one can see by looking at the Venn diagram in Fig. 3 for the set of all points in the sample space (think of it as discrete) with probability greater than zero. The points inside circle \( A \) are all the ones (of positive probability) for which \( A \) is true, those inside \( B \) (including those inside \( A \)) the ones for which \( B \) is true, and so forth. That \( A \) lies inside \( B \) follows from the fact that \( \tilde{B} \) is the set of points outside \( B \), and the first equality in (4) tells us that this set has no points in common with \( A \). In the same way the other equalities tell us that \( B \) lies inside \( C \), which lies inside \( D \). Hence, as evident from the figure, \( A \) has no points in common with \( \tilde{D} \), the set of points outside \( D \). But this contradicts the final inequality in (4).

What is wrong? Let us first analyze the situation using the histories approach, which demands that sample spaces be specified before probabilities can be defined. Since nothing

\footnote{The translation to Mermin's notation in Sec. IX of Ref. 3 is as follows: \( A = 2'G, \tilde{A} = 2'R, B = 1R, \tilde{B} = 1G, C = 1'G, \tilde{C} = 1'R, D = 2R, \tilde{D} = 2G \).}
has been said in advance, the sample space of \( \Pr(\bar{A}, \bar{B}) \) must be specified implicitly by the fact that the corresponding event algebra contains \( A \) and \( \bar{B} \). Since \( A \) commutes with \( B \), and therefore \( \bar{B} \), there is such an event algebra, which contains negations, thus \( A \) and \( B \), and conjunctions, such as \( A \land B \) corresponding to the projector \( AB \). The result is a unique sample space associated with a particular orthonormal basis of the four-dimensional Hilbert space, with the basis vectors representing four mutually-exclusive possibilities. Similarly, each of the other probabilities in (4) corresponds to a particular orthonormal basis or sample space for which the corresponding probability makes sense. It turns out that the four different bases associated with the four probabilities in (4) are all distinct, so each probability refers to a different sample space. This is also obvious from (3), since one cannot combine sample spaces containing noncommuting projectors. And this, says the historian, explains what is wrong with our argument based on the Venn diagram in Fig. 3: it assumed a single sample space. Hence the contradiction was a product of careless reasoning: you cannot combine probabilities referring to different sample spaces.

While this noncombination rule is equally valid for classical and quantum physics, in the former it can usually be ignored, because behind one’s probabilistic reasoning there is always a single sample space. Even if it has not been explicitly identified, the fact that all probabilities refer to this single sample space means that no contradictions of the type we are considering can arise. But in quantum mechanics there is not always a single sample space. Ignoring the rules can then lead to trouble, as in the present example, which can be thought of as a probabilistic analog of the mistake a beginning student makes when he carelessly assumes that in quantum theory \( xp \) and \( px \) are the same because they are the same in classical mechanics.

Mermin’s argument follows a slightly different route. First, using the fact that \( \Pr(A) \), \( \Pr(B) \) and \( \Pr(C) \) are greater than zero, he uses \( \Pr(B | A) = \Pr(A, B) / \Pr(A) \), etc., to rewrite (4) in the equivalent form

\[
\begin{align*}
\Pr(B | A) &= 1, \\
\Pr(C | B) &= 1, \\
\Pr(D | C) &= 1, \\
\Pr(D | A) &< 1.
\end{align*}
\]

Logically combining the first three equalities leads to the conclusion that \( \Pr(D | A) = 1 \), which contradicts the final inequality. From the fact that he deduced a contradiction using conditional probabilities, Mermin draws the conclusion that while the joint probabilities such as \( \Pr(A, B) \) make sense, conditional probabilities such as \( \Pr(B | A) \) do not make sense. And since \( \Pr(B | A) \), the probability of \( B \ given \ A \), makes no sense, this can only be because \( A \) is not “given”, that is, it is not part of physical reality.

The preceding sentence is a paraphrase of what Mermin says in the third to last paragraph of Sec. IX of Ref. 2. He words it a little differently, making use of the fact that \( A \) is, in his rendering of Hardy’s paradox, a property associated with a subsystem, and from this concludes that there is something suspicious about properties of subsystems. But since the analysis given above makes no reference to subsystems, I think Mermin’s reasoning ought
to exclude from physical reality any quantum property (subspace, projector) which might conceivably be a participant in a similar sort of argument leading to a contradiction. It seems unfair to limit attention to properties that refer to subsystems or properties that can serve as conditions inside conditional probabilities. In any case it seems likely, given that a system can be split into subsystems in various ways, etc., that few interesting properties (0 and 1 are uninteresting properties) will escape banishment into nonexistence. Hence it seems to me that, modulo one or two unimportant nuances, Mermin’s “correlations without correlata” asserts the existence of any probability $\Pr(E)$ of the form (2), along with the nonexistence of every $E$. In probabilistic terms, the probabilities exist, but there is no sample space or event algebra.

This is a rather startling solution (assuming it is one) to the problem of introducing probabilities into quantum mechanics. Notice that the contradiction requires the noncommutivity in (3), for otherwise quantum probabilities assigned using (2) obey all the usual rules of probability theory. Therefore what one encounters in Hardy’s paradox, formulated in this way, is the basic logical problem pointed out by Birkhoff and von Neumann, Sec. 2.2, manifested in the fact that the usual rules for probabilities don’t work the way they should. It is useful to compare Mermin’s solution to this problem with those discussed earlier. Birkhoff and von Neumann allow all propositions, all projectors, to be part of a single logical structure, and get rid of contradictions of the sort Mermin (or Hardy) have discovered by appropriate alterations of the rules of logic. The histories approach with its single framework rule denies the validity of certain logical combinations which Birkhoff and von Neumann would allow, and thereby preserves classical rules of reasoning on appropriately circumscribed domains of discourse. Mermin’s solution is to abolish the entire logical structure based on subspaces of the Hilbert space, or at least deny that this has anything to do with the physical world. I know of no other quantum interpretation which even comes even close to this in terms of pure philosophical audacity. The Birkhoff and von Neumann scheme of merely revising propositional logic is rather tame compared with a proposal to abandon it altogether when dealing with the quantum world!

I find Mermin’s proposal to be at least an implicit endorsement of the idea, which quantum historians share with Birkhoff and von Neumann, that the conceptual problems of quantum mechanics are fundamentally logical problems, that is to say, they arise because at least some of the rules of reasoning with which we are familiar in everyday life break down, do not apply, or need to be changed in some way when we come to the quantum domain. There is a sense in which the great revolutions of modern science consist in calling into question principles which at least since the time of the ancient Greeks have been regarded as self-evident, obviously and universally true, and showing that they are only approximations, or only apply in a limited domain. The geocentric universe, the fixity of species, the absolute nature of time, the validity of Euclid’s geometry for describing the world. Why should the rules of reasoning be an exception? My answer to Mermin’s question, “What is quantum mechanics trying to tell us?” (the title of Ref. 2), is that in the quantum world we have located the limits of validity of the propositional logic which scientists have applied so successfully in the classical world. Thus we need to formulate new rules.

The trouble with Mermin’s audacious solution to the logical problem, in my opinion, is
that it goes much too far. It is throwing out the baby with the bath water, or sawing off the branch on which you are seated, to express my dismay in hackneyed phrases that good writers like Mermin would avoid. Having gotten rid of the properties or propositions he thought were the source of the trouble, the result is a conceptual structure that lacks significance. How does one attach any meaning to a probability without a referent, to $\text{Pr}(A)$ when $A$ is meaningless? How does one distinguish it from the numerically-different probability $\text{Pr}(B)$ of the equally meaningless $B$?

Another approach to understanding Mermin, which seems initially more plausible, is that $A$ has a symbolic or mathematical meaning, so that $\text{Pr}(A)$ is the “probability associated with symbol $A$”, and hence can take on a different value from the probability $\text{Pr}(B)$ associated with symbol $B$, even though $A$ and $B$ have nothing to do with the real world. Theoretical physicists sometimes use abstract symbols whose physical contents is not clear — think of Feynman diagrams in field theory — but which can be manipulated according to well-defined rules and used to calculate things, such as cross sections, which have a closer connection with the real world. Perhaps for Mermin the nonexistent referents of his probabilities play this role, as on the right side of (2). But then what am I to make of the left side? Can $\text{Pr}(E)$ be checked experimentally, and if so how? Normally probabilities are checked by repeating an experiment many times and looking at the ensemble of outcomes. But Mermin rejects ensembles, see Sec. III of Ref. 2, as a way of interpreting his probabilities. At the end of the day I have to acknowledge that I am unable to make sense of his objective probabilities, and I fear that Mermin may be in the same situation; some of his remarks in Secs. II and III of Ref. 2 point in that direction.

A second problem with Mermin’s proposal, or maybe it is a different aspect of the problem just discussed, is its macroscopic or classical or “correspondence” limit. I think it leads, almost inevitably, into a quagmire called “consciousness”, and for this reason does not satisfy his desideratum 1 as I interpret it. Before giving reasons, I should say a few words about my own position on the proper relationship of quantum physics and consciousness research. I think the two need to be firmly disentangled. This could be done by producing a correspondence limit for quantum theory which makes sense of the entities which chemists and biologists are constantly talking about — molecules, proteins, cells, what have you — in quantum terms, one which explains why such talk is reasonable and shows why what our colleagues believe to be true is (at least in a suitable approximation) true from a quantum perspective. We should be able to say to the neurophysiologists, “Your way of talking about things, while not the language of quantum mechanics, is a quite adequate approximation to what results from a fully quantum-mechanical description of the sort we physicists know how, in principle, to construct, which is extremely awkward and ill-suited to your purposes. You really don’t need it, because your talk about axons, synapses, contact potentials, and the like makes good physical sense, and is in agreement with our understanding of the quantum nature of the world. Stop worrying about Penrose. Instead, go solve your problems in the way that seems sensible to you. You have our blessing.”

Alas, the Ithaca interpretation cannot pronounce such a blessing, because “correlations without correlata” has the wrong correspondence limit. There is nothing that allows it to distinguish big macroscopic systems with enormous Hilbert spaces from microscopic spin
systems. The same sorts of contradictions can arise for the former as for the latter, and thus, in the Ithaca interpretation, the probabilities of macroscopic properties make sense, but the properties themselves do not exist. This immediately raises the question of why our colleagues (not to mention we ourselves) believe in the existence of axons and chairs and the like, while supposing that probabilities are abstract, unphysical things — exactly opposite to the real state of affairs according to the Ithaca interpretation. In this manner the problem of consciousness, in the form of the mistaken beliefs of a large part of the scientific community, not to mention the rest of the human race, is immediately dragged onto center stage in a fashion which I find bizarre.

Mermin devotes a significant amount of space in Ref. 2 to the problem of consciousness: it comes up in Secs. II and X, and all of IV is devoted to this topic. He does not claim to understand it, and considers it a significantly more difficult problem than quantum interpretation. I shall not try to either summarize or dispute his ideas, as this would require a rather lengthy essay on a topic for which I possess little expertise. What he writes does not indicate that he has managed to cleanly separate consciousness theory and quantum theory. Instead his strategy, as I understand it, is to plunge ahead in his effort to understand the quantum mechanical description of the nonconscious world while leaving inconsistencies between consciousness and his doctrine of the nonexistence of correlata to be solved at some later date. To be sure, his is a new theory, and if no one could propose a new theory without first solving all the outstanding problems, nothing would get published. Theoretical physicists are in the habit of issuing promissory notes against the day when loose ends will be properly tied down. If I am hesitant to accept such a note, it is not because I doubt Mermin’s honesty. On the contrary, it is because I believe him when he says that consciousness is a much more difficult problem than quantum mechanics. Which is why the latter needs to be cleanly separated from the former if we are to make progress, and it seems to me that the Ithaca interpretation does not accomplish this.

But can quantum historians do any better? The answer is a qualified “Yes”. First, and this needs no qualification, the ontological ordering of properties and probabilities in the histories approach is the same as in classical physics. Properties correspond to subspaces of the Hilbert space, the counterparts of subsets of a classical phase space, and this is how classical physics describes real physical properties. Probabilities of properties represent partial information about the system in cases in which more information is at least potentially available (more on this in Sec. 4.2). Thus the Ithaca interpretation’s insistence on the unreality of things most people consider real is absent from the histories approach, along with the accompanying temptation to start discussing consciousness.

Second, and this does need qualification, the histories approach has an appropriate correspondence limit. How the classical world emerges from quantum theory has been studied by Gell-Mann and Hartle, Brun, and Omnès. While I myself have not participated in this work, I have tried to summarize the main ideas in Ch. 26 of Ref. 12. Here is a quick summary of that summary. Classical physics emerges when you use a quasi-classical framework for quantum theory. It employs appropriate projectors, or families of projectors, on (typically rather large) subspaces chosen so that they represent the properties of “macroscopic” (i.e., in comparison with elementary particles) objects used in everyday discourse, including
scientific discourse when it is not focused on the specifically quantum aspects of matter. A quasi-classical framework is an authentic quantum mechanical description of the world, no more and no less than the unitary or the dragon or the practical frameworks of Sec. 3.2. A key point is that a single quasi-classical framework suffices for answering all “classical” questions about the world. (Actually there are many different, mutually incompatible quasi-classical frameworks which yield descriptions which are indistinguishable at the macroscopic level; for present purposes we can simply think of using one of these.) All probabilities which arise in this framework, and thus all the probabilities needed for classical physics, can be dealt with using ordinary probability theory, without worrying about inconsistencies of the type that appear in (I).

The qualification is that actual demonstrations that classical physics emerges in this fashion are technically quite difficult, as you can see by looking at Refs. [14] and [28]. We have a general idea of how the scheme should work, along with some specific calculations to back it up. No nasty surprises have emerged thus far. But until you have searched the forest you cannot be sure there are no dangerous animals lurking there, and the emergence of classical physics from the quantum world covers a lot of territory still under exploration.

4.2 Density matrices as fundamental and objective

The second pillar of the Ithaca interpretation is that the density matrix is a fundamental objective property, and provides a complete description of a single quantum system. Mermin regards this as a consequence of desideratum 5, Einstein locality: if \( \mathcal{A} \) and \( \mathcal{B} \) are two systems far enough apart so that they are not interacting, nothing done to \( \mathcal{B} \) should have any effect on \( \mathcal{A} \). It is helpful to consider a specific case: two spin-half particles \( a \) and \( b \) in an entangled spin-singlet state \( |\psi\rangle \). The reader probably knows how this is usually discussed: a measurement of \( S_{bz} \) for particle \( b \) that yields a value of \(+1/2\) results in the wave function somehow collapsing into a state \( |z_a^+\rangle \), \( S_{az} = -1/2 \), for particle \( a \), while the measurement outcome \( S_{bz} = -1/2 \) will produce \( |z_a^-\rangle \). On the other hand a measurement of \( S_{bx} \) will result in a collapse with \( S_{az} = -S_{bx} \), and so forth. Taken literally, this description seems to imply long-range nonlocal influences that alter (or produce) the state of particle \( a \) depending upon the choice and the outcome of the measurement on particle \( b \). Einstein did not like such nonlocal influences, and Mermin and I share his distaste. But what to do about them?

One thing you can do is calculate the reduced density matrix using a partial trace over particle \( b \),

\[
\rho_a = \text{Tr}_b(|\psi\rangle\langle\psi|),
\]

where \([\psi]\) is short for \( |\psi\rangle\langle\psi| \). It is then easy to show that if nothing is perturbing particle \( a \) — no magnetic field, no measurement — the choice of which measurement is made on particle \( b \) has no effect on \( \rho_a \). Hence if \( \rho_a \) tells us everything that can possibly be said about the internal properties of particle \( a \), these cannot be changed by what is done to particle \( b \), and Einstein locality as defined in desideratum 5 will be satisfied. By an internal property I mean something like the value of \( S_{az} \), while Mermin means the corresponding probability distribution. On the other hand, the correlation between particles \( a \) and \( b \) that tells one that \( S_{az} = -S_{bz} \) is not an internal property and cannot be deduced from \( \rho_a \).
Arguments supporting the idea that its reduced density matrix provides a complete description of the internal properties of a subsystem are given Sec. 3 of Ref. 1, and are the central focus of Ref. 3. The discussion is based on the formula

$$\rho = \sum_j p_j |\psi_j\rangle$$

expressing a density matrix $\rho$ as a sum over terms corresponding to an “ensemble” $\{p_j, |\psi_j\rangle\}$, where $\{p_j\}$ is a collection of probabilities, and $\{|\psi_j\rangle\}$ a collection of normalized, in general nonorthogonal, pure states. If $\rho$ is not a pure state there are (infinitely) many inequivalent ensembles that can be used to represent it in the form (7), and if these ensembles can in some sense be regarded as physically distinct, then the density matrix by itself is not telling us all there is to know. In particular, believers in nonlocal influences will associate an ensemble $\{|z^+_a\rangle, |z^-_a\rangle\}$, equal probabilities, with the reduced density matrix $\rho_a$ when $a$ and $b$ are in a spin singlet state and $S_{b_z}$ is measured on particle $b$. That is, either $S_{a_z} = +1/2$ or $S_{a_z} = -1/2$ with the same probability, and we cannot say which since we are only considering particle $a$ in isolation, not as something correlated with the outcome of the $S_{b_z}$ measurement. On the other hand, if $S_{a_z}$ is measured, the resulting ensemble is $\{|x^+_a\rangle, |x^-_a\rangle\}$. In this perspective these ensembles represent distinct states of affairs, despite the fact that no measurement carried out on $a$ can reveal the difference. (For this reason nonlocal influences cannot be used to transmit information, a point which has received significant attention in the published literature. See, for example, Sec. 4.6 of Ref. 23.) Mermin, on the other hand, does not believe that there are any objective difference between the two ensembles in this and other analogous cases, and his discussion of the topic in Ref. 3 combines the wit and wisdom for which he is justly famous. I see no point in trying to repeat or summarize it in my own dull words. Instead, I call an intermission during which the reader who has not yet done so can take time to read and enjoy the dialogue between Yvonne and Zygmund!

My own assessment of Mermin’s argument is that while it represents a good first step in defending Einstein locality, one can do a lot better, but in order to do a lot better one needs to abandon the idea that the density matrix of a subsystem is an objective and irreducible description, and instead treat it as a quantum analog of a classical probability distribution, something you use when the information available to you is less than what is, in principle, potentially available. I shall (of course) be using histories to discuss the situation, and for this purpose it is helpful to allow for a time dependent $|\psi(t)\rangle$ obtained by integrating Schrödinger’s equation starting with an initial entangled state $|\psi(0)\rangle$ at $t = 0$. The time dependence comes from a Hamiltonian in which there is no interaction between the two particles, but one or both of them could be in a magnetic field. For most of the discussion I will assume zero magnetic field, so that $|\psi(t)\rangle$ is the same as the initial $|\psi(0)\rangle$.

The probability that $S_{a_z} = +1/2$ at time $t$ can be written as

$$\text{Pr}(S_{a_z} = +1/2) = \langle \psi(t) | (|z^+_a\rangle \otimes I) |\psi(t)\rangle = \text{Tr}_a (\rho_a(t)[z^+_a])$$

where $\rho_a(t)$ is defined by the time-dependent version of (6). The event algebra on which this probability is defined must include $[z^+_a]$ and its negation $[z^-_a]$, and the coarsest (smallest)
sample space for which this is true consists of these two projectors. This sample space does not contain $[\psi(t)]$, and it is inconceivable that it could somehow be extended to include $[\psi(t)]$, because $[\psi(t)]$ does not commute with $[z^+_a]$ or $[z^-_a]$: the projector onto any entangled state of two spin-half particles does not commute with that for any nontrivial property of either spin. Hence it is meaningless to think of the two-spin system as somehow being in the entangled state $|\psi(t)\rangle$ when at that $t$ one uses $|\psi(t)\rangle$ to compute the probability (8).

The careless habit of physicists in thinking that one can ascribe properties to separate spins while believing that the combination is in an entangled state is, from the perspective of the quantum historian, just that — a careless habit, which can and does get people into trouble.

But if $|\psi(t)\rangle$ is not a physical property, how can it appear in (8)? In this context it functions not as a physical property or quantum beable, but as a mathematical device to assist calculation, a pre-probability in the notation of Sec. 9.4 of Ref. 12, to which I refer the reader for a detailed discussion. If $|\psi(t)\rangle$ in (8) is merely a device for calculation and not a physical property, the same must be true of $\rho_a(t)$. (Note that $\rho_a(t)$ is not really needed, since anything one can calculate from it can be obtained just as well from $|\psi(t)\rangle$, and one can use $|\psi(t)\rangle$ to compute other things, such as the correlation between the two particles, which can’t be obtained from $\rho_a(t)$.) Thus $\rho_a(t)$ is a pre-probability and cannot be more real than a probability, so in the histories approach it is not a beable. Note once again how real vs. imaginary gets reversed when one goes from Ithaca to histories.

Unlike Mermin, the historian claims that a more refined, more precise, more informative description of particle $a$ is available, at least in principle, than that provided by $\rho_a$. For the spin singlet state, $\rho_a$ assigns probabilities of $1/2$ to each of the two possible values of $S_{az}$. This is very little information, and corresponds to not knowing the outcome of a coin toss. How can we do better? By measuring $S_{az}$ using a Stern-Gerlach apparatus, and seeing whether its value is $+1/2$ or $-1/2$. Alas, you say, the measurement will perturb the particle and the outcome will not tell us whether $S_{az}$ was $+1/2$ or $-1/2$ before the measurement. You will indeed learn nothing if you insist on using the dragon framework, Sec. 3.2, that you learned from your textbook. Instead use an appropriate framework, the same thing experimental physicists do when they design real apparatus to measure real properties. They are not fools!

Having reduced Mermin’s irreducible description, can we still defend Einstein locality? Indeed we can, with a much more robust and compelling argument than Mermin’s. For one can show — see Ch. 23 of Ref. 12 for the detailed justification — that if at time $t_1$ particle $a$ has, for example $S_{az} = +1/2$, and at $t_2 > t_1$ some spin component (it doesn’t matter which one) of particle $b$ is measured, then at any time $t_3 > t_1$, including $t_3 > t_2$, it is still the case that $S_{az} = +1/2$. (This assumes that particle $a$ is not in a magnetic field.) You can replace $S_{az} = +1/2$ with $S_{aw} = +1/2$, where $w$ is any direction in space, and result is the same: the distant measurement leaves it unchanged. In brief, the historian can carry out an explicit calculation that demonstrates Einstein locality. And as a corollary provides a very simple proof that nonlocal influences cannot transport information: they don’t exist!

This argument is not available to Mermin, who thinks $S_{az} = +1/2$ is meaningless. However, a modified form can be constructed for someone who only believes in correlations. The fact that nothing happens to $S_{az}$ when $b$ is measured at $t_2$ can be expressed using joint
probabilities

\[
\begin{align*}
\Pr([z^+_a], t_3; [z^-_a], t_1) &= 0, \\
\Pr([z^-_a], t_3; [z^+_a], t_1) &= 0.
\end{align*}
\]  

(9)

which are easily calculated using histories, see Ch. 23 of Ref. 12 for the general strategy. I myself find it a bit clearer to write

\[
\begin{align*}
\Pr([z^+_a], t_3 | [z^+_a], t_1) &= 1, \\
\Pr([z^-_a], t_3 | [z^-_a], t_1) &= 1,
\end{align*}
\]  

(10)

but (9) can be used by someone suspicious of conditionals.

However, there is no way to derive either (9) or (10) from \( |\psi(t)\rangle \) or from the reduced density matrix \( \rho_a(t) \). This seems to have escaped people who suppose that the “wave function of the universe”, thought of as a function of time satisfying Schrödinger’s equation, somehow provides a complete description of the time dependence of a quantum system. One way to see why this is not so is to use the classical analogy of a Brownian particle that starts at the origin at \( t = 0 \). The usual simplified theory leads to a probability distribution density

\[
\rho(r, t) = (4\pi Dt)^{-3/2} \exp[-r^2/4Dt],
\]  

(11)

for the Brownian particle to be located at a point \( r \) at time \( t \), where \( r \) is the magnitude of \( r \) and \( D \) is the corresponding diffusion constant. This \( \rho(r, t) \) provides a useful probabilistic description, something that can be checked against experiments. But even on a probabilistic level there is more that can be said. Suppose that at time \( t_1 = 5 \) seconds the particle is at \( r_1 \). Then at \( t = 5 + \Delta t \) seconds the probability of its location conditional on this information will not be given by (11), but by a different expression involving \( r - r_1 \) and \( \Delta t \). The point is that you cannot use the single time-dependent expression (11) to describe what is going on, for it lacks information about correlations of positions at different times. Clearly (11) does not describe the motion of an individual Brownian particle. It is more appropriately thought of as describing an ensemble: a collection of many Brownian particles, or the results of repeating an experiment on one Brownian particle many times.

In the same way, \( \rho_a(t) \) in our quantum example is missing the sort of information needed to derive (9) or (10). That \( S_{ax} \) has a probability of 1/2 of being + at both \( t_1 \) and \( t_3 \) is consistent with its being the same at both times, (9), but also with its having reversed its sign at some point during the intervening time interval, say at \( t_2 \) when the measurement was carried out on particle \( b \). There is nothing wrong with \( \rho_a(t) \), but it simply is not an appropriate tool for calculating correlations of properties at different times. The same comments apply to \( |\psi(t)\rangle \). Quantum histories, on the other hand, are an appropriate tool, and for this reason satisfy Mermin’s desideratum 3, which says we should be able to describe the behavior of individual systems even in cases in which the theory cannot provide deterministic predictions. The Ithaca interpretation, at least as long as it relies on \( \rho_a(t) \) or \( |\psi(t)\rangle \), cannot obtain (9), and is limited to what I think is best thought of, using the analogy between \( \rho_a(t) \) and \( \rho(r, t) \), an ensemble rather than a single system description.
5 Reality and Its Representations

Mermin has a brief discussion of quantum histories in Sec. XI of Ref. 2. In a footnote he remarks that the term incompatible as used in the histories approach has something in common with Bohr’s complementary, and he thinks it would be better if the latter were used in place of the former. My response is that while there is a relationship between the concepts, there is also a risk of confusion, a point to which I shall return below. Mermin goes on to say that the histories approach liberates Bohr’s complementarity from the context of mutually exclusive experimental arrangements, and then adds:

The price that one pays for this liberation is that the paradoxical quality of complementarity is stripped of the protective covering furnished by Bohr’s talk of mutually exclusive experimental arrangements, and laid bare as a vision of a single reality about which one can reason in a variety of mutually exclusive ways, provided one takes care not to mix them up. Reality is, as it were, replaced by a set of complementary representations, each including a subset of the correlations and their accompanying correlata. In the consistent histories interpretation it is rather as if the representations have physical reality but the representata do not.

The last sentence is at first glance rather perplexing, as the historian’s motto of “no probabilities without a sample space” translates into “no correlations without correlata,” which would seem to correspond to “no representations without representata,” the exact opposite of what Mermin asserts. I am indebted to him for the following clarification: “representations” are frameworks in histories terminology, and the denial of physical reality to the representata was intended to refer to the impossibility of combining sample spaces of separate incompatible frameworks into a single sample space. Even with this clarification I find myself in almost complete disagreement with Mermin’s statement, and since the issues involved are rather central to a correct interpretation of the histories approach, I shall attempt to briefly state my own position on the relationship of frameworks to the physical reality they in some sense represent.

To begin with, only a very naive realism would assert that the mathematical constructs used in theoretical physics are identical with the physical world which many of us believe is “out there,” independent of our attempts to describe it, and to a great extent immune to our efforts to change it. Instead, our theories are, at best, abstract mathematical representations of that reality. Electrons do not solve Schrödinger’s equation, protons do not live in Hilbert space, and even in the pre-quantum era planets did not move on orbits in phase space. The relationship of our mathematical and conceptual structures to the world “out there” is a subtle and difficult philosophical problem. My attitude, perhaps better my faith, is that reality is something like our best scientific models of the world. In particular, a Hilbert space of wave packets and spins, while it is only an abstract model, reflects reality better than does a classical phase space.

The phase space is not so much wrong as outdated, and it served very well as a representation of physical reality into the early 20th century. Since a point in phase space comes as close as possible to representing or describing a classical system as we think it “really
is,” it seems sensible to assign the same task to a ray in Hilbert space, the closest quantum analog of a point in phase space. The ray is, in this sense, a mathematical representation of what I believe, but cannot prove, is the reality “out there” in an objective world. This connection between the quantum Hilbert space and the real world, which seems to me central to the physical interpretation of quantum histories, is what is being denied by the Ithaca interpretation’s “correlations without correlata.”

Let us return to the problem which seems behind Mermin’s denial that the histories approach provides a consistent representation of reality: the fact that incompatible sample spaces cannot be combined. Part of the difficulty may be that “incompatible” used in this way is a technical quantum term referring to something with no good classical analog. In particular, it means something very different from “mutually exclusive” as applied to distinct elements of a classical (or quantum) sample space. Various critics have confused these two terms and thereby seriously misunderstood what the histories approach is all about, and this is one reason I do not think it is a good idea to replace “incompatible” with Bohr’s “complementary”, at least insofar as the latter is associated (Bohr was hardly a model of clarity) with mutually exclusive pieces of apparatus. The following comments will, I hope, help clarify the difference; lengthier discussions will be found elsewhere (Ref. [11] and Ch. 27 of Ref. [12]).

For a spin-half particle, \( S_x \) and \( S_z \) are incompatible observables, and the projectors \([z^+]\) and \([x^+]\) represent incompatible properties, since \([z^+]\langle x^+\rangle\) is not zero and not equal to \([x^+]\langle z^+\rangle\). In order to measure \( S_x \) for a particle traveling along the \( y \) axis one needs to rotate the Stern-Gerlach apparatus through 90° from the orientation needed to measure \( S_z \), and these two are mutually exclusive experimental arrangements, as are any two macroscopically-distinct states of affairs when viewed quantum mechanically: the product of the projectors is zero. Note that \([z^+]\) and \([x^+]\) do not and cannot belong to a single quantum framework, whereas the two orientations of the apparatus can and do belong to a single quasi-classical framework. For this reason, measurements, while they can supply useful insights, are not a good, or at least an adequate way to try and understand quantum incompatibility. Instead, one has to build up one’s intuition on the basis of microscopic quantum examples analyzed in a consistent way that is not dependent upon a notion of macroscopic measurement. To this end I recommend toy models of the sort used extensively in Ref. [12].

Even though there is no really good classical analog of quantum incompatibility — which is why it is so hard to get it straight — judicious use of classical models, such as the following, can sometimes help. Imagine a spinning object with angular momentum \((L_x, L_y, L_z)\), and suppose that for some reason I describe it by giving the value of \( L_z \) and not that of the other two components. Such an impoverished description constitutes the \( L_z \) framework, and \( L_x \) and \( L_y \) frameworks are defined in the same way. Using the \( L_z \) framework doesn’t mean one cannot use the \( L_x \) framework, and whether or not some \( L_z \) description is true has no bearing on whether an \( L_x \) description is true. Clearly no “law of nature” tells one that one of these frameworks is “correct” and the others are “incorrect.” Instead, if it is the \( z \) component of angular momentum that interests me, I will need to use the \( L_z \) framework, because in the \( L_x \) and \( L_y \) frameworks \( L_z \) is (obviously) not defined. Also I can, if I wish, use the \( L_z \) framework up to some point in time, and then switch over to the \( L_x \) framework for later times without
altering the angular momentum of the body; all I have done is switch my attention from one aspect to another.

Now consider a quantum spin-half particle with its $S_x$, $S_y$, and $S_z$ frameworks. Every statement in the preceding paragraph is still correct if $L$ is replaced by $S$. Go back and read it over again with this in mind. The difference between classical and quantum is that you cannot combine two incompatible quantum descriptions, so specifying values of both $S_x$ and $S_z$ for the same particle at the same time is meaningless, whereas the classical descriptions are always compatible and therefore always combinable. This is why the discussion of classical angular momentum in the previous paragraph seems so unnatural, even though it is correct: in classical physics one never has any reason to talk about $L_z$ while not talking about $L_x$ and $L_y$. The situation in quantum physics is different, but it is nonetheless misleading to think of the $S_x$ and $S_z$ descriptions as mutually exclusive, as if one being true implies that the other is false. Which framework is to be used depends on the sort of description the theoretical physicist wants to construct. He is of course perfectly free to construct all sorts of incompatible descriptions and contemplate them in any way he finds helpful in better understanding the quantum world. However, he will be badly mistaken if he thinks he can combine two of them into a single description that somehow corresponds to the reality of what is going on in his colleague’s laboratory. This may be something of what Bohr had in mind when he spoke of “complementarity,” and I would like to believe that it is. But if so, anchoring the concept firmly in the mathematical structure of Hilbert space rather than tying it to possible arrangements of macroscopic apparatus is a necessary step in producing a tool of real utility to the quantum physicist, something that can go beyond the usual arm-waving appeal to the uncertainty principle and be used to disentangle something like Hardy’s paradox.

A particularly important framework for understanding the histories approach to quantum reality is the quasi-classical framework mentioned earlier, the one appropriate for describing the everyday world. As already noted in Sec. 4.1, this framework provides what is in principle (it would, obviously, be hideous to use it in practice) a genuine quantum-mechanical description of how things develop in time, using histories based on projectors representing actual properties of the world. The dynamics is stochastic, and only one of the histories to which quantum mechanics assigns probabilities will correspond to what actually happens. But what actually happens actually happens, and in this respect the histories approach provides a robust realism in marked contrast to the Ithaca interpretation. What about MQS states, Schrödinger’s cat? There are certainly frameworks which contain such things, and no law (in particular, no law of nature) prevents physicists from using them. Think of such a framework as describing the “$S_x$” aspect of the world in a situation in which the quasi-classical framework describes the “$S_z$” aspect. These descriptions are not mutually exclusive in the sense that if one is correct the other must be false. Think again of classical $L_z$ and $L_x$. But you cannot combine them, for attempting to do so produces something which doesn’t live in the Hilbert space.

Returning to the topic of physical reality as viewed quantum mechanically, it seems to me rather natural to allow it to include everything that can be described by a quasi-classical framework, with the obvious proviso appropriate for a stochastic theory: only one of the
histories actually occurs. This way everything that is part of reality in macroscopic classical physics has a quantum counterpart. Including other quantum frameworks on an equal footing as possible or potential descriptions of physical reality is then a natural idea, and there seems to be nothing wrong with it so long as one keeps in mind that quantum reality understood in this way is such that its diverse incompatible aspects cannot be represented by a single picture of the world in the manner we are accustomed to in classical physics. To be sure, one may also take the position that unless the frameworks (representations) satisfy classical combination rules, what they represent cannot count as part of genuine physical reality. If this is Mermin’s position, I disagree with him, for I believe that the most interesting developments of modern science are precisely those that have caused significant changes in our view of reality, and quantum mechanics belongs on that list.

6 Conclusion

The Ithaca interpretation and the histories approach share a common vision of what constitutes a good interpretation of quantum mechanics. As indicated in Sec. 1, I agree wholeheartedly with the first five of Mermin’s six desiderata, and for reasons which I have alluded to from time to time in this paper, I believe the histories approach, as summarized in Sec. 3.1 satisfies all five. On the other hand, it seems to me that the Ithaca interpretation is deficient in two respects.

The first and most serious problem is connected with the first desideratum: The theory should describe an objective reality independent of observers and their knowledge. If words are to have their usual meaning, “reality” must somehow be connected with what most people, including most quantum physicists, think constitutes the real world, with its chairs and photomultiplier tubes and neurons and the like. And most of us think that the actual outcome of a coin flip — it came up tails on this particular occasion — is more real than the probability of 1/2 assigned to it before the experiment took place. But Mermin claims, if I have not misunderstood him, that in the quantum world probabilities are real and their referents are not; my difficulties with this are explained in Sec. 4.1.

To be sure, the quantum world may be sufficiently different from the world of everyday experience that words cannot have their usual meaning, and I think this must to some extent be the case. All of the great revolutions of modern science have required some revision of the meaning of words. Think of how our notion of “time” was altered by relativity theory. It would be disappointing if quantum mechanics, which is surely much more revolutionary than relativity, did not require similar changes. In Sec. 2.1 I explain why the use of Hilbert space rather than phase space mathematics in quantum theory raises crucial issues about how we reason about the world, and in Secs. 2.2 and 3 some proposals for dealing with this. I can hardly criticize innovation when the histories approach that I advocate requires a major revision of “classical” ways of thinking when dealing with the quantum world.

However, quantum logic, quantum histories, and Copenhagen or textbook quantum mechanics (at least when viewed from a histories perspective) have in common the fact that in the correspondence limit of macroscopic systems their vision of reality corresponds, pretty
much, to what we believe about the ordinary world. The Ithaca interpretation does not. This does not by itself mean that it is wrong: remember that Galileo’s opponents thought it obvious that the earth was at rest, whereas we have come to accept the fact that it is, instead, in rapid motion. But it does mean that a lot more work needs to be done to make sense out of the Ithaca interpretation, and if a theory of consciousness is needed in order to explain why the commonsense view of reality is wrong, I am rather pessimistic about the project. At least we need to explore the possibility that quantum mysteries can be satisfactorily solved in a less radical manner.

The other deficiency I see in the Ithaca interpretation has to do with desideratum 3: The theory should describe individual systems — not just ensembles. Here the issues are more subtle. The histories approach is capable, Sec. 4.2, of providing a more detailed description of the properties of one of the two particles prepared in an entangled state than is possible using a reduced density matrix, contrary to Mermin’s claim that the latter is objective and irreducible. To use an analogy, the historian is willing to say that the outcome of this particular coin toss was $T$, whereas Mermin can only assign equal probabilities to $H$ and $T$. I myself have no problem with assigning a probability to a particular event, such as whether or not it will snow in Ithaca on December 25 of 2010. But it seems to me that such an assignment does not really describe the event, and by analogy the reduced density matrix does not describe an individual system, even though it may supply us with some useful information. And to the extent that a reduced density matrix $\rho_a(t)$ is the quantum analog of the Brownian particle probability distribution in (11), it seems appropriate to think of it as describing an ensemble rather than an individual system.

I would agree that the Ithaca interpretation satisfies the other desiderata: it describes small systems, measurements do not play a fundamental role, and it preserves Einstein locality. In the case of Einstein locality (Sec. 4.2) it seems to me that the histories approach achieves sharper results, but at least Mermin and I agree on the absence of those mysterious nonlocal influences.

The Ithaca interpretation is much younger than the histories approach, and Mermin is quite frank in acknowledging that his notion of objective probability is tentative, and that there are a number of other unresolved problems and loose ends; see in particular Sec. XII of Ref. 4. These will need to be tied down a bit better before one can say whether or not this approach is really effective in taming quantum mysteries. In the meantime the histories interpretation shows that the vision represented by his first five desiderata is not just wishful thinking on Mermin’s part. There is already one consistent interpretation of quantum mechanics that fits the bill, and it will be interesting to see if this can also be done in a completely different way. It is high time to clean up the conceptual mess which has produced such headaches for physicists (I am particularly concerned about beginning students), and served as a breeding ground for all manner of crazy philosophical oddities. Even if Mermin and I do not agree on how to accomplish this, we seem to share a remarkably similar vision of what the result ought to look like when the task is complete.
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References

[1] N. D. Mermin, “The Ithaca interpretation of quantum mechanics,” *Pramana* **51**, 549 (1998).

[2] N. D. Mermin, “What is quantum mechanics trying to tell us?” *Am J. Phys.* **66**, 753 (1998).

[3] N. D. Mermin, “What do these correlations know about reality? Nonlocality and the absurd,” *Found. Phys.* **29**, 571 (1999).

[4] J. von Neumann, *Mathematische Grundlagen der Quantenmechanik* (Springer-Verlag, Berlin, 1932), English translation: *Mathematical Foundations of Quantum Mechanics* (Princeton University Press, Princeton, 1955).

[5] G. Birkhoff and J. von Neumann, “The logic of quantum mechanics,” *Annals of Math.* **37**, 823 (1936).

[6] R. Omnès, “Consistent interpretations of quantum mechanics,” *Rev. Mod. Phys.* **64**, 339 (1992).

[7] R. Omnès, *The Interpretation of Quantum Mechanics* (Princeton University Press, Princeton, New Jersey, 1994).

[8] R. Omnès, *Understanding Quantum Mechanics* (Princeton University Press, Princeton, New Jersey, 1999).

[9] R. B. Griffiths, “Consistent histories and the interpretation of quantum mechanics,” *J. Stat. Phys.* **36**, 219 (1984).

[10] R. B. Griffiths, “Consistent Histories and Quantum Reasoning,” *Phys. Rev. A* **54**, 2759 (1996).

[11] R. B. Griffiths, “Choice of consistent family, and quantum incompatibility,” *Phys. Rev. A* **57**, 1604 (1998).

[12] R. B. Griffiths, *Consistent Quantum Theory* (Cambridge University Press, Cambridge, U.K., 2002).
[13] M. Gell-Mann and J. B. Hartle, “Quantum Mechanics in the Light of Quantum Cosmology,” in Complexity, Entropy and the Physics of Information., W. H. Zurek, ed. (Addison-Wesley, 1990), pp. 425–458.

[14] M. Gell-Mann and J. B. Hartle, “Classical equations for quantum systems,” Phys. Rev. D 47, 3345 (1993).

[15] M. Jammer, The Philosophy of Quantum Mechanics (Wiley, New York, 1974).

[16] W. Feller, An Introduction to Probability Theory and Its Applications, vol. I (John Wiley & Sons, New York, 1968), third ed.

[17] A. Einstein, B. Podolsky, and N. Rosen, “Can quantum-mechanical description of physical reality be considered complete?” Phys. Rev. 47, 777 (1935).

[18] E. Schrödinger, “Die gegenwärtige Situation in der Quantenmechanik,” Naturwissenschaften 23, 807 (1935), for an English translation, see [19].

[19] J. A. Wheeler and W. H. Zurek, eds., Quantum Theory and Measurement (Princeton University Press, Princeton, N. J., 1983).

[20] J. A. Wheeler, “On recognizing “law without law”,” Am. J. Phys. 51, 398 (1983).

[21] J. S. Bell, “Against measurement,” in Sixty-Two Years of Uncertainty, A. I. Miller, ed. (Plenum Press, New York, 1990), pp. 17–31.

[22] K. Gottfried, Quantum Mechanics (W. A. Benjamin, New York, 1966).

[23] R. B. Griffiths, “Consistent resolution of some relativistic quantum paradoxes,” (2002), quant-ph/0207013.

[24] L. Hardy, “Quantum mechanics, local realistic theories and Lorentz-invariant realistic theories,” Phys. Rev. Lett. 68, 2981 (1992).

[25] N. D. Mermin, “Quantum mysteries refined,” Am. J. Phys. 62, 880 (1994).

[26] T. A. Brun, “Quasiclassical equations of motion for nonlinear Brownian systems,” Phys. Rev. D 47, 3383 (1993).

[27] T. A. Brun, Applications of the Decoherence Formalism, Ph.D. thesis, California Institute of Technology (1994).

[28] R. Omnès, “Quantum-classical correspondence using projection operators,” J. Math. Phys. 38, 697 (1997).

[29] M. Redhead, Incompleteness, Nonlocality, and Realism (Oxford University Press, Oxford, 1987).

[30] N. D. Mermin, private communication.