The origins of time asymmetry

Mark Tapani Pallas

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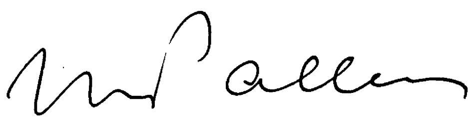
Declaration

This is to certify that, unless otherwise indicated, this thesis is entirely my own work. It is the result of research carried out by me while a candidate for the degree of Doctor of Philosophy at the Australian National University.



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Abstract

This thesis is about the role played by time in quantum mechanics. It investigates a perennial problem in physics and philosophy of science, known as 'the problem of the direction of time'. We experience time as going only one way in the world, from past to future. This natural temporal order is rather remarkable in view of the time-symmetry of the underlying laws of physics. Many attempts have been made to derive this 'arrow' of time from these underlying laws. All claims to have done so have foundered, owing to the difficulty of obtaining an asymmetric conclusion from symmetric premises.

Recently, the philosopher Huw Price has advocated a new approach to thinking about time's puzzles. He argues that we ought to do so from an 'Archimedean' vantage point that is 'outside' time to avoid being misled by the temporal asymmetries of our own natures and habits of thinking. This is a familiar story in the history of science. The Copernican and Darwinian revolutions were shifts in perspective to a less geocentric and anthropocentric view. However, Price claims that such a shift has not yet been achieved with respect to time. Price claims that the most promising approach to the interpretation of quantum mechanics remains poorly appreciated, owing to the fact that the nature and significance of our anthropocentric causal intuitions have not been properly understood. That approach involves taking seriously the notion that the future can affect the past.

This thesis investigates the question of whether quantum mechanics gives reason for thinking that the future can affect the past – and so for doubting the intuition of a one-way direction of time.

The methodology is to concentrate on the interpretative problem of quantum mechanics. The thesis spells out the nature of that problem, and shows how backward causation (advanced action) can be used to resolve it. In doing so, four main claims are argued:

(1) The standard interpretation of quantum mechanics is philosophically unsatis-

factory.

(2) Price's local advanced action strategy provides a natural heuristic for tackling the main counterintuitive aspects of quantum mechanics, namely its quantization of energy, complementarity, non-locality, and stochasticity.

(3) Contra Price, the collapse of the wave function does not render the standard interpretation of quantum mechanics time-asymmetric in a lawlike way.

(4) Price's local advanced action proposal to interpret quantum mechanics is misleading in one important respect. (The necessary adjustment to the required local advanced action proposal is made clear.)

Additionally, a novel heuristic proposal employing advanced action for the interpretation of quantum mechanics is put forward in the last chapter.

Acknowledgments

This thesis has been many years in the making. It is now my pleasure to thank those who made it all possible. Peter Roeper, as my supervisor, painstakingly and generously read many drafts; his suggestions, questions and conversation were invaluable. From him I learned the meaning of 'argument' in a philosophical context. I am indebted to Jack Smart for his interest and encouragement, particularly in the early stages of the project, when he was still at the ANU. I am warmly grateful to Thomas Mautner for encouragement and support during my time at the ANU. I should like to thank Edwin Birks for his patience in the face of my all-too frequent expositions of my latest ideas. I'm indebted to Craig Savage for his willingness to engage with me on the more technical chapters of the thesis. I should also like to thank Bev Shallcross and Reta Gear for their administrative support, delivered with unfailing courtesy and good cheer. Finally, I should like to thank two people whose faith in me has never wavered. The first is Karen van den Broek; she has helped me in more ways than I can count. The second is my mother, to whom I dedicate this thesis.

Dedicated to my mother.

Ah Mr. Gibbon, another damned, fat, square book.
Always scribble, scribble, scribble, eh?

(the Duke of Gloucester on being presented with vol. 2
of The Decline and Fall of the Roman Empire)

Contents

Introduction

Another curiosity which strikes us is the divorce in physics between time and time's arrow.

(A.S. Eddington, 1928)

The past is different from the future. The temporal asymmetry of the world sits uneasily with the symmetry of the relevant underlying laws of physics, which for practical purposes exhibit no preference for either direction of time. This is known in the physical literature as 'the problem of the direction of time'. It is the subject of this thesis. How does the temporal asymmetry arise? What is its status? Is the asymmetry lawlike, or merely de facto? Can the asymmetry perhaps be ascribed to our own human temporal perspective as has recently been argued? If so, what might the world look like from a sufficiently detached atemporal viewpoint? Would such a viewpoint entail the existence of not only forward-directed but also backward-directed time (advanced action)? If so, in what proportions, and what might be evidence for the latter? What would the existence of backward-directed time *mean*? Is it perhaps tantamount to the existence of backward causation? How would backward causation fit into present physical theory? And if it exists, why is it that we never seem to 'see' any, instead finding both causation and time always forward directed and in perfect lockstep? These questions also constitute part of the subject matter of 'the problem of the direction of time'.

These are big questions, and to make progress we shall have to narrow our focus. We shall not be concerned with all aspects of the problem of the direction of time. For instance, we shall not specifically concern ourselves with the stock philosophical debate about the status of the past-present-future distinction, or the 'flow' of time.[1] I shall simply assume the basic tenets of the 'block universe'

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[1] In the philosophical literature the expression 'the problem of the direction of time' has a wider scope, including the objectivity or otherwise of the past-present-future distinction and the status of the 'flow' of time. Does the temporal mode of our perception of the world have ultimate significance, or is time no more than a sequence of events ordered with reference to elements of itself by relations such as 'earlier than', 'later than' and 'simultaneous with'? The issues range from free will, fatalism and predestination to tense logic and the nature of truth. The philosophical problem can be traced back, by way of St. Augustine and Aristotle, to Heraclitus of Ephesus and Parmenides of Elea. In this thesis I shall be concerned solely with the physical problem of the direction of time.

view, namely that there is nothing special about the present, the relation between the past, the present and the future being perspectival, like that between 'here' and 'there', and that there is no objective 'flow' of time. Such a position is not uncontentious, Eddington for example writing, 'If anyone holds this view I cannot answer him by argument; I can only cast aspersions on his character.'

Nor shall we concern ourselves with the question of whether 'time itself' is asymmetric, as some suggest, as I'm not quite sure what the question means in spite of ongoing attempts at clarification. Instead, we shall confine our attention to the asymmetry of things *in* time, i.e. the asymmetric way in which we find them along the time axis. We shall resist being drawn into the philosophical 'realism'/'antirealism' debate despite its indirect bearing on our question. Finally, and perhaps surprisingly, we shall not specifically concern ourselves with cosmology, even though we are keen to track down the origin of time's arrow – which is usually said to arise from how things started off in the first place in the initial singularity.

The main reason for avoiding cosmology is lack of space. There is another field of enquiry on which I want to concentrate, closely connected with the direction of time problem. The other reason is that cosmology is a highly speculative science, with paradigm closely following paradigm and the values of measurable quantities being revised by orders of magnitude on a regular basis. If the history of the subject is any guide, cosmology is the art of going wrong not only with confidence but heroically. Moreover, the level of certitude ascribed to current ideas (such as inflation) seems to be proportional to their *ad hoc*ness, tending to prevent the voicing of dissenting views.[2] Although I shall engage in considerable speculation myself in what follows, it will be speculation in a field about which a great deal is known, which at least necessitates cautious tip-toeing through the minefield of known fact, and being careful not to lay down an epicycle in the wrong place.

The field on which I want to concentrate is quantum mechanics. The plan is to tackle the problem of the direction of time by a flank attack, by focusing on the interpretational problem of quantum mechanics. Why does quantum mechanics need interpretation, and what is the connection of the interpretative problem of quantum mechanics with the direction of time? Taking the first part of the question first, quantum mechanics needs interpretation because of the philosophical unsatisfactoriness of the standard interpretation. That is not an especially contentious view, the physicist and Nobel-laureate Murray Gell-Mann for example characterizing quantum mechanics as 'not a theory, but rather a framework within which we believe any correct theory must fit'.[3] Even though the interpretation of quantum mechanics is the vehicle for investigating the prob-

[2] Examples abound; for some stories see e.g. Hawkins 1997, and Hoyle 1994.

[3] Gell-Mann 1981, p. 170.

lem of the direction of time, it is only a vehicle, and I shall not give a comprehensive historical survey of quantum mechanics, or an exegesis of its various interpretations, even those involving advanced action. A great many interpretations exist, of subtlety and complexity, and much has been written on them, and it is entirely beyond my powers to do them justice. I shall take a different tack, and go right back to the basics of quantum mechanics and try to zero in on those features of it that have given rise to the interpretative problem in the first place, and which must be taken into account in any proposed interpretation. I shall then try to see if the nature of the quantum mechanical interpretative problem provides a clue for making progress with the other problem – that of the direction of time. In particular, does it give reason for doubting the intuition of a one-way direction of time? Might it be that the natural explanation of those aspects of quantum mechanics giving rise to the interpretative problem is that time is two-directional? If so, that would have ramifications for the more general problem of the direction of time. This is the answer to the second part of the question.

In regard to the connection between quantum mechanics and the direction of time, I refer to the project of the Sydney/Edinburgh philosopher Huw Price. Price is best known for his questioning in the pages of *Nature* of Stephen Hawking's claim to have derived the arrow of time from time-symmetric laws. The fact that the explanation of time's arrow remains obscure strongly suggests to Price that we're missing something fundamental in our thinking about time. To latch on to whatever it is that we're missing, we need to subject ourselves to a *shift in perspective* – to look at old things in a new way. That's a familiar idea in the history of science. The Copernican and Darwinian revolutions are dramatic examples of perspectival shifts, as is the theory of relativity. Quantum mechanics is a perspectival shift in progress. Price, too, advocates a certain kind of shift in perspective. Even though we are creatures *within* time, Price believes that we ought to think about time's puzzles from an 'Archimedean' vantage point that is 'outside' time to avoid being misled by the temporal asymmetries of our own natures and habits of thinking. He notes that we have a tendency to project the idiosyncrasies of our own makeup onto the world, with the consequence that our view of the world has often ended up looking pretty idiosyncratic in hindsight, embodying the peculiarities of our own anthropocentric standpoint.

In spite of the shift to a less geocentric and anthropocentric view following the above conceptual revolutions, Price argues that when it comes to time, we still haven't managed the transition. We remain unable to distinguish between how the world *is*, and how it *appears* to be from our particular standpoint. Consequently, our present view of the temporal structure of the world remains constrained and distorted by the contingencies of the standpoint.

Take cosmology. In cosmology all roads in the search for an explanation of temporal asymmetry in the universe lead to the single question: Why was the universe in a very special low entropy state early in its history? In trying to answer this question, Price argues, cosmologists have failed to adopt a sufficiently detached atemporal viewpoint, helping themselves instead to the soft options of double standards and special pleading. Consequently, they've failed to realize how hard it is to show that the universe must be in the required low-entropy state at *one* end of time near the big bang without also showing that it must be in that same state at the *other* end near the big crunch. But if low entropy at one end entails low entropy at the other, the consequent decreasing entropy of a recollapsing universe would result in a reversal of the direction of time as the universe begins to recollapse. Cosmologists who rule out the latter case often do so on the same grounds that would also rule out the low entropy beginning. Unsurprisingly, this biased procedure has led in the work of these scientists to time-asymmetric conclusions. But without an independent justification of this bias, the work tells us nothing about the asymmetry of the world, but only about the asymmetry of the assumption of the investigators.

Price claims that the atemporal viewpoint that he advocates also has important ramifications for that most puzzling of the puzzles of modern physics – the meaning of quantum theory. In his opinion, the most promising understanding of quantum theory has been almost entirely overlooked because physicists and philosophers have failed to notice how thoroughly our ordinary view of the world is a product of our asymmetric viewpoint.

The counterintuitive nature of quantum mechanics can be explained, according to Price, by supposing that the fate of a quantum system may depend on not only its past but also on its future. The atemporal perspective enables us to see that the counterintuitive 'paradoxes' of quantum mechanics (e.g. Schrödinger's cat, EPR & Bell's theorem) vanish once it's realized that on the quantum level, there is backward causation in the world. What is more, we can avail ourselves of locality and realism (and even free will) notwithstanding special relativity's prohibition of faster than light signalling – provided that we're willing to accept that all of the weirdness of quantum mechanics is best interpreted as showing nothing more than the fact that the future can – and does – affect the past. An important consequence of taking this view is that the traditional problem of the temporal asymmetry of the wave function is rendered unproblematic since the wave function is then seen to be just an incomplete description; it is an incomplete description because, as it turns out, it doesn't incorporate future causes, which can be just as important as past causes.

A large part of the attraction of Price's approach is that it also goes some way toward dissolving the tension between the time-symmetry of the laws of physics and the time-*a*symmetry of the world of our experience. The asymmetry can be understood as being in part *anthropocentric* in origin – a projection of our own temporal asymmetry. It arises from our failure to adopt a sufficiently detached standpoint when considering the matter. For example, in the context of quantum mechanics, Price suggests that had the *real* lessons of the nineteenth century debate about the temporal asymmetry been appreciated by the turn of that century, then quantum mechanics might have been simply the kind of theory of microphysics that the twentieth century might well have expected, and the standard interpretation might have never been invented.[4]

To understand what Price has in mind when he speaks of the 'real lessons of the nineteenth century debate', we need to take a closer look at the traditional entropic problem of the direction of time. Those lessons are crucial to understanding our problem. Here are the details.

One of the most striking things about the world is its ceaseless change: the world is made anew from one moment to the next. The concept of change presupposes the concept of time, and vice versa, the one being dependent on the other for its meaning. Although change can occur in any order, processes in the world have a natural temporal order, which is rather remarkable. Many metaphors have been coined to convey the peculiar one-directedness of such processes, including 'time's arrow', 'time's flow', and the 'river of time'. The river of time flows only one way. Whether we have crossed the Rubicon, shorted a thousand T-Bond futures, or settled on the Bombe Napoléon Flambée for dessert, we must wear the consequences. *Alea iacta est.*

Just as the past is fixed, the future may be fixed, too. In classical physics, to specify the positions and velocities of the members of a closed system at any instant is to determine their history for all time, both in the past and the future. Relativity theory agrees. For an observer in one state of motion, the Andromedan space fleet to invade the Earth is already on its way, whereas for an observer in another state of motion relative to the first (e.g. walking past the first in the street), not even the decision to invade has yet been taken. But in that case, how can there still be uncertainty as to the outcome of that decision? 'If to *either* person the decision has already been made, then surely there cannot be any uncertainty. The launching of the space fleet is an inevitability.'[5] So also for every other event. One is reminded of the finger of fate in Edward Fitzgerald's well-known English rendering of the Rubáiyat of Omar Kháyyam:

> The Moving Finger writes; and, having writ,
> Moves on: nor all your Piety nor Wit
> Shall lure it back to cancel half a Line,
> Nor all your Tears wash out a Word of it.

Another striking thing about the world is its relative permanence. Although the

[4] That is not to suggest that Price thinks that the asymmetry of the second law of thermodynamics is anthropocentric. He does not.
[5] Penrose 1989, pp. 303-4.

Rubicon is gone, Rome itself still stands. And despite the constant change in the prices displayed on the speculator's computer monitor screen, there is the comforting permanence of the monitor itself, the elegant office, the concrete high-rise, the teeming city, the ubiquitous pollution... And Bombe Napoléon Flambée is *sempiternally* an unwise choice of dessert.

To try to make sense of the permanence of things, theorists rely on the concept of laws of nature. The idea is that given that a system 'starts off' in some particular state, laws determine how it changes over time. (It's not a case of 'anything goes'.) The concept of law not only goes a considerable way toward accounting for the permanence of the world (given that there is a world to start off with), but it can be used to predict its future states.

Owing to the complexity of physical systems – a problem early on for example in a gambling context – the concept of law was extended to included statistical laws. Of such, the second law of thermodynamics is the best known.[6] This law has an especially secure place in the canon, Eddington for example remarking that it holds the 'supreme position among the laws of Nature'.

> If someone points out to you that your pet theory of the universe is in disagreement with Maxwell's equations – then so much the worse for Maxwell's equations. If it is found to be contradicted by observation – well, these experimentalists do bungle things sometimes. But if your theory is found to be against the second law of thermodynamics I can give you no hope; there is nothing for it but to collapse in deepest humiliation.[7]

The second law states, very roughly, that if a closed system is left to itself, its entropy or 'disorder' tends to increase over time. The increase in entropy is equivalent to loss of information. Quite generally, entropy is a measure of the amount of information we have discarded, or never possessed, when we view a system from the 'outside'. A deck of cards provides an example. Start with the deck in a highly organized state according to some external ordering schema. Shuffle well. The order is broken up in spite of the symmetry of the underlying classical laws. We no longer know where each card is in the deck. Moreover, the inverse of the original shuffling process, as one continues shuffling the deck, is extremely unlikely to occur within human timeframes. Analogous considerations apply to many or most things in our experience.

Owing to the constant increase in entropy over time, the second law is used as a physical signpost for the direction of time. The original idea was that we can tell by physical measurement of the entropy whether t_1 is before or after t_2. That

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[6] The second law may be stated as follows: $dS = dQ/T$. The meaning of the equation is this: If a small quantity of heat dQ per unit mass is supplied to any substance at a temperature T (on the absolute scale), the corresponding increase of entropy dS is dQ/T.
[7] Eddington 1928, p. 74.

is because the quantity dS/dt in the second law is always positive and increasing. The greater the entropy, the later the time. (Hence, too, Clausius's eventual 'heat death' of the universe.) Another suggestive similarity between time and entropy is that the direction of time, like that of entropy, is a macrophysical or 'global' property. It emerges when we look at a system from the outside. Draw a branching world line on a space-time map. We are unable to tell from the world line itself, in spite of its branching, whether its direction is forward or backward in time without reference to something outside the world line itself.[8] (What that something *is*, is a matter of ongoing debate.)

The obvious next step was to try to *derive* the entropic time asymmetry of macroscopic systems from the kinetic theory of motions of their microscopic components – to derive, in effect, a time-asymmetric conclusion from time-symmetric underlying classical laws. Boltzmann was the first to claim to have done so with his *H*-theorem in 1872. The theorem described the effects of collisions on the distribution of velocities of molecules of a gas. It seemed to show that a gas which started off in any non-equilibrium velocity distribution must monotonically approach thermodynamic equilibrium. A crucial assumption of Boltzmann's derivation was the molecular chaos assumption – that the velocities of colliding molecules are independent of one other.[9] The initial derivation soon came unstuck owing to the reversibility objection of Loschsmidt,[10] and the later recurrence objection of Zermelo and Poincaré. In hindsight that is small surprise, as Boltzmann was trying to do the statistical-mechanical equivalent of 'squaring the circle', namely derive an asymmetric result from symmetric premises. This is what Price had in mind when he mentioned (see p. 5) the 'real lessons of the nineteenth century debate' about the temporal asymmetry.

Boltzmann's response was to move to a probabilistic version of the *H*-theorem. In this version, there was no asymmetry. The entropy increase applies equally in both temporal directions. Nonetheless, if we were to examine systems over vast time periods, they would nearly always be found in states that are close to equilibrium – which is the natural state of matter. The time-symmetry

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8 In this respect, the time-ordering of the world line is in principle like the ordering of the deck of cards in the above example. Just as no amount of scrutiny of the markings on the cards alone can tell you whether the deck is highly ordered or not, so also no amount of scrutiny of the world line alone will tell its temporal direction. In both cases, we need to make reference to information outside of the system itself, namely a coding schema.

9 Although the mechanics of the *H*-theorem are time-symmetric, its assumptions are not. These assumptions are: that motions are random (the micro-independence assumption), that we introduce averages, and that we use probabilities to calculate only in one direction, namely the direction of the 'future'.

10 Loschsmidt's objection was that, owing to the time-reversal invariance of the underlying dynamical laws governing the evolution of the system, for each thermodynamic evolution in which entropy increases there must be another possible one in which it decreases.

was a crucial insight. But it was obtained at a cost. That cost is that it makes it very puzzling why entropy is not even higher in the past than it is in the present. The problem remains with us today. Boltzmann himself succumbed to its complications and committed suicide.

Boltzmann's *H*-theorem makes it obvious that there is nothing at all remarkable about the fact that entropy of a system goes up in the future if the system presently happens to be in a special highly organized state. In that case it's simply natural for the present order to 'unwind', as it were. The problem is that reversibility and the statistical treatment of entropy imply that entropy is overwhelmingly likely to have been *even higher* in the past. But that is contradicted by our experience. We find that entropy in fact *decreases* more and more the further we look into the past. This is remarkable. To see how remarkable, take a closed system such as a box containing gas which happens to be in a special state at time t. Suppose that most of the molecules of the gas have quite by chance congregated into one corner of the box. In that case, the gas is in a statistically highly unlikely low-entropy state. Statistical mechanics tells us that if we were to look at the system at some time later than t, the chances would be overwhelming that the gas molecules would have readjusted and spread themselves about the interior of the box more uniformly through random collisions with the walls of the box and each other, i.e. the entropy of the gas would have gone up. Similarly, had we looked at the system at a time *earlier* than t, statistical mechanics again tells us that the chances are overwhelming that we would have found the gas molecules distributed much more uniformly throughout the interior of the box – and thus in a higher entropy state – than they were at time t.[11]

The usual response to the problem is to point to one-off cosmological boundary conditions. The universe began in an extremely special initial state. That state was one of such low entropy that it is overwhelmingly likely that entropy will increase, and keep on increasing until the universe reaches thermodynamic equilibrium. This has the desired outcome of explaining away the apparent falsification of the prediction of the second law as regards the past, and enabling the observed phenomena to be consistent after all with the time-reversal invariance of the second law – such invariance simply being masked by the boundary conditions.

Such a solution of the conundrum leaves something to be desired. Callender, for example, remarks that it is as if we were suddenly to find potatoes everywhere looking like Richard Nixon and have this 'explained' by reference to the initial conditions of the big bang, which, we are told, simply made this happen.[12] Clearly such an explanation would be just shifting the explanatory burden from one place to another.

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11 After Penrose 1989, p. 316.
12 Callender 1997, p. S227.

Another problem with the usual explanation is that *gravitational* entropy is not well-understood, and thus the concept of entropy is not well-understood cosmologically. Which brings us to the point that the entropic or thermodynamic arrow of time is not the only such arrow. There are three other principal arrows to be found in the physics literature – the radiative arrow (both electromagnetic and gravitational), the cosmological arrow, and the quantum-mechanical arrow, each of which picks out a temporal asymmetry in the world. The radiative arrow refers to the fact that although there is plenty of retarded radiation in the world, there seems to be no advanced radiation. The cosmological arrow refers to the fact that the universe is expanding and not contracting. The quantum-mechanical arrow refers to the fact that the collapse of the wave function renders quantum-mechanical systems irreversible. Additionally, there is the psychological arrow – which describes our inner awareness of time's order, even its supposed 'flow'. A large part of the main 'problem of the direction of time' seems to be to describe the origin of *all* the 'arrows', and their interrelation. Such has not been achieved, and despite the considerable progress that has been made, the connection between the time-symmetric laws of physics and the time-asymmetry in the world is not well-understood.

The persistence of disagreement and conceptual confusion after centuries of pondering the difference between the past and the future by philosophers and scientists of the greatest stature suggests that Price is right when he says that we are missing something fundamental in our picture of time. Others have thought so too, and a wide net is cast to capture this missing ingredient. Some investigators look to time-asymmetric law for the answers, while others are content to manipulate conventional thermodynamics, as in statistical theories of an entropically founded arrow of time (the hypothesis of 'branch systems'). Many believe that not only the asymmetry between the past and the future but also the apparent 'flow' of time has its origin in the quantum wave function collapse. Some seek an explanation in the operation of the brain itself. Others retire behind metaphysical theories of time such as presentism. Still others, though convinced of the temporal symmetry of the world, remain puzzled by our powerful subjective impression to the contrary. The philosopher Jack Smart writes that though he is sure that the notion of our advance through time, or the flow of time ('as if bearing us down a river to the great waterfall which is our death') is an illusion, he is not sure how the illusion comes about.[13] Death and decay are always there to remind us sharply that there is a sense in which the arrow is real, and has a physical basis. Time is special. As Huw Price notes, few big issues lie so close to the surface, and remain accessible to almost all educated people, and yet seem so far from being answered.

In 1900 Lord Kelvin delivered the following pronouncement to the British

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13 Smart 1989, p. 38.

Association for the Advancement of Science: 'There is nothing new to be discovered in physics now. All that remains is more and more precise measurement.' One might think that Lord Kelvin hadn't spent much time thinking about the problem of the direction of time. (But of course he had – in his younger days he'd been one of the greats on the subject. He had just forgotten!) In 1974, the physicist P.C.W. Davies wrote that it is extraordinary that the explanation of such a fundamental aspect of everyday experience as the difference between the past and the future should remain obscure and paradoxical after consideration by people such as Boltzmann, Einstein, Schrödinger, Eddington, and the Ehrenfests,[14] adding for good measure in the preface to his 1995 book, *About Time* that he ended up more confused about time after writing the book than he had been before. The philosopher John Earman has argued that the main problem with 'the problem of the direction of time' is to figure out what the problem *is*, or is supposed to be. As Hume said in another context, '... even the rabble without doors may judge from the noise and clamour, which they hear, that all goes not well within...'

'What, then, is time?' asked St Augustine in 400 A.D. 'If no one asks me, I know; if I wish to explain to him who asks, I know not.' Although much has changed since St Augustine's day, his remark would be appropriate even if made today.

Well, that is the problem of the direction of time, as passed on to us from Boltzmann. Price's proposed solution seems at first sight beguilingly simple. The asymmetry is in us rather than in the world. It is a projection from the kind of perspective we have as temporally asymmetric agents in the world. It arises from our failure to adopt a sufficiently detached 'Archimedean' standpoint when considering the matter. In this sense the proposed solution is conventional. But Price's proposed solution is more sophisticated than that. The asymmetry is not *just* conventional. Our *de facto* temporal orientation as agents *requires* us to choose the convention that we do. Thus, there is also an objective element to Price's solution. Even so, its conventional element allows for the possibility that we may be living in a Gold universe after all, in which the low-entropy big bang is followed by an equally low-entropy big crunch. At least, there is as little (or as much) reason to rule out the one as there is to rule out the other. Likewise, the objective asymmetry introduced into microphysics by the collapse of the wave function in the standard interpretation turns out to be nothing more than a construct of the interpretation – another illusion created by our failure to adopt the appropriate atemporal stance.

An important part of the project of the present thesis is to provide a critical assessment of Price's advanced action proposal, with especial regard to the

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14 Davies 1974, p. 1.

interpretation of quantum mechanics. Why the emphasis on Price's advanced action proposal? After all, Price is not the only investigator to have proposed using advanced action. Other have preceded him, with more highly developed models than anything Price has come up with. The reason is that, in addition to the philosophical sophistication of Price's advanced action proposal, there is a feature of it that makes it stand out from most of the others. In Price's proposal, the advanced action is always local, in the sense that it is transmitted by the particles themselves within light cones. There are no faster-than-light influences. This is a crucial advantage.

We now turn to particulars. This thesis will argue four main claims:

(1) The standard interpretation of quantum mechanics is philosophically unsatisfactory.

(2) Price's local advanced action strategy provides a natural heuristic for tackling the main counterintuitive aspects of quantum mechanics, namely its quantization of energy, complementarity, nonlocality, and stochasticity.

(3) Contra Price, the collapse of the wave function does not render the standard interpretation of quantum mechanics time-asymmetric in a lawlike way.

(4) Price's local advanced action proposal to interpret quantum mechanics is misleading in one important respect. (The necessary adjustment to the required local advanced action proposal is spelled out.)

Additionally, a heuristic proposal of a speculative nature employing advanced action for the interpretation of quantum mechanics is put forward in the last chapter.

These claims provide the structure of the thesis. There are six chapters. In the Chapter 1, we march up directly to the capital or centre of most that is strange and counterintuitive in physics, namely quantum mechanics. The plan is to go back to the basics of the subject and take a fresh look at the entire business. The reason is three-fold. First, doing so will help gain a clear understanding of the essential elements of the quantum-mechanical picture of the physical nature of matter, and just how this picture differs from the classical one. Second, it will enable us to pin down the exact nature of the interpretational problem of quantum mechanics, with all its elements clearly displayed. We particularly want to see if there is some feature of quantum mechanics that constitutes the essence of its interpretational problem. It turns out that there is such a feature. Third, the analysis will lay the groundwork for my later critical analysis of Price's advanced action proposal. It is the interpretational problem of quantum mechanics that the proposal is supposed to solve. The approach is philosophical rather than mathematical. The philosophical unsatisfactoriness of the standard interpretation becomes evident (the first claim).

Chapter 2 examines the role of time in quantum mechanics. We need to know if quantum mechanics is time-reversal invariant, like classical mechanics. We look at how time and time reversal enter into quantum mechanics. Important issues arise with a bearing on my later critical assessment of Price's proposal. The issues concern the question of whether the laws of quantum mechanics are time-reversal invariant. The laws in question are Schrödinger's equation and the reduction/projection postulate (collapse of the wave function). One of Price's main arguments for the advanced action interpretation of quantum mechanics is that the standard interpretation unnecessarily introduces an objective asymmetry into quantum mechanics by way of the wave function collapse. In the advanced action interpretation, on the other hand, there is no objective collapse as the wave function is not objectively real. It has the same ontological status as the classical probability function.[15] An objective collapse is an artefact of the standard interpretation. Clearly, if the collapse is not objective, the asymmetry generated by it can hardly be objective either. Thus, going to an atemporal picture results in the restoration of symmetry (subject to the Schrödinger evolution itself being symmetric).

I investigate the details of the collapse in the standard interpretation and find that, contrary to Price (and Penrose too), it does not render quantum mechanics objectively asymmetric in that interpretation (the third claim). I also briefly consider an argument for the time-reversal non-invariance of the Schrödinger equation itself, advanced by Callender. Its implications are spelled out.

In Chapter 3 we return to the problem of the interpretation of quantum mechanics by way of the Einstein-Bohr debate on the nature of quantum reality and the EPR thought experiment advanced by Einstein and colleagues against Bohr's 'Copenhagen' interpretation. The nature of the experiment and EPR's assumptions are described in some detail, as is Bohr's response. This thought experiment and the ensuing debate serves well to show the exact nature of the interpretational problem, highlighting the unexpected features which any interpretation must take into account, in this way setting things up nicely for our subsequent investigations.

The debate also lays the ground for the introduction of Bell's impossibility proof, a mathematical theorem which appeared to resolve, unambiguously and

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15 The same is true according to any hidden variable theory, as far as I know. Bell makes an interesting point in this connection, though. He points out (1987, p. 205) that even though the GRW theory contains no HV's, we can propose the GRW jumps as the basis of the 'local beables' of the theory. The jumps are centred on a particular space-time point (x,t). They are the mathematical counterparts in the theory to real events at definite places and times in the real world.

forever, the Einstein-Bohr debate in favour of Bohr. The proof is described. The 'impossibility' refers to the impossibility of the existence of local hidden variables of the kind Einstein sought. Bell's proof seemed to provide at long last the means of resolving the dispute between Einstein and Bohr. His proof appeared to vindicate Bohr by showing that local causality had to be abandoned. Our interest in Bell's proof is not just academic. It is rather in seeing how Price's advanced action proposal, with its local causality, fares vis-à-vis Bell's prohibition of local causality. The account of Bell's proof in this chapter is thus closely connected with our investigation of Price's advanced action proposal in the next chapter.

The chapter goes on to look at the current state of play in the interpretation of quantum mechanics, critically analysing six non-standard attempts at interpretation. It ends with a brief but suggestive idea on the interpretation of quantum mechanics by the cosmologist Dennis Sciama in 1958. Sciama was concerned with evading von Neumann's ban on hidden variables. However, his proposed solution is equally applicable to Bell's ban, and Sciama's scheme was a kind of an advanced visitation of Price's proposal, Sciama playing (Marlowe's) Doctor Faustus to Price's Faust.

In Chapter 4 we come to the philosophical core of Price's proposal. The core has three elements: (a) Price's analysis of the role of the independence assumption in microphysics; (b) his assessment of backward causation in terms of a weaker convention for assessing counterfactuals than the usual one; and (c) his variant of the 'agency' theory of causation. Each element questions a deep-rooted intuition arising from our temporal and anthropocentric perspective, in accordance with Price's belief that we ought to think about time's puzzles from a vantage point that is 'outside' time to avoid being misled by the temporal asymmetries of our own natures and habits of thinking. We now briefly look at each in turn.

(a) The chapter begins by focusing on the independence assumption of physics (known as the 'molecular chaos' assumption in statistical mechanics). It is a very general assumption about initial conditions. It says, in effect, that the properties of interacting systems are independent *before* they interact, though not afterwards owing to the interaction, which is why we may expect correlations between separated systems after they have interacted but not before. The independence assumption is thus explicitly time asymmetric. It needs to be clearly understood that it is an *assumption*, even though formulated as a principle. It is simply an expression of a powerful intuition arising out of our lack of experience of *macroscopic* pre-interactive correlations. However, as Price points out, there is no evidence of the lack of such correlations on the *microscale*, and we have no experiential warrant for extending the principle there. The evidence is rather *for* such correlations. Accordingly, Price proposes relaxing the independence assumption on the microscale in the direction of the future. The effect is to provide a loophole for the kind of correlations between separated systems predicted by Bell's theorem and subsequently experimentally observed. Price is quite right. It seems impossible to argue against this aspect of Price's proposal, and the only response left for those who don't like it is the Eddington one.

(b) The viability of the proposal to relax the micro-independence assumption in the direction of the future depends on being able to avoid causal paradox. Price points to a loophole first proposed by the philosopher Michael Dummett, which showed that even if later events do causally affect earlier events, paradox is avoided if it is impossible to find out, *before* the occurrence of the later cause, whether or not the claimed earlier effect has occurred. That is, to avoid paradox, we need to subscribe to the convention for assessing counterfactual dependency that says, 'hold fixed only that portion of the past which is accessible in principle', rather than the stronger mode which says, 'hold fixed the entire past'. Dummett's loophole finds a natural home in quantum mechanics, since the backward causation required to make sense of the EPR-Bell type of correlations on the advanced action hypothesis is a special case of the weaker, 'hold fixed the *accessible* past' kind of backward causation. Price shows in the case of a photon passing through a pair of rotated polarizers how evidence (or rather *lack* of evidence) of its state when in-between the polarizers is consistent with it being correlated with not only the past polarizer but also the future polarizer it is yet to encounter, consistent with backward causation playing a part in determining its state. Its state is impossible to determine even in principle. According to Price, such a view not only avoids causal paradox and restores symmetry; it also opens the way for a much more classical picture of quantum mechanics than has been thought possible. Price is entirely correct as regards the avoidance of causal paradox. As regards the restoration of symmetry claim, though, I argue in Chapter 2 that quantum mechanics is symmetric as it is. Price's third claim (that the view opens the way for a much more classical picture of quantum mechanics) I assess in Chapter 5 at greater length.

(c) Finally, there is Price's perspectival variant of the Humean conventionalist or 'agency' theory of causation. Its philosophical interest is that it manages to combine both conventionalism and objectivity in a natural and convincing way, permitting an objective content to the claim that there is not only forward but also backward causation.

The philosophical core of Price's proposal is both sophisticated and plausible. It appears to provide a breakthrough in the interpretation of quantum mechanics (claim 2).

In Chapter 5 we turn to a critical analysis of Price's proposal in the light of the quantum-mechanical formalism. I note that Price is not putting forward an advanced action model, but rather a general strategy for utilizing advanced action. I try to tease out what the proposal actually amounts to in physical terms. It turns out there are three closely related aspects of it that give rise to unease. None represents an objection against advanced action *per se*, or even specifically against Price's proposal. They are rather attempts at clarifying certain aspects of the proposal that remain so far largely unexplored in Price's writings, and highlighting potential problem areas, having regard to the future development of the proposal. The first is Price's claim that the advanced action proposal doesn't conflict with realist intuitions of Einstein's kind, even though the proposal seems to entail, in effect, that the Heisenberg indeterminacy relations remain the last word in physics as far as predictability goes. If so, nothing would seem to change in physics, and that is surely not the outcome Einstein had in mind. The second is to do with the heuristic adequacy of the proposal in certain specific respects. The proposal has concerned itself so far only with non-relativistic quantum mechanics, whereas it ought to focus specifically on relativistic quantum mechanics, and on the 'doubling up' of the solutions of the energy equation in the Dirac wave equation (and the Klein-Gordon equation). And the third raises the question of whether the proposal entails that only some causation is backward, or whether one half of all causation is so. The latter, it would appear, which is not really very surprising, seeing that quantum mechanics is missing half of the causal determinants required to make it a deterministic theory. Such a picture, in which both forward and backward causation are equally involved in every microevent, sits uneasily with Price's talk of a 'limited retrodependency', 'in exceptional cases', in the world, even landing Price in a mini-'basic dilemma' of his own (as we shall see), the two horns of which are these:

(a) He can admit that quantum mechanics should be interpreted as revealing that no quantum event can occur without both forward and backward causation being involved in an essential way; e.g. if backward causation is invoked to explain the Bell correlations, there must be backward causation involved in all quantum-mechanical processes, quite regardless of whether or not the systems concerned happen to be in singlet states; or

(b) he can admit that his advanced action strategy can't explain Bell.

The chapter concludes with a list of what may reasonably be considered to be the required elements of an advanced action theory, as distinct from a general strategy.

Chapter 6 is my own somewhat Quixotic attempt at providing a theory of 'beables',[16] to use Bell's term. It is far from being a fully worked out theory, however, and is better described as an attempt at interpreting the existing for-

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16 Those elements which might correspond to elements of reality – to things which exist – quite independently of observation.

malism of quantum mechanics using advanced action. Its attraction is in the connections it makes between very different and seemingly unconnected ideas and issues. Some of these are: quantization and wave-particle duality (which permit the characteristic self-interference and superposition of states of quantum-mechanical systems), the negative energy solutions of Dirac's relativistic wave equation, Price's advanced action proposal and perspectival view of temporal asymmetry, and the cosmological constant problem.

It answers Bell's question of what is it that 'waves' in wave mechanics.[17] It shows how advanced action explains the mysterious quantization of energy, and how Planck's constant, Bohr's complementarity and Heisenberg's indeterminacy relations fall out of advanced action in a natural way. It gives an answer, in terms of 'hidden variables', to the question of why things happen in quantum mechanics on the basis of 'insufficient cause', and to the related question of whether the wave function describes a single particle or an ensemble. It shows how matter gets its marching orders from both past and future boundary conditions, and is thereby enabled to 'know' exactly how to behave – even though its behaviour *looks* intrinsically probabilistic. It amounts to a 'hidden variable' interpretation of quantum mechanics. A consequence of the proposal is that not only the laws but also the boundary conditions of the world are symmetric.

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[17] Bell 1987, p. 187.

1

The Strange World of the Quantum

We now know that the moon is demonstrably not there when nobody looks.

(David Mermin, physicist, 1989)

The present chapter may be read on several levels. First, it is as an introduction to textbook quantum mechanics. It sketches in broad outline the historical development of the subject, and summarises the main features of the standard interpretation of quantum mechanics, including the formalism. Second, it is an introduction to the interpretative problem thrown up by quantum mechanics. Quantum mechanics arose as a result of a new and completely unexpected general feature of the world discovered in 1900, namely the quantization of energy. But in spite of the highly satisfactory mathematical formalism of quantum mechanics, its central conceptual feature – the quantization – remains ill-understood, and is the root cause of the interpretative problem of quantum mechanics. That problem concerns the nature of reality, in particular the real nature of an unmeasured quantum object. The problem is introduced in this chapter by briefly describing and contrasting two famous and diametrically opposed historical positions on the interpretation of quantum mechanics, namely those of Bohr and Einstein. Third, the present chapter may be read as an assessment of the philosophical satisfactoriness of the standard interpretation. One of the claims of this thesis is that the standard interpretation of quantum mechanics is in fact philosophically unsatisfactory. This chapter shows in just what way it is unsatisfactory. It also poses the question, to be followed up in later chapters, of why it *remains* unsatisfactory more than seventy years after the birth of quantum mechanics. Is the way forward perhaps blocked by a concept or an assumption that needs to be discarded? If so, might such a concept be connected with the general problem of time asymmetry?

1.1 The meaning of quantum theory

There is a fable of the seven blind men of Hindustan who ran into an elephant. One felt the leg and said that it must be a tree; another felt the trunk and said it was a snake; a third felt the ear and said it was a palm-leaf, and so on. In the early decades of the twentieth century, experimenters ran into a similarly perplexing situation. One set of experiments, demonstrating the photoelectric effect, strongly suggested that light is a particle that can be localized.[1] Another set of equally good experiments, demonstrating interference effects, strongly suggested that it is a wave. Which was correct? It was decided that neither was correct. In some situations light behaved like a particle, and in others like a wave. However, it never behaved *exactly* like either. What was going on?

The problem arose from the quantization of energy. It was experimentally found that on the atomic scale, the transfer of energy was a discontinuous process, energy apparently always being transferred in jumps of a certain size, $\Delta E = h\nu$, called a *quantum*, where h is Planck's constant.[2] The quantum is an indivisible unit of energy.[3] David Bohm wrote in his famous 1951 textbook on quantum mechanics that 'the transfer of a quantum is one of the basic events in the universe, and cannot be described in terms of other processes'.[4] Nowadays the hypothesis of quantization is known as the *principle* of quantization.

Max Planck was the first to postulate (in 1900) that simple harmonic oscillators can possess only total energies that take on discrete values given by

$$E_n = nh\nu \qquad n = 0, 1, 2, 3, \ldots$$

where ν is a frequency expressed in cycles per unit time, and n is now known as the *quantum number* of an allowed *quantum state*. Planck's equation says that the

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1 A particle is 'an object that can always be localized within a certain minimum region, which we call its *size*'. (Bohm 1951, p. 24.) Moreover, classically, a particle possesses both a momentum and a position which are in principle determinable.

2 Planck's constant h is a constant of action. It has the dimensions of energy × time, and momentum × distance. Its magnitude is 6.626×10^{-34} kg.m^2.s^{-1}; in electron volts × seconds its magnitude is 4.2^{-15}eVs. Physicists use the term 'action' for energy (in joules) × time (in seconds) in measuring simple oscillations, or momentum × distance in most other uses. As an illustration of the latter, consider a 3-kilogram ballbearing rolling on a frictionless surface at a speed of 4 metres per second. It has a momentum of 12 kg.m.s^{-1}. In travelling a distance of 5 metres with that momentum, its *action* is 60 kg.m^{2}s^{-1}. This works out at 9×10^{34} units of h. It is evident that h is very small.

Although h signifies a quantum of action rather than of energy, energy itself is quantized because of the relation discovered by Planck, namely $E = h\nu$, where ν is frequency (expressed in cycles per second. Thus h expresses a relation between energy and frequency, functioning as a proportionality constant: $h = E/\nu$. (The relation is more commonly written as $\hbar = E / \omega$. where $\omega = 2\pi\nu$ and $\hbar = h/2\pi$, representing the quantization of angular momentum.) It was the relation between energy and frequency that was novel in Planck's discovery, not the concept of an 'action', which was well known in classical mechanics. Steven Weinberg, for example (1993, p. 110n), observes that 'Planck's constant ... provides the conversion factor between [the older systems of units of energy, such as calories or joules or kilowatt hours or electron volts] and the natural quantum-mechanical unit of energy, which is cycles per second...'.

3 Bohm 1951, p. 27.

4 Bohm 1951, p. 27.

total energy of the oscillator is proportional to the frequency of its oscillations multiplied by 0, or h, or $2h$, or $3h$, or ... nh.[5]

Planck was also the first to suggest that an oscillator can gain or lose energy only by some discrete amount which he called a *quantum*, its magnitude given by $h\nu$. For example, the electric oscillators inside atoms could only absorb or emit energy in discrete amounts $h\nu$. In other words, energy could be emitted only in little 'bursts', the total amount of energy in each burst being proportional to the frequency ν of the radiation in the burst.[6]

Einstein extended Planck's postulate (in 1905) by proposing that radiant energy is not only *absorbed* and *emitted* in discrete amounts, but also that it *exists* in space distributed in a discontinuous way, in the form of quanta. Radiation possesses a kind of 'molecular structure in energy', as he put it, which contradicts Maxwell's theory. A single quantum of radiant energy is called a *photon*. The energy of a photon is given by:

$$E = h\nu,$$

where ν is, as in Planck's law, a frequency expressed in cycles per unit time.[7] According to Einstein, this double nature of radiation is a major property of reality.

Frustrated physicists went about saying that on Mondays, Wednesdays and Fridays light is a wave, and on Tuesdays, Thursdays and Saturdays it is a particle – and on Sundays it is neither.

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5 It was later found that Planck's postulated energy quantization of the simple harmonic oscillator was in error by the additive constant $h\nu/2$, owing to its not containing the zero-point energy. (Eisberg & Resnick 1974, p. 241.)

6 Here is an example. Atoms emit light in bursts, discontinuously, and of certain characteristic energies. Each burst of light carries with it a unit of time, namely the period of its vibration. For example, the period associated with the yellow light from a sodium atom is 1.9×10^{-15} seconds. The amount of energy coming from the sodium atom during one of these bursts is found to be 3.4×10^{-19} joules. Here, then, are the two ingredients of a quantum of action: a period, and an energy. Multiply them together, and we obtain 6.5×10^{-34} joule–seconds. That is the quantity h.

The remarkable thing is that we keep on getting the same numerical result even with other atoms. Take another source of light – hydrogen, calcium, or any other atom. The period associated with the emission will be a different number of seconds, and the amount of energy will be a different number of joules; but their product in any one burst will always be the *same* number of joule–seconds. Moreover, by simply appropriately rotating our own, we can make even the numbers of joules and seconds to come out the same – showing that, even though there are many different material atoms, there is only *one* quantum of action. (This example is after Eddington 1929, pp. 183-4. The modern number for h is 6.626×10^{-34} joule-seconds [J.s].)

7 In Einstein's picture, a photon of frequency ν has exactly the energy $h\nu$; it doesn't have energies that are integral multiples of $h\nu$ (though of course there can be many photons of frequency ν, so that the total energy at that frequency can be $nh\nu$. (Eisberg & Resnick 1974, p. 37.)

Although energy comes in discrete chunks (is quantized), the chunks can come in any and all sizes, because the range of ν is a continuum. Photons, for example, have a continuous range of energies because their energies are determined by the frequencies of the continuous spectrum of electromagnetic radiation. The quantization of energy is not well understood. In 1965, Richard Feynman observed:

> We do not understand energy as a certain number of blobs. You may have heard that photons come out in blobs and that the energy of a photon is Planck's constant times the frequency. That is true, but since the frequency of light can be anything, there is no law that says that energy has to be a certain definite amount... there can be any amount of energy, at least as presently understood. So we do not understand this energy as counting something at the moment, but just as a mathematical quantity, which is an abstract and rather peculiar circumstance.[8]

Pondering on the *physical sense* of the frequency in Einstein's relation $E = h\nu$, it seemed to Prince Louis de Broglie in 1923, then a graduate student at the Sorbonne, that the results of Einstein's investigation of light quanta must apply quite generally. He was convinced that the double nature of radiation and of material corpuscles is a major property of reality and must be extended to all matter. Moreover, it seemed to him that the material corpuscle is the seat of some kind of a *periodic internal process*, of frequency $\nu = E/h$ (from Einstein's relation).

This led to the next major development. In his 1924 PhD thesis, de Broglie proposed that the motion of a microscopic body is governed by the propagation of an associated guiding wave. In this conception, the mysterious frequency in Einstein's relation didn't directly relate to the (hypothetical) internal periodic behaviour of the body, but instead to the frequency of the guiding wave. However, the wave, which always accompanied the body, had to be in phase with the internal periodic behaviour of the body. De Broglie showed that if at the beginning the periodic behaviour of the moving body is in phase with the wave, this 'harmony of phase will always persist' – which suggested to him that 'any moving body may be accompanied by a wave and that it is impossible to disjoin motion of body and propagation of wave'.[9]

De Broglie proposed that the wave properties of matter are related to its corpuscular properties in the same quantitative way that Einstein had shown to be the case for radiation. That is, the energy-frequency relation $E = h\nu$ and the related momentum-frequency relation $p = h\nu/c$ apply not only to radiation but also to matter, the latter in the form $p = h\nu/V = h/\lambda$, where p is the momentum of the particle, V is the speed (the phase velocity) of the matter wave and λ is its wavelength. Rearranging, we have

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[8] Feynman, Leighton & Sands 1965, I- 4, p. 7.

[9] De Broglie 1924, pp. 446-58, cited by Jammer 1966, p. 244, note 173. (This article is an English summary of de Broglie's three 1923 papers in the journal *Comptes Rendus*.)

$$\lambda = h/p$$

which is known as the de Broglie relation. It predicts the *de Broglie wavelength* λ of the matter wave that is associated with the motion of a material particle having a momentum p.[10] De Broglie's relation connects in a single equation two apparently incompatible states of being for matter, namely being a *wave* (having the wavelength λ) and being a *particle* (having the momentum p).[11] The corresponding equation for the *frequency* of the matter wave is

$$\nu = E/h.$$

from $E = h\nu$. The frequency is proportional to the object's relativistic mass, the relation between the frequency and mass being given by $m = h\nu/c^2$ (where m is the relativistic mass). Rearranging, $\nu = mc^2/h$; i.e. $\nu = E/h$. (The relation $\nu = E/h$ is more commonly written, $\omega = E/\hbar$, where $\omega = \nu \times 2\pi$ and $\hbar = h/2\pi$, representing the quantization of angular momentum.) The expression for the frequency may also be written as $\nu = pV/h$, from $p = h\nu/V$ (where V is the phase velocity of the matter wave, this latter always being the reciprocal of the associated particle's velocity multiplied by the square of the speed of light c, i.e. $V = c^2/v$, or $Vv = c^2$). It would seem that matter always comes in quanta of mass $h\nu/c^2$, or equivalently, quanta of energy $mc^2 = E = h\nu$.

It is evident from the above formulae that the momentum and wavelength of a particle/wave are not independent quantities in de Broglie's proposal, but that an inverse or reciprocal relation exists between them, such that the product of the two quantities is always the constant h. (The fact that h is a constant means that the product of those two quantities [momentum and wavelength] is always the same for all observers, no matter what their state of motion relative to the particle/wave or to each other.)[12] However, a peculiarity of de Broglie waves is that the wave magnitudes such as the wavelength, wave number and phase velocity of the waves, which depend on the momentum p of the particle, are defined only at the place where this momentum has a meaning at the instant considered, i.e. at

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10 After Eisberg & Resnick 1974, p. 63.

11 When the wavelength λ was evaluated for an electron of typical momentum p, it was found to be of the order of atomic dimensions. (Bohm 1957, p. 76.)

12 Relativistically, and with $c = 1$, the relation between the two is $\frac{m_0 v}{\sqrt{1-v}}\lambda = \kappa$ (where $\kappa = h/c$). In other words, the product of the particle's relativistic mass and its velocity (as a fraction of the speed of light), multiplied by its wavelength is 2.21×10^{-42} kilogram metres. In the case of photons, which always travel at c, the relation between wavelength and momentum is even simpler. We have $\lambda = \frac{h}{mc} = \frac{\kappa}{m}$. Rearranging, $\lambda m = \kappa$. Selecting more natural units by setting $\kappa = 1$ gives simply $\lambda m = 1$. In other words, we have $\lambda = 1/m$, and $m = 1/\lambda$. The corresponding relation for frequency is $\nu = m$.

the location of the particle [as presumably revealed by a measurement]. Elsewhere in space, these magnitudes, and so the wave itself, are left undetermined.[13]

De Broglie's matter waves were discovered experimentally in 1927, with just the (inferred) properties predicted by him. Now matter, too, had been shown to exhibit an analogous duality to that exhibited by light.

In 1925 Erwin Schrödinger, building on the foundations laid down by Planck, Einstein and de Broglie, provided a quantitative theory of matter waves, applying to both light and matter.[14] In the equation named after him, he specified the laws of wave motion which the particles of any microscopic system obey. According to his theory, the evolution and behaviour of de Broglie's matter waves is described by an unobservable mathematical object called a *wave function*. A wave function is a mathematical expression giving a complete description of the motion and other kinematic properties of a wave. The quantum wave function provides the probability distribution of the values of all the various quantities that can be measured. Whenever physicists work out a problem in quantum mechanics, e.g. calculate the probabilities of the future behaviour of a quantum system, it is to the wave function that they must turn. Schrödinger's equation gives the form of the wave function when the potential energy corresponding to the forces acting on the associated particle is specified.

We shall look at Schrödinger's equation in more detail in §1.2. The important point for the present purposes is that the equation presupposes that the system of interest behaves like a wave.

Now, the fact that a material particle such as an electron displays both wave and particle behaviour is not in itself especially mysterious. After all, classical water waves are constituted of the collective behaviour of large numbers of particles. What *is* mysterious is how these properties coexist in the case of quantum objects. A quantum object never behaves *exactly* like a particle or *exactly* like a wave, but rather 'it acts exactly as a member of an ensemble whose dynamics is given by the quantum theoretical formulation'.[15]

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13 D'Abro 1951, p. 609.

14 Although de Broglie had postulated that a microscopic particles of matter is always associated with the propagation of a guiding wave, his proposal lacked generality in two important ways: (a) it did not explain how the guiding wave propagates, or governs the motion of the particle, and (b) although de Broglie's postulate successfully predicted the wavelengths of matter waves inferred from measurements of the diffraction patterns obtained by passing particles of matter through diffraction gratings, it did so only in cases where the wavelength remained essentially constant. But the wavelength does not remain constant when the particle is acted upon by a force, as is typical (Eisberg & Resnick 1974, p. 138). In such cases, functions more complicated than simple sinusoidal ones are needed to describe the associated wave. Thus de Broglie's idea needed to be generalized. The generalization was achieved by Schrödinger.

15 Zimmerman 1966, p. 497.

Our classical intuitions say that if two descriptions are mutually exclusive, one of them (if not both) must be wrong. In the quantum case it seems instead, at least if we are to believe the usual account, that it is our classical intuitions that are wrong. It is a remarkable fact[16] that J.J. Thomson was awarded the Nobel Prize in 1906 for having shown (in 1897) that the electron is a particle, and his son, G.P. Thomson was likewise awarded the Nobel Prize in 1937 (together with C.J. Davisson) for having shown that the electron is a wave (in 1927). In neither case did the Nobel committee make a mistake. (J.J. Thomson first discovered the electron, and he characterized it as a *particle* with a definite charge-to-mass ratio. His son G.P. Thomson later discovered electron diffraction, or the *wave* characteristics of an electron.) De Broglie himself received the Nobel Prize in 1929 for his prediction.

The peculiar state of affairs described above is known as *wave-particle duality*. Wave-particle duality is an instantiation of a general feature of quantum systems known as *complementarity*. Complementarity was proposed as a principle by Niels Bohr in 1927. In the case of wave-particle duality, the principle takes the following form:

> As a description of microentities and microprocesses, neither a particle description nor a wave description is fully adequate. Between them however, they form a complete, complementary description.[17]

Consider the dynamical variables of classical physics. The principle of complementarity states that there is a limitation in the number of such *independent* variables. For every classical dynamical variable (or parameter) there exists a corresponding, conjugate classical variable such that the simultaneous use of the two variables is restricted to an extent determined by the magnitude of h. The better we determine the value of one, the less we can know about the value of the other. h thus expresses the *natural limit* to the accuracy with which conjugate dynamical variables can be measured.

The two classical variables related in this way by h are also said to be *conjugate complementaries* – 'conjugate' meaning 'joined in a reciprocal relation', and 'complementary' meaning 'complementing each other'. They are also often called 'canonically conjugate variables'. Technically, the expression refers to pairs of variables such as position and momentum represented in the quantum-mechanical formalism by quantum-mechanical operators that do not commute (§1.2[b][h]). Take Heisenberg's indeterminacy relation between the momentum and the position of a particle. We may write the relation (non-relativistically) in one dimension as follows: $\Delta p_x \Delta x \approx h$. This means that the indeterminacies in the x-axis momentum and x-axis position have a product of the order of h. Rear-

[16] Pointed out by Jammer (1966, p. 254).
[17] Hughes 1989, p. 228.

ranging (and making $h = 1$ to obtain natural units), we have $\Delta p_x \approx 1/\Delta x$ and $\Delta x \approx 1/\Delta p_x$. It is evident that the more we know about one parameter (the momentum), the less we can know about the other (the position), and vice versa. Our knowledge of one parameter is in a *reciprocal, or inverse relation* to our knowledge of the other parameter. Yet we would need to know both for a complete (classical) description of the motion of a particle. This is the essence of complementarity – a *mutual exclusiveness* of descriptions (they are conjugate), coupled with a *need of both descriptions* for an exhaustive characterization in terms of classical physics of the object of interest (they are complementary). Now, in the case of the canonically conjugate variables of classical mechanics, such as momentum and position, or energy and time, one of the pair is always related to the *causal* aspect of matter and the other to the *space-time* aspect. Therefore the causal and space-time aspects are complementary.[18] Nor is the principle of complementarity restricted to dynamical variables. It applies to more general concepts. As we've seen, wave-particle duality is an example of complementarity.

Heisenberg's indeterminacy relations are mathematical expressions of the principle of complementarity, i.e. of the limitation in the number of independent classical variables.[19] For that reason the indeterminacy relations are less fundamental than the complementarity itself (as has been pointed out by many authors).[20] It also follows that the breakdown of the classical concept of determinacy entailed by the indeterminacy relations (and therefore the intrinsically probabilistic character of quantum mechanics itself in the standard interpretation) is a consequence of quantum complementarity – and the limitation in the number of independent variables expressed by that principle. However, it needs to be borne in mind that the complementarity principle itself is ultimately an interpretation of the relations expressed by the principle of quantization (and h), even though it has come to be regarded as a principle in its own right.

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18 For a discussion of the relation between the causal and space-time aspects of matter in quantum mechanics as opposing potentialities, see Bohm 1951, pp. 156-61.

19 Even if there existed no specific, written down Heisenberg uncertainty relations, the mere fact of the existence of Planck's constant would in any case serve us notice of such a reduction in the number of independent variables, as has been pointed out by Prigogine & Stengers (1985, p. 223). Consider de Broglie's relation $\lambda_B = h/p$ connecting wavelength to momentum, where λ_B is the de Broglie wavelength, h is Planck's constant and p is momentum. Planck's constant implies a relation between momenta and length (length being closely related to the concept of spatial coordinates). Therefore position and momentum can no longer be independent variables as they are in classical mechanics. The reduction in the number of independent variables (known as 'complementarity' in the Copenhagen interpretation) is a straightforward consequence of quantization – which is reflected in the quantum-mechanical formalism. In this sense, complementarity may also be said to be a straightforward consequence of the formalism, as is often asserted, e.g. by d'Espagnat (1995, p. 225). However, it is important to emphasize that quantization lies behind it all.

20 E.g. Zimmerman 1966, p. 493; Heisenberg 1967, pp. 105-6 (cited in Bub 1997, p. 190); Resnick 1972, p. 191; Englert, Scully & Walther 1994, p. 60; Whitaker 1996, p. 160.

Here is how David Bohm contrasts the principle of complementarity with the older, classical concept of a system:[21]

> This principle [of complementarity] is clearly in sharp contrast to the classical concept of a system that can be described by specifying all the relevant variables to an arbitrarily high precision. For, in the quantum theory, complementary pairs of variables are to some extent opposing potentialities, either of which can be made to develop a more precise value but only under conditions wherein the other develops a less precise value. This means, of course, that complementary variables are not actually incompatible, provided that they are not too precisely defined; it is only the *complete* precision of definition of each which is incompatible with that of the other.[22]

It needs to be emphasized, though, that the wave-like properties of the electron are *inferred* from its ability to exhibit interference-effects over wide regions of space.[23] For this reason, it may be that there is no need to place much emphasis on the wave aspect of matter. As Bohm says, that is just the simplest workable hypothesis.[24] It is rather the *interference* that is crucial, and the resulting non-classical probabilities of events. '[I]nterference occurs whenever there is more than one possible way for a given outcome to happen, and the ways are not distinguishable by any means'.[25] It is also important to realize that this quantum-mechanical interference is not like the ordinary interference of waves, because each quantum system interferes only with itself, never with another system. Moreover, unlike electromagnetic waves, matter waves are never observed, all quantum events ultimately consisting of particle-like events.[26] This last point is

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21 Bohm 1951, p. 160.

22 Cf. Bell 1987, p. 190, where Bell suggests that by 'complementarity' Bohr meant 'contradictoriness'. That does not seem to be the case, though, admittedly, Bohr took perverse delight in emphasizing how non-classical quantum reality was, and even contradictory if one insisted on using an inappropriate mode of talk.

23 Bohm 1951, p. 129.

24 Bohm 1951, p. 174.

25 Englert, Scully & Walther 1994, p. 54. For example, in the two-slit experiment photons can reach the detecting screen in two possible ways when both slits are open – through slit one and slit two. If we can determine through which slit the photons passed through, there can be no interference.

26 Though, to be sure, even the concept of 'field' when we speak of electric and magnetic fields is a theoretical construct, as are all the terms used in physics. We never observe the field itself, but only the behaviour of matter placed in the field. In the *statistical* (or ensemble) view of the intensity of an electromagnetic field, introduced by Einstein, the waves (whose strength is measured by $\overline{\varepsilon^2}$ [the average value over one cycle of the square of the electric field strength of the wave]) may be regarded as guiding waves for photons. The waves themselves have no energy, as there are only photons. In this view, the waves are a *construct* whose intensity measures the average number of photons per volume (Eisberg & Resnick 1974, p. 71). The assumption behind the statistical interpretation of electromagnetic wave intensity is that for a single photon, the wave description just doesn't apply. The wave must be interpreted as applying to an ensemble of objects.

nicely made by Herbert:

> All quantum measurements when scrutinized at their finest level of resolution consist of tiny particlelike events called 'quanta', or 'quantum jumps' – flashes of light on a phosphor screen, for instance; or a bubble, spark, or click in a particle detector; the blackening of a silver grain in a photographic emulsion; or the sudden excitation of a light-sensitive molecule in your eye. The world when looked at closely appears to be made of little dots, much like color photos in a magazine... [27]

Nonetheless, even though all quantum events ultimately consist of particle-like events, it remains true that if we want to *calculate* anything in quantum mechanics, we must first assume for the purposes of our calculation that *the system of interest is a wave*. This cannot be emphasized too strongly according to J.S. Bell.[28] Our mathematics is wholly concerned with waves. It is the mathematics of the motion of this wave that is developed in a precise way in quantum mechanics; there is no hint of particles or particle trajectories in the mathematic (hence the term 'wave mechanics').

An obvious question arises. Is there some unifying concept that can be used to make sense of the different and apparently incompatible findings, somewhat like the way the concept of the elephant unifies the findings of the seven blind men? No agreed-upon unifying concept has yet been found.

A step towards such a concept was taken by Max Born in 1926. Born proposed a *statistical* interpretation of the wave function, providing an important connection between the wave and particle descriptions of quantum systems. According to this interpretation, the intensities of the waves (squared amplitudes) determine the *probabilities* of the presence of particles, with $|\Psi|^2$ being the probability density of particles of matter. Born's interpretation, however, has by no means dispelled the interpretative problem posed by the wave function, as subsequent developments quickly showed.

The present position is this. There is no disagreement among physicists about the *use* of the wave function, e.g. to confirm/disconfirm theories about things such as the structure of the atom. While there is agreement about the theory's use, there is disagreement, however, about its *interpretation*. At issue is: what is the real nature of an unmeasured quantum entity? The issue thus concerns the nature of reality. The disputants can be broadly divided into two camps, holding opposing views. On the one hand there is the group who continue to accept the orthodox ontology, known as the *Copenhagen interpretation* (after the Dane Niels Bohr). Perhaps the majority of physicists still belong to this group, notable exceptions, however, being found among those physicists working

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[27] Herbert 1988, pp. 160-1.
[28] Bell 1987, p. 187.

in the philosophical foundations of quantum mechanics. On the other hand there have been and still are many theorists, both physicists and philosophers, who reject the orthodox ontology on each of its major tenets. Einstein is the best-known example. I shall label this group the 'Einsteinian realists'.

The central tenets of the Copenhagen interpretation are as follows:

Copenhagen interpretation

1. A quantum wave function contains a *complete description* of the quantum system associated with the wave. This is true whether the quantum system is a complex system of interacting objects or a single entity.

2. Since the description is complete, all quantum systems (or entities or objects) represented by identical wave functions are physically identical.

3. In Bohr's original version of the Copenhagen interpretation, the classical dynamic attributes of matter (such as position and momentum) cannot be extended to an unmeasured quantum system. They just do not exist until they are brought into existence by an act of measurement. The classical dynamic attributes are manifestations of the entire experimental situation, and depend in an essential way for their existence on the interaction of a system with a measuring device.[29] *Prior to* measurement, the quantum wave function expresses the entire weighted sum with complex number weighting factors (a linear superposition), of all the possible dynamic alternatives available to the system; all these alternatives are 'live' candidates (or potentialities) for the finally measured value. In what may be called the 'standard interpretation' today (a variant of the original Copenhagen interpretation[30]), the classical dynamic attributes actually exist even when unmeasured, but in an incompletely defined form. A quantum system has just those properties that would be revealed by experiment. When a quantum system is in a state of linear superposition of possible states, it may be said to exist in each of the superposed states (albeit incompletely defined), even when the states are mutually incompatible. In this view, the wave function provides a description of the objective properties of the system.

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29 'Measurement' in quantum mechanics usually refers to any process of interaction between a quantum-mechanical system and some kind of essentially classical macroscopic apparatus, in which information about the system is acquired through the interaction, and encoded in the wave function. Generally, this is quite independently of any observer. However, 'measurement' can also encompass measurements that are interaction-free, see e.g. Kwiat, Weinfurter & Zeilinger 1996, pp. 52-8. The nature of measurement is an unsolved problem in quantum mechanics.

30 But based on a different interpretative principle.

4. The difference in the measured behaviour of quantum systems represented by identical wave functions arises from an intrinsic quantum indeterminacy. Given two absolutely identical states of a quantum system ('causes'), the outcomes ('effects') can differ. The difference in outcomes is inexplicable even in principle except by reference to an inherent randomness in quantum systems.

The central tenets of the Einsteinian realist interpretation are as follows:

Einsteinian realist interpretation

1. A quantum wave function of the standard formalism does not contain a complete description of the quantum system associated with the wave.

2. Therefore quantum systems (or entities or objects) represented by identical wave functions can differ.

3. The classical dynamic attributes of matter (such as position and momentum) can be extended to an unmeasured quantum system. It is false to say that such attributes are brought into existence only by measurement, depending for their existence on an interaction between the system and measuring device. Prior to measurement a quantum system does not exist in a superposition of all the possible alternatives available to it; instead the system exists in a state possessing a single determinate description. It's just that the theorist/experimenter, mistakenly relying on the incomplete quantum formalism, is ignorant of the true state of the system (because the wave function does not completely represent it), and conflates his lack of *knowledge* of the state of the system with the state of the system itself.

4. The difference in the measured behaviour of quantum systems represented by identical wave functions arises from the fact that the quantum systems were physically different before measurement even though described by identical wave functions. The wave function fails to represent (save in the average) certain parameters or 'hidden variables' which also need to be taken into account in representing the complete state of the system – but which are omitted in the formalism. This is the explanation of the *statistical* character of quantum mechanics, i.e. why the wave function accurately predicts only the behaviour of an *ensemble* of quantum entities and not that of individual quantum entities. According to its proponents, this view dissolves, at least in principle, the difficulty faced by the Copenhagen interpretation of deciding whether the wave function describes a single quantum entity (such as an electron) or an ensemble. The difficulty is that probabilities cannot be measured directly. But as soon as we introduce statistics, we're necessarily talking of

an ensemble, because that's what statistics is made for – comparison of many similar (but not identical) cases with different outcomes. On this view, quantum mechanics remains a correct and even complete statistical theory, but not a complete description of the underlying elementary processes.

There is also a growing number of theorists who reject the orthodox ontology only on *some* of the above points. There are those who believe that the Schrödinger dynamics of the standard formalism need to be amended in some way, either to eliminate the collapse or else incorporate it within the dynamics, e.g. Bohmian Mechanics and the Ghirardi-Rimini-Weber scheme. There are also those who believe that only the interpretation of the orthodox formalism needs to be changed, e.g. the 'Many Worlds' and Modal interpretations. There is also 'the new orthodoxy', represented by the Decoherence and Histories interpretations, which sit somewhere in between. We shall look at these interpretations in §3.5.

More than seventy years after the birth of quantum mechanics there is still no consensus on how it should be interpreted. The interpretative problem is closely connected with the interpretation of Planck's constant h. The magnitude of h sets the boundaries or limits on the quantization of the world. If h were somehow to become infinitely small, quantum effects would disappear. Equally and alternatively, if c were to become infinitely large, the same result would apply. Electrons, for instance, would have neither frequency nor wavelength.

We have seen that the breakdown of the classical concept of determinacy entailed by the Heisenberg indeterminacy relations (and therefore the intrinsically probabilistic character of quantum mechanics itself in the standard interpretation) is a consequence of quantum complementarity – and the limitation in the number of independent variables expressed by that principle. Moreover, the principle of complementarity itself is simply an interpretation of the relations expressed by the principle of quantization. Evidently, the entire mystery of the subject can be sourced to the existence of quantization, described in the equations of quantum mechanics by Planck's constant. Accordingly, it seems to me that any attempt at interpreting quantum mechanics would do well to focus on understanding the quantization exhibited by nature on the subatomic scale. Why is energy quantized? Was David Bohm right when he said that the transfer of a quantum of energy is 'one of the basic events in the universe and cannot be described in terms of other processes'? What is the significance of the Einstein and de Broglie relations $E = h\nu$ and $\lambda = h/p$? More explicitly, what is the significance of the relation between energy and frequency, and wavelength and mass (or momentum), expressed by these relations? I shall call such an approach 'interpreting h', for short.

In that context, it needs to be understood that h was introduced into physics as an *ad hoc* stratagem to devise equations that work and so to make the theoretical apparatus conform with experiment (first by Planck himself to avoid the ultraviolet catastrophe). In other words, h was introduced empirically, as a measure of the quantization of energy indicated by experiment, and it has the character of *deus ex machina*. It has that character because the quantization of energy itself has that character. Nobody knew then why energy should be quantized, and nobody does today. By the same token, nobody knew then what h 'means', and nobody does today,[31] save that (a) it is the natural quantum-mechanical unit of energy (cycles per second)[32] and (b) it expresses the natural limit to which canonically conjugate variables can be measured (a peculiar 'halving' of the number of independent classical variables, see Appendix[h]). We could also say, following Sciama, that h is a 'measure' of the amount of deviation of quantum mechanics from classical mechanics.[33]

Even though nobody really understands the quantization of energy (the 'meaning' of h), there are some clues. In mechanics, actions arise from generalizing our physics to four dimensions. Action (energy × time) is the invariant four-dimensional equivalent of energy—the same for all observers even when they are in motion relative to one another and disagree about the magnitude of the energy and time components of the action.[34] In an analogous way, the relativistic *interval* of space-time is an invariant four-dimensional equivalent of distance – the same for all inertial observers even when they disagree about the measured distance and time components of the interval. And relativity of course has its own constant, c (frequency of an electromagnetic wave × its wavelength) which is the

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31 Except in the trivial sense that it is a proportionality constant.

32 Energy is defined in today's quantum mechanics as 'the change in phase (in cycles or parts of cycles) of the wave function of the system at a given *clock* time when we shift the way our clocks are set by one second' (Weinberg 1993, p. 110n). It is this way that h is the natural quantum-mechanical unit of energy, even though h is a unit of action and not of energy. Another way of seeing this is to set $h = 1$, whereupon we have $E = \nu$. Moreover, we have seen that Einstein himself spoke of radiation possessing a kind of 'molecular structure in energy' (Einstein 1949, p. 51).

33 Sciama 1958, p. 78.

34 So is momentum × distance. Heisenberg's indeterminacy relation $\Delta p_x \Delta x \geq h$ is just an alternative way of writing $\Delta E \Delta t \geq h$ (and vice versa), as is suggested by setting $c = 1$, so that energy (mc^2) = mass (m). When we do so, time (t), or better, period (τ) is a length. (The period of a light wave is the inverse of its frequency (ν), so that $\tau = 1/\nu$. The latter, in turn, is given by the wavelength of light divided by its speed, so that $\lambda/c = \lambda/1 = \lambda$.) The essential equivalence of the two relations is also evident from the following rearrangement (after Hewson 1985, p. 489) of the elements of the momentum × distance indeterminacy relation to obtain the energy × time indeterminacy relation:

Recall first that momentum = force × time, and energy = force × distance. Then write the following steps:

(1) $\Delta p_x \Delta x \geq h$.
(2) Δmomentum. Δdistance ≥ h
(3) (force × Δtime). Δdistance ≥ h
(4) (force × Δdistance). Δtime ≥ h
(5) Δenergy. Δtime ≥ h.

same for all inertial observers even when they are in motion relative to one another and disagree about the magnitude of the frequency and wavelength components of the constant. The point is, perhaps we generally shouldn't talk about energy in the quantum-mechanical realm, but rather about action, just as in the relativistic realm we generally need to talk about space-time instead of space or time separately. Additionally, it may be that electrons and other quantum-mechanical spin-half particles ought to be regarded as some kind of four dimensional objects owing to the additional degree of freedom arising from their spin.[35] Indeed, Goswami in his 1997 textbook on quantum mechanics writes: 'The electron is a four-dimensional particle! In addition to the three measurements we need to tell where the electron is [or proton, or neutron..., or atom], we need another measurement to tell which way its spin is pointing, the value of the z-component of its spin, S_z. Four measurements, four dimensions.'[36]

Quantum mechanics exists in at least four different standard mathematical formalisms: Schrödinger's wave mechanics, Heisenberg's matrices, Dirac's postulational (or algebraic) approach using complex vectors, and Feynman's path integrals (least action) approach.[37] Physicist Edward Speyer writes: 'In each of these quite different formulations, h is introduced arbitrarily, empirically. Planck's constant enters into almost every quantum calculation. Sometimes it cancels out, and remains hidden, but it is always lurking nearby.'[38] The point is, we don't really understand quantization, of which h is the expression. It seems clear that the central role of h in quantum mechanics constitutes a main part of

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35 Spin is the intrinsic angular momentum of an elementary particle or group of particles. The fundamental unit of spin angular momentum is $\frac{1}{2}\hbar$. For details, see §1.2(i).

36 Goswami 1997, p. 353.

37 Schrödinger's wave mechanics is of course just one formalism of quantum mechanics (albeit the most simple and natural one, at least according to J.S. Bell [1987, p. 187]). There are equivalent and alternative (i.e. standard) formalisms which make no reference to waves, e.g. Heisenberg's matrix mechanics and Dirac's postulational approach using complex vectors. In the relativistic formalism, there is Feynman's propagator approach. Apparently, Landé, too, has shown that quantum theory can be 'very efficiently developed without the analogy of wave motion' (Zimmerman 1966, pp. 485-6). The vector approach relies on the fact that it is possible to formulate quantum mechanics using complex vectors, and to regard wave functions as just one particular instantiation of the vector algebra. (In the matrix formulation, the vectors are row and column matrices.) What all the formalisms have in common, though, is the representation of the state of a microphysical system by a linear combination of eigenfunctions (or eigenvectors), and the presence of interference between the possible states of the system, and the calculation of probabilities from averages taken over ensembles. What is really at issue is the *interpretation* of this quantum-mechanical picture, and in particular, the self-interference exhibited by quantum-mechanical systems. The difficulty, as Bell notes in the context of EPR (1987, p. 150), isn't created by any particular *picture* of what goes on at the microscopic level, but rather 'by the predictions about correlations in the visible outputs of certain conceivable experimental set-ups'. And *these* are ultimately due to the quantization of matter on the microscopic level.

38 Speyer 1994, p. 149.

the mystery of the subject.

We shall presently go on to look at some of the difficulties of interpreting quantum mechanics. First, here is a little illustration of what an interpretation focusing on quantization (and h) might look like.

As already said, one way of looking at h is as a 'measure' of the amount of deviation of quantum mechanics from classical mechanics. (This 'measure' is both quantitative and qualitative.[39]) Now, suppose that we wanted to put forward a 'hidden variable' theory utilizing advanced action (i.e. backward causation) to account for this deviation – and the puzzles of quantum mechanics generally. The theory is such that it contains advanced action in the following way. Half the necessary boundary conditions for arbitrarily accurate predictions must refer to the past and half to the future of the moment t. This is a postulate of the theory. In such a theory, our ignorance of the future boundary conditions suffices to account for the amount of deviation of quantum mechanics from classical mechanics (of which h is a measure).[40] In that case it is clear that, whatever else h might be, it is also a measure of our ignorance of the future. By interpreting quantum mechanics along the advanced action lines of our theory, we are also interpreting h in a certain way (namely as a measure of our ignorance of the future). The converse also applies, not formally of course (in the sense of logical entailment), but heuristically. Now, if quantum mechanics is deducible from a more basic theory (such as our hypothetical advanced action theory), then presumably h will be expressed in such theory in terms of quantities fundamental to the theory.[41] What might these fundamental quantities be in the case of our hypothetical advanced action theory? Well, the quantities fundamental to the basic theory would obviously be advanced action and future boundary conditions. If these could be brought in so that they act in 'symbiosis' with retarded action (forward causation) and past boundary conditions in a way that avoids explicit nonlocality, they could play the role of Einstein's 'hidden variables', thereby explaining the deviation of quantum mechanics from classical mechanics, expressed by h. (The theory would need to be such that it makes evident the connection between energy and frequency, and wavelength and mass expressed by the Einstein and de Broglie relations $E = h\nu$ and $\lambda = h/mv$, and also how h expresses the natural limit to which canonically conjugate variables can be measured [the halving of the number of independent classical variables mentioned above], on which more later.) Such a theory is put forward in Chapter 6.

We now take a brief look at the main features of quantum mechanics.

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39 It is quantitative in that h is of a certain magnitude, namely 6.626×10^{-34} $kg.m^2.s^{-1}$. It is qualitative in that h is of certain dimensions, namely energy × time; alternatively and equivalently, momentum × distance.

40 As far as I know, this idea was first proposed by the physicist Dennis Sciama (1958, pp. 76-8). See §3.6.

41 This point was made by Sciama (1958, p. 78).

1.2 The main features of the formalism of quantum mechanics

[I]n spite of many attempts at rationalization, the quantum theory remains primarily a postulated formalism, justified chiefly by the fact that it works. Historically, when the formalism failed, it was simply modified (as for example by the introduction of 'spin') until it did work.

(E.J. Zimmerman, 1966)

Modern quantum theory takes as its starting point not Bohr's Copenhagen interpretation, but instead an approach initiated by Dirac and von Neumann, and which also owes something to David Bohm's conception of the quantum properties of matter as incompletely defined potentialities.[42] In this approach, in contradistinction to that of Bohr, the concept of *quantum state* plays a key role.[43] In particular, the wave function yields a complete description of the objective properties of an individual system even when they are not observed.

In this view, objects and properties *actually exist* even when unmeasured, albeit mostly in an incompletely defined form. A quantum system has just those properties that would be revealed by a measurement. Those properties are assigned probability one by its wave function. This is the 'eigenvalue-eigenstate link', also known as the 'eigenvalue-eigenfunction link'.[44]

When a quantum system, such as an electron, is in the general state of linear superposition of various possible states,[45] it may be said to exist in or partake of *each* of the superposed states, even when these are incompatible. To the extent that they are incompatible, there is an objective indefiniteness to the general quantum state. Even so, it is always correct to speak of the state of a system. This approach is the standard interpretation today.[46] Even though it differs in an important way from Bohr's interpretation (Rosenfeld calling it 'a radical difference in conception'[47]), it is sometimes said to be a version of the Copenhagen interpretation.

The mathematical bones of the above conception of 'state' are given in the following summary, together with some analysis. The analysis continues in the sections that follow. However, the analysis is necessarily of limited scope, owing to the somewhat introductory nature of the present chapter. Chapters 2 and 3

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42 Regarding that conception, see Bohm 1951, Ch. 6, §§9,13,22, *passim*.

43 Bohm & Hiley 1993, p. 17.

44 The eigenvalue-eigenstate link is the assumption that an observable of a system has a determinate value, or the system has a determinate property, only if the state of the system is an eigenstate (i.e. eigenfunction) of the observable, or an eigenstate of the projection operator representing the property. (After Bub 1997, p. 239.) For the meaning of 'eigenstate'/'eigenfunction', see §1.2(c).

45 The concept of linear superposition is explained in §1.2(a) below.

46 Healey 1998, p. 82.

47 Cited in Stapp 1993, p. 56.

contain more detailed analysis of the conception.

The main features of the standard interpretation of quantum mechanics are:

(a) Description of the system in terms of a wave function.
(b) Use of linear hermitian operators to represent physical quantities.
(c) Use of eigenvalue/eigenfunction equations.
(d) Expansion postulate.
(e) Measurement postulate.
(f) Reduction postulate (also known as projection postulate).
(g) Use of macroscopic measuring apparatus.
(h) Heisenberg's indeterminacy principle.
(i) Spin.
(j) Pauli's exclusion principle.
(k) Essential complexity of the quantum-mechanical description of state.

The purpose of the following summary (a longer version of which is located in the Appendix) is not to attempt a comprehensive account of the formalism and theoretical basis of quantum mechanics in the standard interpretation, but simply to identify those concepts that differ from those of classical mechanics, thereby raising conceptual/philosophical issues relevant to the present enquiry. These concepts are generally interdependent, even though they appear grouped under their own headings. In particular, 'Heisenberg's indeterminacy principle', under heading (h), is not a separate principle or postulate at all, but rather a consequence of the preceding ones – which themselves, however, have been formulated having close regard to the Heisenberg indeterminacy. As for (k), 'Essential complexity of the quantum-mechanical description of state', it largely spells out some of the features and consequences of (d), 'Expansion postulate'.

In the present chapter we shall limit our treatment to non-relativistic quantum mechanics and work in the Schrödinger formalism.

(a) ***Description of the system in terms of a wave function***

For every physical system there exists a wave function determined by the physical situation, which contains all possible information about the system. The wave function is symbolized by Ψ.[48] Quantum mechanics gives rules for finding the wave function for different situations and extracting information from it. The wave function contains all the information about the observables associated with

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48 The symbol denoting the quantum wave function in the coordinate realization, written in one dimension (i.e. for a particle moving in the x-direction only), is $\Psi(x,t)$. In three dimensions it's written $\Psi(x,y,z,t)$ [or $\Psi(q,t)$ or $\Psi(\mathbf{r},t)$]. If spin is included, it may be written as $\Psi(q,s,t)$.

the system it represents. Whenever physicists work out a problem in quantum mechanics, e.g. calculate the probabilities of the future behaviour of a quantum system, it is to the wave function that they must turn. The wave function itself, though, is not an observable quantity, nor does it have a direct physical interpretation.

Schrödinger's 1926 equation describes the time-development of the wave function, and thereby of the state of the associated physical system. It plays the same role in quantum mechanics as the equations of motion in classical mechanics. In three dimensions, for a single particle of mass m, it is written as follows:

$$-\frac{\hbar^2\nabla^2}{2m}\Psi(q,t)+V(q,t)\Psi(q,t) = i\hbar\frac{\partial\Psi(q,t)}{\partial t},$$

where $V(q,t)$ is the potential energy describing the forces acting on the particle, and $\nabla^2 = \partial^2/\partial x^2 + \partial^2/\partial y^2 + \partial^2/\partial z^2$.[49]

In one dimension, the equation reduces to

$$-\frac{\hbar^2}{2m}\frac{\partial^2\Psi(x,t)}{\partial x^2}+V(x,t)\Psi(x,t) = i\hbar\frac{\partial\Psi(x,t)}{\partial t}.$$

Throughout this section, I shall usually, for reasons of convenience, give the one-dimensional form of equations.

If the system's initial state Ψ at time t is assumed to be known, Schrödinger's equation describes its subsequent time-evolution. The evolution is continuous and deterministic, and proceeds as if the wave function were a classical field described by some classical field equation such as that of Maxwell.

In the Schrödinger formalism, the wave function must be everywhere well behaved, meaning that it is continuous, has a single value at every point in space and at every time, and a continuous first derivative. Furthermore, it must conserve total probabilities (discussed in [e] below), such that for a single particle described by the normalized wave function,[50] for example, the total probability

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[49] The symbol ∇^2 ('del squared') is known as the Laplacian operator. It is not only convenient to write but also indicates here that the equation changes its form for various coordinate systems just as vectors do.

[50] 'Normalized' means that the probabilities obtained on the basis of the expansion coefficients must sum to one. The wave function is assumed to be normalized, because the total probability that the particle is somewhere in space must be unity. If Ψ is not already normalized, it can be normalized by multiplying it by a suitable constant such as A, such that $|A|^2\int\Psi^*\Psi dq = 1$. (Bohm 1951, p. 177n.) Before the procedure of normalization is carried out, the amplitude of the wave function is arbitrary, because the linearity of the Schrödinger equation allows the wave function to be multiplied by a constant of arbitrary magnitude and still remain a solution to the equation. Normalization fixes the amplitude by fixing the value of the multiplicative constant. (Eisberg & Resnick 1974, p. 153.)

of finding it somewhere is given by

$$\int_{-\infty}^{\infty} P dq = \int_{-\infty}^{\infty} \Psi^* \Psi dq = 1,$$

where Ψ^* is the complex conjugate of Ψ. That is, the integral must have a finite value, meaning that the total amount of the observable associated with the wave function is conserved, regardless of its particular distribution.

The final thing that needs to be mentioned here in connection with the wave function Ψ is the *principle of linear superposition.* A basic principle of classical wave theory, applying for example to electromagnetic waves, is that if Ψ_1 and Ψ_2 are possible wave functions representing states of the system, then any linear combination of them, $a\Psi_1 + b\Psi_2$, where a and b are arbitrary real-number weighting constants (also known as *amplitudes*), is also a possible wave function Ψ, and therefore the representation of a possible state of the system. It seems necessary to assume some such hypothesis to account for the interference of the waves, and the production of wave packets.[51] This basic principle of wave theory also applies to the wave functions of quantum theory. Given that all permissible wave functions must be solutions of Schrödinger's wave equation, the sum of the two solutions must also be a solution. This must be so since the wave equation is linear.[52] In other words, the new state, too, is completely defined by the two original states, provided that the relative weights of a and b are known, and also the phase difference of the two systems. Since that is so, we must assume that between these states there exists a relationship such that whenever the system is definitely in one state, say Ψ, it can also be correctly regarded as being partly in each of two or more other states, say Ψ_1 and Ψ_2.

(b) ***Use of linear hermitian operators to represent physical quantities***

Every physical observable or classical dynamic quantity (e.g. potential energy V, total energy E, momentum p, coordinates q, spin s), is modelled by a linear hermitian operator that operates on the wave function. For the momentum p_x, the corresponding momentum operator is $\hat{p}_x = \frac{\hbar}{i}\frac{\partial}{\partial x}$.[53] For the position x, the corresponding position operator is simply $\hat{x} = x$ (operating by x is the same as simply multiplying by x). For the total energy E, the corresponding operator is $\hat{E} = i\hbar\frac{\partial}{\partial t}$.

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51 Bohm 1951, p. 174.

52 A linear relation between two variables is one that can be represented graphically as a straight line. A linear equation such as Schrödinger's, is one in which the sum of any two of the solutions of the equation is also a solution of it.

53 Which may also be written as $\hat{p}_x = -i\hbar\frac{\partial}{\partial x}$.

Operators are mathematical functions that operate on other functions. Like the wave functions on which they operate, operators have no direct physical significance. They take as their input any quantum-mechanical wave function, and give as output either a different wave function, or possibly the same wave function times a real constant which is the measured value of the observable.

To each operator there corresponds an ensemble of numerical values (its 'spectrum'), which may be discrete or continuous. An operator is called 'hermitian' if its expectation value (defined below) is real. Similarly, the eigenvalues of hermitian operators are real, sharp, and physically realizable.

Consider the equation

$$\overline{f(x,p_x,t)} = \int_{-\infty}^{\infty} \Psi^*(x,t) f_{op}\left(x, -i\hbar\frac{\partial}{\partial x}, t\right)\Psi(x,t)dx,$$

where $\overline{f(x,p_x,t)}$ is the average of many measurements of the observable $f(x,p_x,t)$ made on identically prepared systems, known as the *expectation value*, and the operator $f_{op}(x,-i\hbar\partial/\partial x,t)$ is obtained from the function $f(x,p_x,t)$ by everywhere replacing p_x by $-i\hbar\partial/\partial x$.

The wave function contains, through the above equation, all the information that Heisenberg's indeterminacy principle allows us to learn about the observables associated with the wave function – information such as the expectation value of the coordinate x, the potential energy V, the momentum p_x, the total energy E, and, in general, the expectation value of any dynamical quantity $f(x,p_x,t)$.[54]

Schrödinger's equation may also be written as

$$\hat{H}\Psi = \hat{E}\Psi,$$

where (in one dimension) $\hat{H} = \frac{\hat{p}_x{}^2}{2m} + \hat{V}$ is a linear hermitian operator representing the total energy of the system.[55] (It is customary to call the operator representing the total energy of a system 'the hamiltonian', designated by $\hat{H}$, after the classical hamiltonian function H – an expression for the total energy of a system in terms of all the position and momentum variables for all the physical objects belonging to the system. $\hat{H}$ is also known as the 'time-displacement operator'.)

(c) *Use of eigenvalue/eigenfunction equations*

If an operator acting on a function gives back the same function, multiplied by

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54 After Eisberg & Resnick 1974, pp. 159-60.

55 In three dimensions, it is written $\hat{H} = \frac{1}{2m}\left(\hat{p}_x{}^2 + \hat{p}_y{}^2 + \hat{p}_z{}^2\right) + \hat{V}$.

some constant (number), the function is said to be an *eigenfunction* of the operator, and the constant its *eigenvalue*. The eigenvalue-eigenfunction equation for a hermitian operator $\hat{O}$ is

$$\hat{O}\psi_n = o_n\psi_n,$$

where ψ_n is the operator's eigenfunction belonging to the eigenvalue o_n. For a hermitian operator, o_n is always real.

Where a function is an eigenfunction of the operator, a measurement of the observable represented by $\hat{O}$ is certain to lead to the result o_n. Take $\hat{p}_x\psi_a = a\psi_a$, where a is an eigenvalue of the momentum operator $\hat{p}_x$, and ψ_a is an eigenfunction belonging to the eigenvalue a. A measurement of p_x is certain to give a. (In this case, $\hat{p}_x\psi_a = a\psi_a$, where a is the actual measurable value of the system in the state ψ_a.[56] Clearly, a must be a real number.) Every possible result of the measurement of an observable, with the system in any state whatever, is one of the eigenvalues of the observable. The converse is also true: every eigenvalue of an observable is a possible result of the measurement of that observable. The set of eigenvalues of an observable are just the possible results of measurements of that observable.[57]

Even though measurements always yield eigenvalues, in the general case, when a system is in a given quantum state Ψ, and provided that Ψ is not an eigenfunction of the operator $\hat{O}$, the observed value of any observable O cannot be predicted. Instead, we can speak of it having an average value for the state, and also a probability for having any specified value for the state, meaning the probability of obtaining such specified value upon measurement of the observable.[58]

Consider again Schrödinger's equation. There are many situations in which the potential energy of a particle does not depend on the time explicitly, the forces that act on it (and so the potential) varying with the particle's *position* only. In such cases the time-dependent equation may be simplified by removing all reference to t by using a standard mathematical technique called 'separation of variables'. The technique consists in searching for a solution in which the wave function can be written as the product of a position-dependent function $\psi(x)$ and a time-dependent function $\varphi(t)$:

$$\Psi(x,t) = \psi(x)\varphi(t),$$

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56 To be an eigenfunction of momentum, the wave function ψ must be of the form $\psi = ae^{ip_x x/\hbar}$. The eigenfunctions of x in momentum space are plane waves, just like the eigenfunctions of p_x in coordinate space. (Bohm 1951, p. 214.)

57 Dirac 1935, p. 32.

58 After Dirac 1935, pp. 30, 44.

where ψ and φ are functions, respectively, of x and t alone. Solutions of this form exist provided that the potential energy does not depend on the time, so that the function for the potential can be written $V(x)$. In that case, the wave function is an eigenfunction of $\hat{H}$.

When the wave function is an eigenfunction of $\hat{H}$, the function $\psi(x)$, which specifies the space dependence of the wave function $\Psi(x,t) = \psi(x)\varphi(t)$, is a solution to the differential equation

$$\left[-\frac{\hbar^2}{2m}\frac{d^2\psi(x)}{dx^2}+V(x)\right]\psi(x) = E\psi(x),$$

called the *time-independent Schrödinger equation*.[59] The equation is time independent because the time variable t does not enter into it.[60]

In addition to the time-independence/dependence, three other important distinctions between the time-independent and the time-dependent equations are: (a) the stationary state form does not contain the imaginary number i, and so its solutions $\psi_n(x)$ need not be complex; (b) it contains explicitly the total energy E; and (c) since the system doesn't gain or lose energy and the energy is well-defined, all probabilities remain constant over time. (In a series of repeated measurements of a dynamical variable in such a state, the individual values obtained will fluctuate from one experiment to the next, but the probability of obtaining a given value will be independent of the time that has elapsed since the state was prepared. This is in contrast to a wave function which is not an eigenfunction of the energy and which moves through space and spreads out so that the probabilities change with time.)[61]

Thus, the eigenfunctions $\psi_n(x)$ exist only for certain values of the energy, E_1, E_2, E_3, ..., E_n, where the energies are the eigenvalues belonging to $\hat{H}$.[62] Corresponding to each eigenvalue there is an eigenfunction (sometimes more than one), $\psi_1(x)$, $\psi_2(x)$, $\psi_3(x)$, ..., $\psi_n(x)$, which satisfies the eigenvalue-eigenfunction equation $\hat{H}\psi_n = E_n\psi_n$. Each eigenfunction is a solution to the time-independent

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59 Eisberg & Resnick 1974, p. 164. Note that since the quantity within the square brackets is the total energy operator $\hat{H}$, the above equation may also be written as $\hat{H}\psi(x) = E\psi(x)$.

60 As for the function $\varphi(t)$, it is the function that specifies the time rate of change of the wave function $\Psi(x,t)$, once the initial value is known. It is an oscillatory function of frequency $\nu = E/h$, given by the expression $\varphi(t) = e^{-iEt/\hbar}$, where E is the total energy of the particle in the system. See Bohm 1951, pp. 227-8 for relevant detail.

61 Point (c) is after Bohm 1951, pp. 225-6.

62 The eigenvalues occurring early in the list may be discreetly separated in energy, but generally become continuously distributed in energy beyond a certain energy. The total energy for a free electron, with $E > 0$ is not quantized at all, but may take any value. (Eisberg & Resnick 1974, p. 120.)

Schrödinger equation for the potential $V(x)$, i.e. an eigenfunction of $\hat{H}$. For each eigenvalue there is a corresponding wave function $\Psi_1(x,t)$, $\Psi_2(x,t)$, $\Psi_3(x,t)$, ..., $\Psi_n(x,t)$, each of which is a particular solution to the Schrödinger equation for the potential $V(x)$.

The importance of the time-independent Schrödinger equation is that it promises to give all the solutions of physical interest in the non-relativistic quantum domain.[63] For example, all solutions of the time-dependent equation can be obtained by superposing stationary-state solutions possessing different frequencies/energies (see [d] below). That results in destructive and constructive interference of the wave functions belonging to the different energies, such interference changing in position with time.

(d) ***Expansion postulate***

An arbitrary wave function Ψ for a physical system can be expanded in terms of a complete set of linearly independent, orthonormal[64] eigenfunctions ψ_n of the Schrödinger equation:

$$\Psi = \sum\nolimits_n a_n \psi_n,$$

i.e.

$$\Psi = a_1 \psi_1 + a_2 \psi_2 + \ \dots\ a_n \psi_n + \ \dots$$

where both the coefficients a_n and the values of the functions ψ_n are generally complex numbers. This is known as the *expansion postulate*. It means that the general state of the system can be expressed as a *coherent linear superposition* of states, with complex-number expansion coefficients (or amplitudes) of all the possible measurable alternatives available to the system. The coefficients a_n determine the probability of the system being in one of the eigenstates, namely the eigenstate ψ_n. Assuming that the ψ_n are normalized, the expansion imposes a restriction on the values of the coefficients, namely $\sum_n a_n{}^* a_n = 1$.

The above conception of the general state of a system has some noteworthy features, in particular its linear superposition of states, complexity, self-interference, and non-separability. See the corresponding entries in the Appendix for details of each.

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63 Eisberg & Resnick 1974, p. 167.

64 'Orthonormal' here means that the eigenfunctions are both orthogonal and have been normalized. We've seen that 'normalized' means that the probabilities obtained on the basis of the expansion coefficients must sum to one. As for 'orthogonal', it means mutually at right-angles in the mathematical state space of quantum mechanics.

Mathematically, 'orthogonality' is defined as the vanishing of the scalar product (the 'dot' or 'inner' product) between two vectors or states drawn from a common point: $\mathbf{u}\cdot\mathbf{v} = |\mathbf{u}|\,|\mathbf{v}| \cos\theta = 0$, where θ is the angle between them.

(e) *Measurement postulate*

Upon measurement of a physical system described by the wave function $\Psi = \sum_n a_n \psi_n$, the coherent linear superposition of states (eigenfunctions of the operator $\hat{O}$) represented by the wave function instantaneously reduces to some particular one of the eigenfunctions of $\hat{O}$, and the measurement yields one of $\hat{O}$'s eigenvalues o_n. Such reduction is also known as the 'collapse of the wave function'.

The *probability* of obtaining the eigenvalue o_n belonging to $\hat{O}$ in any particular measurement of the physical observable O is given by $|a_n|^2$.This is known as the measurement postulate. The measurement postulate represents a physical interpretation of the expansion coefficients in terms of probabilities, the probabilities, however, depending quadratically on these wave functions. This is a crucial conception of quantum theory, in that it provides a connection between the seemingly incompatible wave and particle descriptions of quantum systems.

The average of many measurements of the observable O on identically prepared systems is known as the *expectation value* of the observable O:

$$\bar{o}_\Psi \equiv \sum_s \int \Psi^*(q_1 ..., s ..., t) \hat{O} \Psi(q_1 ..., s ..., t)(dq_1 ...)$$
$$= \sum_n |a_n|^2 o_n,$$

where $\bar{o}_\psi$ is the expectation value of the observable, Ψ^* is the complex conjugate of Ψ, dq is an element of volume (= dx, dy, dz for a simple particle), s is the spin, and for a normalized eigenfunction $\psi(q)$, $\sum_n |a_n|^2$ is unity. In general, when a system is in a given quantum state (when the system's wave function Ψ is given), and provided that Ψ is not an eigenfunction of the operator $\hat{O}$, the observed value of any observable O cannot be predicted. Instead, an average of many measurements is needed to obtain the expectation value.[65] The observed value of the observable O fluctuates about some mean (namely the expectation value).

If the eigenvalues are not discrete but continuous, as is the case with position, probabilities have to be replaced by *probability densities*. The relation between the probability density $P(q,t)$ and the wave function Ψ is $P(q,t) = \Psi^*(q,t)\Psi(q,t)$.[66] The probability density is used to specify the prob-

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[65] If, however (as we saw in (c) above), the state of the system is chosen such that $\hat{O}\psi_n = o_n \psi_n$, i.e. where both (i) o_n is an eigenvalue of the operator $\hat{O}$ and (ii) the chosen wave function ψ_n is an eigenfunction belonging to the eigenvalue o_n, then the observable O, e.g. the momentum p_x, has a predictable and reproducible value which never fluctuates. In that case, it is its conjugate variable x which fluctuates, becoming completely indefinite. (After Bohm 1951, pp. 209-10.)

[66] This is in position space. In momentum space the corresponding relation is given by $P(k) = \Psi^*(k)\Psi(k)$, where k is the propagation vector (the direction in which the phase

ability $P(q,t)dq$ of finding the particle associated with the wave function $\Psi(q,t)$ in the infinitesimal volume element dq in the vicinity of q at time t. Using the probability density, we can obtain the expectation value of the particle's position by weighting each position q with its associated probability density and integrating:

$$\bar{q} = \int qP(q,t)dq = \int \Psi^*(q,t)\hat{q}\Psi(q,t)dq.$$ [67]

A consequence of the fact that physical significance of Ψ is confined to its absolute square is that Ψ is not simply a classical probability function representing our knowledge of the system. That's because the superposed alternative possible states of the system can interfere with each other. An example is provided by Young's two-slit experiment, where, on the quantum level, one possibility is reinforced and the other wiped out by interference of the system with itself, as the macroscopic interference pattern obtained reveals.

Consider a modern electron version of Young's 1900 two-slit experiment in the context of Born's interpretation of matter waves (§2.1). An electron gun is used to send electrons, one by one, toward a screen with two tiny slits A and B in it set close together. The electrons all have the same initial momentum, and therefore the same wave function. What is the probability that an electron will pass through one or the other of the two slits to hit the detecting screen?

In classical physics, to find the probability of two independent events, one simply adds the probabilities of each. Classically, the probability of a particle passing through one or the other of the two slits to hit the second screen is given by the probability of the particle passing through one slit *plus* the probability of its passing through the second slit.

However, electron diffraction experiments show that the wave functions for the electron don't combine in this simple way. If both slits are open, an interference pattern of bright and dark fringes is built up on the detection screen. The bright fringes consist of many tiny white dots each of which is produced by the arrival of an individual electron. The dark fringes indicate the arrival of few or no electrons. The pattern of bright and dark fringes emerges, albeit slowly, dot by dot, even if the intensity is made so low that only one particle traverses the slit-system at a time – or even if many different photographic plates from different, otherwise identical experiments are superposed.

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of the wave changes). Its magnitude or spatial frequency k is $2\pi/\lambda$ $[= \mathbf{p}/\hbar]$. (Bohm 1951, p. 93.)

[67] Note that $P(q,t)dq = \Psi^*(q,t)\Psi(q,t)dq$ is an actual probability: a real number – the probability that the particle will be located in the selected infinitesimal volume between q and dq at time t, whereas $P(q,t) = \Psi^*(q,t)\Psi(x,t)$ is the probability density: a *function* – a probability per volume element for a particle to be located near the coordinate q at time t. This is likely to be different at different coordinate-points (which is why it is a function).

The pattern shows that we need to *first* add up the wave functions (or amplitudes) corresponding to the electron entering slit A and slit B (these being the superposed possibilities) and *only then* square their sum to get the correct probability (rather than squaring each wave function separately and then adding the squares as above). Denote the wave function at an arbitrary point behind the slits by $\Psi(x) = \Psi_A(x) + \Psi_B(x)$, where $\Psi_A(x)$ represents that part of the wave reaching the point x that has come from slit A, while $\Psi_B(x)$ represents that part which has come from slit B. If both holes are open, the probability $P(x)$ that the electron will reach the point x is generally not, as the classical theory of probability would imply, $P_A(x) + P_B(x)$, i.e. $P(x) = |\Psi_A(x)|^2 + |\Psi_B(x)|^2$. Instead, the probability is given by

$$P(x) = |\Psi_A(x) + \Psi_B(x)|^2 = |\Psi_A(x)|^2 + |\Psi_B(x)|^2 + \Psi_A{}^*(x)\Psi_B(x) + \Psi_B{}^*(x)\Psi_A(x).$$[68]

The last two terms are *interference-terms*, which are additional to the single-slit terms $|\Psi_A|^2$ and $|\Psi_B|^2$, and which account for the pattern of bright and dark fringes.[69] The interference terms are generally different from zero and would not be present if the experiment involved a probability distribution of classical particles, coming either through slit A or slit B. The presence of the interference terms is characteristic of the behaviour of waves, and is taken to indicate in the standard interpretation that we've encountered the *wave properties of matter*. The mathematics, it turns out, is the *same* as for water-waves (as Feynman points out),[70] save that the amplitudes of the quantum waves are complex rather than real. The ability of electrons to exhibit the wave-like property of self-interference is characteristic of all quantum-mechanical systems.[71] And there is even more trouble to come from the same source.

That is because the quantum theory of measurement predicts that the self-interference can be made to take place or not take place at will even *after* the electron has already passed through the slits and travelled much of the way to the detecting screen. Only at that point in the experiment does the experimenter (or a random-number generator) take the decision as to the measurement strategy, using a simple but fast mechanism, thereby determining, as John Wheeler put it, 'what kind of indelible evidence shall be produced: "which-slit" evidence, or

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68 Bohm 1951, p. 121.

69 The sum of the last two terms (the interference terms) may also be written as the product $2|\Psi_A(x)||\Psi_B(x)|\cos\theta$, where θ is the phase, i.e. $a_A{}^* a_B + a_B{}^* a_A = 2|a_A||a_B|\cos\theta$.

70 Feynman, Leighton & Sands 1965, III-1, p. 6.

71 For a detailed discussion of the two-slit experiment, the role of the interference terms in measurement and the quantum theory of measurement generally, see Bohm 1951, Ch. 6, §§3-8, & Ch. 22. For more recent accounts, see e.g. Hughes 1989, pp. 226-31; van Fraassen 1991, p. 111; Albert 1992, pp. 12-14; Goswami 1997, pp. 107-15.

"double-slit" evidence',[72] i.e. evidence consistent with a scatter pattern, or with an interference pattern.[73]

Such 'retroaction', Bohr explicitly pointed out in 1949,[74] is to be *expected* on his (Copenhagen) interpretation of quantum mechanics with its doctrine of *complementarity*, according to which the dynamic attributes such as position and momentum do not exist until they are actually observed – and even then they are relational – manifestations of the entire experimental arrangement. In particular, once we locate the electron, we lose information about its momentum. As soon as we do so, we also lose information about its wavelength, as is implied by de Broglie's relation $\lambda = h/p$ connecting wavelength and momentum. But if there still existed interference fringes, we could measure the wavelength from their spacing. Thus the interference pattern itself must be destroyed. (This can also be seen if we apply the Heisenberg indeterminacy principle not just to quantum entities such as electrons, but also to the macroscopic measuring apparatus such as the two-slit screen used in the experiment. If the position of the slits can be known only to an accuracy equal to or greater than the separation between the fringes, the fringes will be impossible to observe.)[75]

Following up on Bohr's remark, Wheeler in 1977 described seven different versions of a *gedankenexperiment* in which such retroaction would be expected to occur, their common feature being that each imposed a choice between complementary modes of observation.[76] Wheeler's experiment (beam-splitter version) was successfully carried out five years later by groups working independently at the Universities of Maryland and Munich.[77]

The lesson seems to be, as Heisenberg once put it, that we learn, not about nature itself, but nature exposed to our methods of questioning. Indeed, according to Wheeler and the Austin School of the Copenhagen interpretation, the lesson is that 'the past has no existence except as it is recorded in the present'. And more generally, no phenomenon is a phenomenon until it is an *observed* phenomenon. 'The universe does not "exist, out there", independent of all acts of observation. Instead, it is in some strange sense a participatory universe.'[78]

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72 Wheeler 1978, p. 28.

73 Notice that it is the *type* of pattern that will be observed that can be selected retrospectively, but not where the individual hits will occur on the screen in each type of pattern.

74 Bohr 1949, p. 230.

75 This last point is after Goswami1997, p. 109.

76 J.A. Wheeler, 'The "past" and the "delayed-choice" double-slit experiment'. Presentation made to a conference at Loyola University, New Orleans, June 2-4, 1977. Reproduced in Marlow 1978, pp. 9-48.

77 Reported by Horgan 1992, p. 75.

78 Wheeler 1978, p. 41.

(f) *Reduction postulate (also known as projection postulate*

We have seen that upon measurement, the coherent linear superposition of states or eigenfunctions represented by the wave function $\Psi = \sum_n a_n \psi_n$, where ψ_n are eigenfunctions of the operator $\hat{O}$, instantaneously reduces (Ψ collapses) to some particular one of the eigenfunctions, and the measurement yields one of $\hat{O}$'s eigenvalues o_n, with a certain probability for each. After a particular measurement has yielded the eigenvalue o_n of $\hat{O}_n$, the system *remains* in the state described by the eigenfunction ψ_n – hence an immediate repeat measurement yields the same result.[79] In other words, once we've obtained such an eigenfunction, we must be able to go on, at least in principle, to measure the observable again and again, in time so short that the wave function hasn't changed significantly (except for the phase factor which isn't relevant), obtaining the same result each time.[80]

(The time period between the original and repeat measurements must be short in order to obtain the same value because, unless the ψ_n is also an eigenfunction of $\hat{H}$, the system does not remain in that state. Instead, the function develops in accordance with Schrödinger's equation.)

The conditions for the actualisation, in the reduction process, of any particular one in preference to another of the various superposed complex-number-weighted possible states or *potentia* is nowhere made explicit in the formalism, the theory giving only the *probabilities* for such actualisation. Indeed, according to the standard interpretation, no such conditions for the actualisation of individual potentia exist.

(g) *Use of macroscopic measuring apparatus*

The measurement postulate is usually taken to presuppose that all measurements in quantum mechanics are to be made with macroscopic observing instruments, i.e. classically describable measuring apparatus, and that macro-observables retain sharp values at all times. For example, position measurements are to be made with macroscopic rods placed between macroscopically separated marks, and time is to be read by a macroscopic physicist from a macroscopic clock.[81] Bohr, in particular, always insisted on the 'indispensable use of classical concepts in the interpretation of all proper measurements'.[82] Consequently, quantum theory seems to require that the world be divided into two – a quantum-mechanically described system, and a 'classical' remainder. The division may be made in particular applications in one way or another according to the degree of

79 Goswami 1997, p. 68.
80 After Bohm & Hiley 1993, p. 18.
81 Zimmerman 1966, pp. 489-90.
82 Bohr 1935b, p. 701. (In Wheeler & Zurek 1983, p. 150.)

accuracy and completeness aimed at. Thus, there appears to be an essential and arbitrary cut between measuring and measured systems, as Bohr, Schrödinger and Bell have all emphasized.

Such a cut exists even in the Dirac/von Neumann approach, in which the world is represented entirely in quantum-mechanical terms. If *everything*, including measuring instruments, is to be represented quantum-mechanically in terms of quantum waves undergoing unitary evolution, then there is nothing *special* about a measuring instrument that could bring about a wave function collapse. Therefore the wave function necessarily develops into a sum of parts that correspond to incompatible *macroscopic* possibilities. But that seems wrong as such are never observed. A cut needs to be put in by hand somewhere in the chain of measurement to accord with the fact that macroscopic observables have determinate values. This is the *measurement problem* of the standard interpretation.

It seems to follow, as Bohm writes, that quantum theory 'does not deduce classical concepts as limiting cases of quantum concepts' after all, differing in this regard from relativity theory in which Newtonian concepts are deduced as limiting cases of the theory.[83] Instead, quantum theory simply *presupposes* the classical level and the general correctness of classical concepts on that level.

(h) *Heisenberg's indeterminacy principle*

Heisenberg's indeterminacy principle is unremarkable in the context of the preceding postulates. That is because two non-commuting operators $\hat{O}$ and $\hat{O}'$ cannot have the same eigenfunctions.[84] That being the case, the theory predicts that any ensemble of particles will have a spread of eigenvalues for the observables represented by $\hat{O}$ and $\hat{O}'$, e.g. x and p_x, such that $\Delta x \Delta p_x \geq \frac{\hbar}{2}$. That is, if many particles are assembled within a small space (Δx), the group must have a large spread of x-momenta ($\Delta p_x \geq \frac{\hbar}{2}\frac{1}{\Delta x}$). Alternatively, if a group of particles all having about the same x-momentum is assembled (Δp_x is small), they must be spread over a large region of space ($\Delta x \geq \frac{\hbar}{2}\frac{1}{\Delta p_x}$). Likewise, for an ensemble of radioactive or unstable particles or microphysical systems, the spread of the energies ΔE which will be observed and the spread of the Δt at the time of emis-

83 Bohm 1951, p. 625.

84 Take the operators for position and momentum. They do not commute, i.e. the results of $\hat{x}.\hat{p}$ and $\hat{p}.\hat{x}$ applied to the same wave function are different. Consequently, we cannot identify a function that would be an eigenfunction of both position and momentum. It follows from the above postulates of quantum mechanics that there can be no state in which both the physical observables x and p_x have a well-defined value. (After Prigogine & Stengers 1984, p. 223.) See also Sachs 1988, pp. 130-2 for some discussion.

sion will be related by $\Delta E \Delta t \geq \frac{\hbar}{2}$. That is, the members of the ensemble will not all radiate precisely the same energy, nor will they all radiate at the same time.

There are as many indeterminacy relations as there are pairs of operators not having the same set of eigenfunctions.[85] These relations also apply when we go from an ensemble of particles to the single particle case, i.e. to an ensemble of measurements of identically prepared single particles. Again, over many runs of the experiment, the same relations will be found to apply. For example, taking the latter one, it will be found that the spread of the energies ΔE which will be observed and the spread of the Δt at the time of emission will be related by $\Delta E \Delta t \geq \frac{\hbar}{2}$.

Another way of proceeding is to derive the indeterminacy relations by combining the de Broglie-Einstein relations, $p = h/\lambda$ and $E = h\nu$ with simple mathematical properties that are universal to all waves, namely $\Delta x \Delta k \geq 1/4\pi$, and $\Delta t \Delta \nu \geq 1/4\pi$ (where k is the spatial frequency or wave number, $1/\lambda$, i.e. the number of waves per unit length). The reason why the de Broglie-Einstein relations are combined with properties universal to all waves is because of 'wave-particle duality'. To calculate anything in quantum mechanics, such as the probable future history of a particle, we need to treat the system in question including the particle itself as a wave of some kind. This is the main significance of de Broglie's relations. See Appendix(h) for the details.

There is nothing remarkable about the indeterminacy relations themselves, given quantum complementarity. The entire mystery of quantum mechanics lies, not in the indeterminacy but in the *complementarity*, and its interpretation – which is inseparable from an interpretation of h.

(i) *Spin*

Identical particles in Schrödinger quantum mechanics need to be ascribed an additional degree of freedom that has no exact classical counterpart. That degree of freedom is the particle's *spin*, which may be described as a kind of intrinsic angular momentum, present even when the particle is otherwise at rest. It is often denoted by S, and is of magnitude $s\hbar$, where s is either an integer (0, 1, 2, ...) or a half-integer (1/2, 3/2, ...). Particles with integer spins are called *bosons*. Examples are the photon, which has spin 1, the pion with spin 0 and the hypothetical graviton with spin 2. Particles with half-integer spins are called *fermions*. Examples are the electron, proton, neutron, neutrino, and their antiparticles, all of which have spin 1/2. Another example is the omega baryon, which has spin 3/2. The component of the spin vector **S** of any elementary particle in any reference direction along which the spin may be measured (such reference direction usually

85 After Zimmerman 1966, pp. 493-4.

defined by a magnetic or electric field) can generally take on $2s + 1$ possible values, from $-s$ to $+s$ in increments of 1. This classically unexpected feature is known as space quantization. It means that whichever reference axis is selected, the only possible values that can be obtained for the spin component along that axis are $\pm 1/2\hbar$. This is so even if the spin was known, pursuant to a previous measurement, to point along a *different* axis.

Two important related features of spin should be noted. The first is that spin cannot be derived from Schrödinger's theory, but must be introduced in that theory as a separate postulate. The reason is that the theory is an approximation which ignores relativistic effects. The spin can be derived, however, from Dirac's relativistic theory, which uses the same postulates as Schrödinger's theory, but replaces the classical energy equation $E = \left(p^2 / 2m\right) + V$ by its relativistic equivalent $E = \left(c^2 p^2 + m_0{}^2 c^4\right)^{1/2} + V$.[86] The second is that, unlike ordinary angular momentum, spin is not a function of time and position, meaning that two otherwise identical states can have different spins. In fact, whenever two states exist having the *same* space and time dependence, they *must* have different spins. Thus, *spin must be considered as part of the wave function itself*. All of the eigenfunctions in the expansion of a wave function of a single particle must be of the same spin (as the particle itself).[87]

And finally, it is worth mentioning that a spin 1/2 particle (such as an electron or neutron) *needs to rotate twice, i.e. by 4π, or by 720°, to return to its initial physical state*. This is indicated by its spin being $1/2\hbar$ and not $\hbar$, i.e. h/720° and not h/360°. After only a 360° rotation, the particle's spin eigenfunctions are the *negatives* of the initial spin eigenfunctions, and so differ by a phase factor.[88] A further rotation of 360° is required to restore the original state. This is the reason why the magnetic field – and so the gyromagnetic ratio – due to the electron's spin, is twice the value expected on the basis of using a classical model such as an electrically charged ball. A similar property would be possessed by a traveller on a surface with the connectivity of a Möbius strip. The traveller would need to circle twice (rotate by 720°) to return to his/her starting configuration.

(j) The Pauli exclusion principle

The principle states that no two particles with half-integer spins, such as electrons, can be in precisely the same state (described by the same wave function), when spin is included in the description of the state. The origin of the principle is mathematical, to do with the existence of symmetric and antisymmetric eigenfunctions and the effects of the exchange of particle labels such as 'right' and

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86 Eisberg & Resnick 1974, pp. 301-2.

87 This paragraph is after Longini 1970, p. 39.

88 Davies 1984, p. 83.

'left'. For details, see Appendix(j).

The exclusion principle, when coupled with the existence of quantized spin, leads to an explanation of a great many otherwise puzzling features of the world, including, just to name a few, the periodic table of elements, the difference in the behaviour of electric conductors and insulators, superconductivity, the existence of dwarf and neutron stars, and the fundamentally different statistical behaviour of particles of integer and half-integer spin. The rules obeyed by integer-spin particles are called Bose-Einstein statistics, and those followed by half-integer spin particles are called Fermi-Dirac statistics.[89] These rules explain why, for example, we perceive well-defined electromagnetic waves such as light waves and radio waves but never electron waves, even though electrons possess an associated wave just like photons do.[90]

(k) *Essential complexity of the quantum-mechanical description of state*

The quantum wave function $\Psi(x,t)$ is complex. The complexity of the wave function means that there are two parts or two functions to the full function, a real part and an imaginary part. This is in contrast to the wave functions of classical mechanics, such as that modelling for example a vibrating string which has only a real part to it. [91] The complexity has physical significance, in the following ways.

(1) Since no complex quantity can be measured by any actual physical measuring instrument, we know that we cannot ascribe a physical existence to the wave described by the wave function, at least in the same simple way that e.g. water waves have a physical existence.

(2) Although complex numbers occur in the equations of classical physics, too, as a computational 'shorthand' to avoid having to do trigonometry, in quantum mechanics, the connection between complex quantities and theory seems more intrinsic. The assumption that the amplitudes (the expansion or

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[89] For a discussion of the two kinds of rules, see e.g. Eisberg & Resnick 1974, Ch. 11; Penrose 1989, pp. 277-8; Feynman, Leighton & Sands 1965, III, Ch. 4.

[90] See e.g. Davies 1984, pp. 144-5; Gribbin 1985, pp. 95-9.

[91] The quantum wave function $\Psi(x,t) = re^{i\theta}$, representing a free particle such as an electron moving in the absence of an accelerating field of force, consists of two wave components at right-angles to each other in a complex plane. We can think of the wave described by the wave function (consisting of the two components) as rotating in abstract space about its axis of propagation. As the phase of the (normalized) wave function increases over time from 0 to $\pi/2$ to $3\pi/2$ to 2π etc., the complex exponential changes in value from +1 to i to –1 to $-i$ and cyclically repeats, maintaining however a constant modulus $|r| = 1$ throughout its phase changes. The phase change is equivalent to rotating the wave about its axis of propagation. (A classical analogue would be a circularly polarized electromagnetic wave, with the electric field rotating around the y axis, and the magnetic field [at 90° to the electric field and also 90° out of phase with the electric field] rotating in sympathy with it.)

weighting factors) can be complex numbers is necessary for the mathematical formulation of the quantum-mechanical principle of superposition.[92] In particular, the ability of an individual quantum system to be able to self-interfere and generally to exhibit a range of associated subtle, non-classical physical properties depends crucially on its being in a linear superposition of states in which the weighting factors are *complex*, and not mere ratios of probabilities.

(3) The complexity plays a part in the quantum-mechanical theory of measurement. The probabilities are obtained by multiplying the quantum wave function by its *complex conjugate*. The result of doing so is always equal to the absolute square of the wave function. The absolute square of the wave function $\Psi(x,t)$ gives the probability density $P(x,t)$ of some particular state of the system being found, e.g. of an electron being found at the coordinate between x and dx, at time $t \pm dt$. The procedure for obtaining the probabilities by multiplying the wave function by its complex conjugate has the character of a *deus ex machina*. It is postulated – and it works.

(4) The complexity in Schrödinger's equation was forced upon Schrödinger. His equation is complex because it relates a first time derivative to a second space derivative, which is necessary because the equation is based on the energy equation which relates the first power of total energy to the second power of momentum.[93] It turned out that it is just not possible in the non-relativistic theory to have other than an equation that is of first order with respect to time and a complex wave function.[94]

1.3 How can quantum mechanics be like that? The problems of interpreting quantum mechanics

'We actually made a map of the country, on the scale of a mile to the mile!' 'Have you used it much?' I enquired. 'It has never yet been spread out', said Mein Herr ... 'we now use the country itself, as its own map, and I assure you it does nearly as well.'

(Lewis Carroll, Sylvie and Bruno Concluded)

Here are brief details of the interpretative problem posed by quantum mechanics. The problem is closely connected with the quantum-mechanical conception of 'state', described in the preceding section.

Classically, if two or more physical systems are put into identical states, their subsequent behaviour will be identical. In quantum mechanics, if two or more quantum systems are put into identical states, i.e. states described by identical wave functions, their subsequent behaviour will generally differ. Yet it is

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92 After Dirac 1935, p. 16.
93 Eisberg & Resnick 1974, p. 147.
94 For a discussion, see Bohm 1951, pp. 84-8.

also maintained that the wave function contains *complete* information about the physical system – which makes the quantum wave function crucially different from any classical probability function. It might be wondered: how can the behaviour of systems described by identical wave functions differ if the wave function contains *complete* information about the systems? According to the standard interpretation of quantum mechanics this is an illegitimate question: quantum mechanics is simply an intrinsically probabilistic theory, and such behaviour is simply a feature of any intrinsically probabilistic theory. Moreover, it is not the business of physics to ask questions that are unanswerable even in principle.

The difference between classical and quantum conceptions of state owes its origin to the quantization exhibited by quantum-mechanical systems, and the ensuing duality exhibited by matter on the quantum level. We have seen that if we want to calculate the probable future evolution of a quantum-mechanical system (i.e. do what quantum mechanics is designed to do), we must first assume that the system is a wave of some kind, described by a wave function. The evolution of this wave function is governed by a deterministic dynamical law, known as Schrödinger's equation (§1.2[a]). In operational terms, the wave function is a short-hand expression of that part of our information concerning the past of the system that is relevant for predicting its future behaviour.[95]. It acts, in effect, to specify the relative probabilities of a selected observable taking on one or another of its possible values upon measurement. Taken together, these relative probabilities constitute a probability distribution for the outcome of a measurement made on the object. The quantum mechanical laws of physics are all about how this wave function evolves in time.

1.3.1 The measurement problem

It's no use, young man. It's turtles all the way down.

(Fred Hoyle, Home Is Where the Wind Blows 1994)

Now, there is a problem with this idea in that Schrödinger's equation (call it a 'first category' law) applies only when the system is unobserved. It needs to be supplemented by a 'second category' of *probabilistic* laws covering those situations in which the system is being observed. In this category of laws are the measurement and reduction postulates (§1.2[e],[f]). The act of observation (or measurement) somehow suspends the deterministic first category laws and allows the probabilistic second category laws to take over in an unexplained way.

Given the standard interpretation, something like this *must* happen. Else

95 Wigner 1967, p. 166.

measurements would generally not have determinate outcomes. For example, take an electron that's initially described by a quantum mechanical wave function according to which the electron is in a superposition of being in region A and being in region B. Suppose the position of the electron were measured. The first category laws (Schrödinger's equation) would predict, not that the electron would be found either in region A or region B, but that a linear superposition of outcomes would occur. Suppose that the measuring device has a pointer with two positions, one to indicate that the electron is in region A, and another to indicate that it is in region B. If the entire system of electron and measuring device is described by the first category laws (in other words, strictly quantum-mechanically), it follows that a measurement ought to transfer the hybrid state of the electron *from the electron to the larger system*. Following a measurement, the electron may indeed be taken to be either at A or at B, i.e. in a determinate though unknown state. However, there is a cost. Instead of the pointer of the measurement device actually *pointing* to either A or to B, the *pointer itself* ought to be found in a superposition of pointing to A and B. In short, the first category laws would predict that the macroscopic measuring device itself would end up in a physical condition in which there is no matter of fact about where its pointer is pointing. It hardly needs saying that this (whatever 'this' might be, precisely, as Albert remarks) is not what is observed upon measurement.[96] Instead, our measurement would generally have a determinate outcome. The pointer would point *either* to A *or* to B. According to the standard interpretation, though, this ought to be impossible. Strictly speaking, to get a determinate reading, the measuring device would first need to be connected to *another* measuring device which reads the output of the first device; the output of this second device in turn would need to be read by a third device, and so on. This chain of measurement needs to be broken by the second category laws. (The alternative would be to postulate an arbitrary cut between measured and measuring systems, i.e. microscopic and macroscopic systems, and stipulate that the laws of quantum mechanics don't apply to macroscopic systems. But that way of trying to evade the measurement problem seems little different, in effect, from the postulation of first and second category laws, and the stipulation that the act of measurement somehow suspends the operation of the first category laws and allows the probabilistic second category laws to take over. We'll look at it in more detail in §1.4.)

'It is an extraordinary peculiarity of the standard textbook formulation of quantum mechanics', writes Albert, 'that there are two very different categories of such laws, one which applies when the physical systems in question are *not*

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[96] Albert 2000, pp. 141-2. The measurement problem is of course well-known. See e.g. Earman 1986, pp. 219-26 for a statement of it, and discussion concerning its insolubility.

being directly observed, and the *other* of which applies when they *are*.'[97] The problem of what to do about this, how to fix it up, is known as the *measurement problem*.[98]

Let us look at the problem more closely. It arises with full force when the standard interpretation seeks to describe *both the measured and measuring systems* in quantum mechanical terms. The standard interpretation takes an observable as having a determinate value only if the quantum state is an eigenstate of that observable. (This is the 'eigenvalue-eigenstate link' mentioned at the beginning of §1.2.) If the state isn't an eigenstate of the observable, *no* determinate value is attributed to the observable in the standard interpretation. Now consider an interaction of two quantum mechanical systems that can be interpreted as a measurement of one system on the other, such as in our electron example above. It follows immediately from the linearity of the Schrödinger evolution and the eigenvalue-eigenstate link[99] that the state of the composite system of electron and measuring device is not an eigenstate of the observable measured in the interaction, and not an eigenstate of the indicator observable functioning as a 'pointer' (or measurement apparatus). Instead, the state of the composite system is the following *superposition*:

$$\psi_{\text{electron-measuring device system}} = (\psi_{\text{electron at A}} \times \psi_{\text{pointer points to A}}) + (\psi_{\text{electron at B}} \times \psi_{\text{pointer points to B}})$$

It is evident that the issue isn't why a *particular* definite result is obtained; it is rather why *any* definite result is obtained at all. That is why Leggett, for one, writes that 'quantum mechanics *absolutely forbids* a measurement to take place, if by 'measurement' is meant a process which has the features ascribed to it in the standard textbook account'.[100] And Stapp writes, 'In short, the mathematical

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97 Albert 2000, pp. 140-1.

98 Albert 2000, pp. 142-3. Craig Callender has succinctly described the measurement problem as of the following three inconsistent propositions: '(1) The wavefunction formalism of QM is representationally complete, i.e. something is an element of reality iff it is represented by the wavefunction. (2) The wavefunction always evolves according to a linear equation of motion. (3) Measurements have determinate outcomes. (Callender 1998, p. 154.)

99 The eigenvalue-eigenstate link is the assumption that an observable of a system has a determinate value, or the system has a determinate property, only if the state of the system is an eigenstate (i.e. eigenfunction) of the observable, or an eigenstate of the projection operator representing the property. (After Bub 1997, p. 239.) For the meaning of 'eigenstate'/'eigenfunction', see §1.2(c).

100 Leggett 1987b, p. 87. He notes (pp. 87-8) that if quantum mechanics is a universal theory, it must apply not only to single atoms and molecules, but also to arbitrarily large collections of them, and in particular to those collections we have chosen to use as measuring devices. So even though it may not be obviously *necessary* to describe these objects, and their interactions with the microsystems whose properties are to be measured, in explicitly quantum mechanical terms, it is at least *legitimate* to do so, in view of the universality of quantum theory. Yet on closer inspection, the notion of 'measurement' has, as he puts it, 'dissolved before our eyes; there is no magic ingredient in the

properties of the wave functions are completely in accord with the idea that they describe the evolution of the *probabilities* of the actual things, not the actual things themselves. The idea that they describe also the evolution of the actual things themselves leads to metaphysical monstrosities.'[101] The problem arises from the standard theorist wanting to have things both ways: the wave function is supposed to have *ontological significance* as representing the quantum state, while at the same time being a *complete description* of quantum reality.

Another way of stating the measurement problem is that quantum mechanics is usually regarded as containing classical mechanics as a limiting case. Yet it seems to require this limiting case for its own formulation – and so does not contain classical mechanics as a limiting case after all.[102] That's because measurement in quantum mechanics always presupposes the existence of macroscopic, essentially classically describable measuring apparatus. Consequently, the standard interpretation of quantum theory seems to require, *de facto* even if not *de jure*, that the world be divided into two – a quantum-mechanically described system, and a 'classical' remainder.

> The necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a *principal distinction between classical and quantum-mechanical description of physical phenomena*.[103]

The division is made in particular applications in one way or another according to the degree of accuracy and completeness aimed at. There is nothing in the mathematics to tell us how to make it – to tell us what is 'system' and what is 'measurement apparatus', or which natural processes have the special status of 'measurements'.[104] Bell writes that the division introduces an essential ambiguity into quantum theory, and it is the toleration of this ambiguity, permanently at the most fundamental level, that is the real break with the classical ideal.

> For me it is the indispensability, and above all the shiftiness, of such a division that is the big surprise of quantum mechanics.[105]

It is only discretion and good taste, born of experience, writes Bell, that enables us to use quantum mechanics. But in a serious fundamental formulation, such

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process of interaction of a microsystem with a measuring device which could lead to the reduction of the wave packet postulated in the standard textbook discussions of the axioms of quantum mechanics' (p. 89).

101 Stapp 1993, p. 55.

102 Or as Bohm put it, it quantum theory does not appear to deduce classical concepts as limiting cases of quantum concepts (Bohm 1951, p. 625).

103 Bohr 1935b, p. 701.

104 Bell 1987, pp. 188, 174.

105 Bell 1987, p. 188.

concepts seem to be inappropriate.[106]

Bohr's original Copenhagen interpretation doesn't have a measurement problem, at least one describable in quite the same terms as the standard interpretation. That's because (a) Bohr repudiated detailed ontology, and (b) he never treats a measurement as an interaction between two *quantum* systems, but rather as an interaction between a quantum and a *classical* system. Bohr always insisted on the 'indispensable use of classical concepts in the interpretation of all proper [quantum-mechanical] measurements'.[107] Wave functions, according to Bohr, are analogous to the probability functions of classical physics in that they are only associated with the study of *finite* systems. More explicitly, only the prepared and measured systems are represented by wave functions. As for the devices that prepare and examine those systems, they are regarded as part of the classical physical world. Their space-time dispositions, such as pointer readings, are to be interpreted as information about the prepared systems under examination (see §3.2). The probabilities obtained are to be interpreted as the probabilities of specified responses, such as pointer readings, of the measuring devices under specified conditions.[108] Consequently, as pointed out for example by Bub, the interpretation doesn't need a special postulate to describe the stochastic 'projection' or 'collapse' of the quantum state of the system onto an eigenstate of the measurement instrument reading and the measured observable, i.e. a state in which these observables are determinate.[109]

Bohr's interpretation is not totally immune to the measurement problem, however, owing to its arbitrary division of the world into quantum and classical systems (prepared and preparing/measuring systems). His interpretation also raises other issues, for example concerning the ontological status of matter waves and the wave function (despite Bohr's repudiation of detailed ontology), the transmission of 'influences' by them (despite Bohr's repudiation of 'influences'), the account to be given of the state of Schrödinger's cat (§1.4), and the nature and ultimate significance of complementarity.

As for the standard interpretation, questions raised by it include the following:

- Are measuring devices to be described quantum-mechanically? If not, how can quantum mechanics be complete if it can't be used to describe both kinds of systems? And why is there an arbitrary cut between measured and measuring systems?

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106 Bell 1987, p. 174.
107 Bohr 1935b, p. 701. (Reprinted in Wheeler & Zurek 1983, p. 150.)
108 See for example Stapp 1993, p. 56, whom I've followed in this and the preceding two sentences.
109 Bub 1997, pp. 3-4.

- Why do two very different types of processes occur within quantum mechanics: (i) In the absence of measurement, a continuous evolution of the wave function (described by first category law); and (ii) a discontinuous collapse of the wave function upon measurement (described by second category law)?

- Why does the collapse need to be *postulated*? Why doesn't it arise naturally within the formalism itself, which is supposed to be complete? What is the nature of the connection between a measurement and the collapse?

- *When* is a measurement? (Just when does the collapse occur?)

- What constitutes a 'measurement' in quantum mechanics, anyway? Can the concept of measurement be defined in physical terms without internal inconsistency?

These questions are highlighted in the 'Schrödinger's cat' thought experiment, described in §1.4. The following additional question arises in connection with measurement in both interpretations:

- How are we to explain the essential complexity of the quantum conception of 'state' – which is the reason for the peculiar quantum measurement rules (e.g. why we multiply the wave function by its complex conjugate to obtain the quantum mechanical probabilities)?

These are all questions connected with the measurement problem of the standard interpretation. However, it needs to be kept in mind that the measurement problem itself is part of a more general interpretative problem of quantum mechanics. The more general problem is: how are we to interpret the quantum-mechanical wave function? What is its ontological status? Is it *real*, in some sense (e.g. does it represent a real wave of some kind), or is it a mere *mathematical construct* with no one-to-one correspondence with anything existing in the world? Or is it something in-between? We also want to know the fuller significance of the apparent wave-particle duality or complementarity exhibited by quantum-mechanical systems. To answer one or more of the above questions is to interpret quantum mechanics.

The focus in this thesis will be on the general interpretative problem rather than on the measurement problem as such. Concentrating on the bigger picture is not without consequences for the smaller picture, however. One of the consequences is that it places the measurement problem in its proper context. That, in turn, will help us assess whether the measurement problem is simply an artefact of the standard interpretation.

Let us begin by taking a closer look at what interpreting quantum mechanics involves, and at some of the difficulties of doing so.

1.3.2 The meaning of the quantum wave function

'Well! I've often seen a cat without a grin', thought Alice; 'but a grin without a cat!'

(Lewis Carroll, Alice's Adventures in Wonderland)

At first sight, there seem to be at least three possibilities:

(a) The wave function describes a physical property of *each* individually existing system; or

(b) It is shorthand for the statistical properties of an *ensemble* (class) of systems; quantum mechanics describes the behaviour of a large number of particles: the wave function never represents a single particle. (Temperature provides a classical analogy as it is a property of the entire ensemble of molecules – it can be defined only for a very large number of molecules; the 'temperature of a single molecule' has no meaning.)

(c) The Schrödinger dynamics of the standard formalism needs to be amended in some way, either to eliminate the collapse or else incorporate it within the dynamics. Proposals that attempt these include Bohmian Mechanics and the Ghirardi-Rimini-Weber (GRW) scheme.

In this chapter we shall concern ourselves only with possibilities (a) and (b). We leave consideration of possibility (c) to Chapter 3 (§3.5).

The difficulty with (a) is that it is hard to see how the assumed physical property of the individual system can suddenly and discontinuously change upon measurement. For example, suppose that the particle described by the wave function is diffracted by passing it through a narrow slit. In that case, prior to measurement, the wave function can be spread out over a large area, implying, on interpretation (a), some kind of physical disturbance of an unknown nature over the entire area. Yet upon measurement, both the wave function and the presumed disturbance are instantaneously localized. Not only is the process of localization unexplained, but it sits uneasily with special relativity.

There is thus a temptation to say that the wave function describes (b) – the properties of an ensemble – thereby hopefully obviating the need to postulate a collapse of the wave function. Many authors describe quantum theory as a theory about properties of ensembles.[110] Now, ensembles can be of two general types:

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110 E.g. Zimmerman 1966; Hartle 1968; Ballentine 1970; Belinfante 1975; Thaller 2000.

(i) The ensemble may be an aggregate of quantum mechanical objects such as photons or electrons actually distributed in space; in this view $|\psi(r)|^2$ expresses the probability that some member of the ensemble possessing the property that is the subject of the proposed measurement is to be found at r.

(ii) The ensemble may simply be an *ensemble of measurements* of identically prepared systems; in this view $|\psi(r)|^2$ gives the *relative frequencies* of the results of the measurements on individual members of the 'ensemble'. An idealized case of an infinitely large collection is presupposed. In quantum theory, when it is postulated that there are no hidden variables, such an ensemble is sometimes called a 'minimal ensemble'; also 'statistical ensemble of states' (e.g. by Bohm). The different members of such an ensemble obviously cannot interfere with one another, and therefore the sudden replacement of the statistical ensemble of wave functions by a single wave function has no physical significance, representing no change in the underlying state of the system itself, but is analogous to the sudden change in classical probability function accompanying an improvement in an observer's knowledge.[111]

The difficulty of interpreting quantum mechanics in terms of ensemble of type (i) is that it cannot be extended to experiments involving single particles. But we have seen that experiments exhibiting interference with observable consequences can be carried out on single particles. Therefore ensemble (i) won't do. We are left with ensemble of type (ii). But ensemble (ii) is consistent with the wave function representing a single particle (given the results of experiments) only if we're willing to postulate either a collapse of the wave function after all, or else local 'hidden variables' of some kind applying to the individual system, analogous to the differing energies of the molecules of a gas at some particular temperature.[112] There are then two possibilities connected with an ensemble of type (ii):

(I) Relative frequencies of results are obtained upon measurements of systems represented by identical wave functions in the standard formalism because quantum mechanics is intrinsically probabilistic (the standard interpretation

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111 This last point is after Bohm 1951, p. 604.

112 Local 'hidden variables' are physical quantities that locally determine the state of an object inside an imaginary surface. Draw an imaginary surface around any object. The principle of local causality then asserts that any influence acting on the object must be due to either: (a) local changes in the state of the object itself, or (b) energy being transmitted through the surface to the object.

The notion of local hidden variables (and the discussion of locality generally) seems to presuppose that the world is spatially three-dimensional. In four dimensions, energy could easily be transmitted to the object without it ever going through the surface, owing to the extra degree of freedom.

of quantum mechanics[113] ['collapse of wave function but no hidden variables' view]); or

(II) Relative frequencies are obtained because systems represented by identical wave functions in the standard formalism can differ (the Einsteinian realist or statistical interpretation of quantum mechanics ['no collapse of wave function but hidden variables' view]). On this view, even the so-called 'pure ensembles' of the standard interpretation differ;[114] such ensembles are (on this view) always 'mixed ensembles'. E.g. if the wave function of a free particle is an eigenfunction of momentum, all members of the ensemble will have not only the corresponding value of momentum, but also a determinate value of position – though the latter values will all be different and not reflected in the wave function.

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[113] See e.g. Belinfante 1975, pp. 8, 14, 56, *passim*; Bohm 1951, pp. 602-4; Hartle 1968, p. 706; Whitaker 1996, pp. 284-6ff.

[114] A 'pure state' of an ensemble is said to exist, according to the standard interpretation, when every element of an ensemble of objects is in the same coherent superposition, i.e. described by the same wave function, which may be expressed as a coherent linear superposition $\Psi = \sum_n a_n \psi_n$, where the set of ψ_n are eigenfunctions (e.g. of energy). Upon measurement, the wave function collapses and one of the ψ_n is realized. 'Pure state is the quantum mechanical description of an ensemble, according to the standard interpretation, which also says that it takes a collapse, a reduction of the state vector, to discontinuously transform a pure state into a mixture of objects in various eigenstates. The actual composition of the mixture, upon measurement, depends on the statistical weights of the various eigenstates in the original coherent superposition.' (Goswami 1997, p. 211.)

According to the Einsteinian realist/statistical interpretation, the above quantum-mechanical conception of 'pure state' is an artefact of the standard interpretation. In reality (it is claimed), the so-called 'pure' state of the standard interpretation is always a mixture of states. Quantum mechanics simply determines the probabilities with which the different eigenfunctions occur in the mixture (and are picked out by measurement). There is never a collapse of the wave function because the wave function is not real. 'Hidden variables' obviate the need for both a real wave function and collapse alike. Quantum mechanics is merely a *statistical* theory (analogous to classical statistical mechanics) applying to large numbers of objects, never to individual objects. In particular, the wave function never describes a physical property of a single object.

The main technical difference between the two accounts of ensembles (the standard quantum mechanical account and the Einsteinian realist/statistical account) is that in the former, if our knowledge is limited to the statistical distribution of the eigenstates, e.g. energy among the objects of the ensemble, i.e. to the probabilities $P_n = |a_n|^2$, then the *phases* of the a_n remain indeterminate, whereas this ignorance is not the case in the latter. (Goswami 1997, p. 522.) Clearly, if the phase is unknowable even in principle, there can be no knowledge even in principle of 'the' actual state of a system (whatever that might mean then), but only of a superposition of possible states.

For discussion of pure and mixed ensembles, see e.g. Belinfante 1975, Ch. 1, also pp. 56-66, *passim*; Bohm 1951, pp. 602-4; Goswami 1997, pp. 286-9, 521-7; Hughes 1989, pp. 162-3; Whitaker 1996, pp. 210-17, 284-9; Wigner 1967, pp. 159-64.

The trouble with interpretation (I) is that it fails to explain how the relative frequencies of results come about upon many repetitions of the identical experiment if there is nothing more to be said – if *nothing* determines the outcome in any single run of the experiment. Why do the predicted probabilities always emerge given a sufficiently large number of repetitions of the experiment?

Interpretation (II) avoids *this* particular difficulty, because in it the wave function represents not physical reality but our *knowledge* of physical reality – which naturally undergoes a change when we make an observation. In this view, the so-called 'collapse' of the wave function simply represents a change in our knowledge of the underlying system (described statistically by the wave function – which of course needs to be rewritten as soon as we obtain additional information about the system).[115] For example, we may have obtained precise information about the state of a member of the ensemble, e.g. its location, which we had previously lacked, and the 'collapse' is nothing more than the change in the wave function necessitated by the incorporation of the new information into it. In this view the collapse is not dynamic, and ontologically the quantum probabilities are mere mathematical fictions – representing nothing more than a mathematical objectification of our ignorance of the complete state of the system. This is known as the *ignorance/knowledge interpretation* of quantum mechanics. In philosophical circles it is also known as the *epistemic interpretation*.

The ignorance interpretation enables one to argue that the measurement problem is merely an artefact of a particular way of interpreting quantum mechanics (the Copenhagen interpretation). In a more complete description of the physical world there must exist (so goes the argument) some kind of presently experimentally undetectable 'hidden variables', hard to characterize at the moment, but which account for the results of existing experiments and serve to maintain (ideally) both determinism and full counterfactual definiteness in quantum systems, or if that isn't possible, at least one of the two.[116] In particular, it might be maintained that quantum-mechanical objects such as electrons are always *corpuscular*, their supposed wavelike properties and the attendant indeterminacy being nothing more than a manifestation of our lack of knowledge of their underlying states.

Something like the ensemble interpretation was not only Einstein's view, but also that of Niels Bohr – the principal formulator of the Copenhagen interpretation. Leon Rosenfeld, who worked closely with Bohr for many years (and who was one of the major advocates of the Copenhagen interpretation after Bohr's

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115 Technically, as Leggett points out (1977, p. 105), by obtaining more information about a system we assign it to a new ensemble.

116 Neither determinism nor full counterfactual definiteness is necessary for a hidden variable theory. However, in the Platonic heaven in which Jack Smart's true theory of bicycle-riding resides with the sentences of our language, there is also a HV theory of quantum mechanics featuring both determinism and full counterfactual definiteness.

death), says:

> For Bohr, there was never any question [as to whether the wave function describes a single electron or only an ensemble]: it was obvious that we are talking of an ensemble, because statistics are made just for that. Probability implies a comparison of many similar cases with different outcomes. So there's no question; it's no problem.[117]

A *simple* or naive ensemble interpretation [ensemble of type (i)] is ruled out, however, as Bohr well knew, because the quantum wave function differs in several significant ways from a classical probability function describing an ensemble: (The first of these has already been mentioned.)

(1) The quantum wave function predicts not only the statistical behaviour of an *ensemble* of identically prepared systems, but also the *individual* behaviour of a single member of an ensemble, predicting for example whether a single photon will interfere with itself or not in some particular experimental situation. The presence or absence of self-interference can have a bearing on where the particle can and cannot be detected. (See below for details.)

(2) In quantum mechanics, measurement results not only in new knowledge being gained, but also in earlier knowledge being 'lost' in the following sense. The wave function provides a complete description of the quantum-mechanical system, and the wave function at later t is completely determined by its state at earlier t. Even so, later actual measurement of the value of some dynamical variable such as momentum is not redundant. The knowledge obtained by sharp measurement is a much stronger piece of information for the purposes of predicting the results of future measurements than merely the probabilities obtainable from the earlier wave function. In classical mechanics such later-obtained information would in principle be redundant if the earlier information of the system was complete. In quantum mechanics, the later knowledge supersedes the earlier knowledge. Any new prediction is made using the new wave function obtained from the actual measurement.[118] (See §2.1 and Appendix[e] for details.)

(3) For completeness, it needs to be mentioned here that a naïve ensemble interpretation is also ruled out by the existence of Bell's theorem and similar 'no-go' theorems. However, Bohr never had a chance to know of Bell's work, so we put discussion of them to one side (until Chapter 3).

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117 Buckley & Peat 1979, p. 28.

118 Schrödinger arguest in this connection that knowledge may be gained but cannot be lost. Therefore, he says, previously correct statements can only have become incorrect. Moreover, a correct statement can become incorrect only if the object to which it applies *changes*. (Schrödinger 1935a, in Wheeler & Zurek 1983, p. 159.)

For the above reasons, the quantum wave function cannot consistently be interpreted in terms of a simple probability distribution in the ignorance sense that underlies the usual understanding of ensemble properties, as exhibited for example in statistical mechanics and insurance statistics [ensembles of type (i)].[119]

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[119] An attempt has been made to evade the problem of choosing between I and II by, in effect, trying to have it both ways. In 1970 the physicist L.E. Ballentine argued that quantum mechanics is merely a statistical theory, in that, even on the microscopic level, it never applies to individual systems but only to ensembles of similarly prepared systems. He called this the 'Statistical Interpretation' of quantum theory. However, Ballentine describes his Statistical Interpretation in mutually inconsistent ways. He states (1970, p. 361) that 'it is most natural to assert that a quantum state represents an ensemble of similarly prepared systems, but does not provide a complete description of an individual system'. He goes on to say (p. 374) that the 'Statistical Interpretation... is completely open with respect to hidden variables. It *does not demand them* [my italics], but it makes the search for them entirely reasonable'. This is possible because, in contradistinction to the standard interpretation, Ballentine leaves open the possibility that the wave function is not a complete description of an individual quantum system. In this, he observes, the interpretation 'is rather like [the opinions] of Einstein' (p. 358). But the question immediately arises: How, then, *are* the states of individual quantum systems to be characterized? *Is* the wave function a complete description of an individual quantum system? If it turns out that it is, that there is nothing more to be added, then Ballentine's interpretation collapses into the standard one, which may *also* be characterised in terms of ensembles (see below). On the other hand, if it is not, then his position entails the existence of hidden variables (which are of course just what is to be added to the wave function description).

The latter possibility is the one Ballentine favours. Further along in his paper, he notes (1970, p. 380) that the statistical interpretation 'would seem to open the door for hidden variables to control individual events'. And in a later paper cited in d'Espagnat 1995, p. 298, he even unambiguously describes the interpretation as a hidden-variables position.

There is, of course, a hurdle that any hidden variable position needs to surmount, namely Bell's theorem (§§3.3-3.4). Ballentine states (1970, p. 380) that Bell's theorem poses a 'severe obstacle' for any hidden-variables position that reproduces exactly *all* of the predictions of QM. He points out a loophole, though. Bell's prohibition of hidden variables need not apply, he writes, to a theory which departs from the formalism of QM, and recovers it only in some limiting case. Therefore (he concludes his paper), '...the next step must be a bold departure from the familiar formalism, as Einstein's theory of gravitation departed from that of Newton' (1970, p. 380).

The trouble with Ballentine's position is that it starts out by purporting to be an interpretation of quantum mechanics, but then transforms mid-stream owing to perceived problems into an interpretation of some hypothetical future theory – a future theory, moreover, which needs to be *nonlocal* if it is not to fall foul of Bell. First, Ballentine explicitly claims that even though his statistical interpretation is not the standard interpretation, it *doesn't need* hidden variables, even though it is open to them. But of course it *does* if it is not to collapse into the standard interpretation (in which case, contrary to Ballentine, it could hardly be 'rather like' Einstein's position). Next, he seems to come round to the view that if his interpretation is to be distinguishable from the standard one, it does need hidden variables. (Whether or not he actually comes round to that view is irrelevant. The point is, his position is incoherent if he doesn't. In the absence of 'hidden variables' of *some* kind, the two cannot be distinguished, Ballentine's interpretation telescoping into the standard one just to the extent that it doesn't need hidden variables.) He also admits that Bell's theorem rules out

It is true that abrupt changes in mathematical quantities, analogous to the collapse of the quantum wave function, often take place in classical probability functions whenever new information concerning the state of the system is obtained. Insurance statistics such as the life expectancy function is an example. However, the life expectance function merely tabulates statistical information – information relating to an ensemble – and nobody supposes it to be in one-to-one correspondence with the actual length of any one person's life (as Bohm points out).[120] Statistics is used only because the complete details on each individual and the environment making up the ensemble are unavailable (even though they exist).

The variables that appear in quantum theory are in some ways analogous to classical statistical functions, in that the wave function predicts only the probability of individual events, and so, as with the classical statistical function, is not in one-to-one correspondence with the system being described. But there are two crucial differences. Contrary to the classical case, (a) orthodox quantum theory denies that the complete details on each individual and the environment even exist; and (b), it turns out that, up to a point, the quantum wave function *can* be applied to a *single* quantum object such as an electron, being in this regard unlike a classical statistical function. A single electron can be made to exhibit the property of self-interference in a variety of experimental situations, with observable consequences. For example, an electron can interfere with itself in a two-slit type experiment, provided both slits are open (§1.2[e]). This has a bearing on where it will end up on the detecting screen behind the slits. Some possible end-locations on the detecting screen, available to the particle when only one slit is open, are positively ruled out when both slits are open, namely those locations where the (hypothetical) waves from the two slits have arrived out of phase. It needs to be emphasized that this is so even in experiments involving only a single electron. Thus, the quantum wave function predicts not only the statistical behaviour of an *ensemble* of systems, but also (up to a point) the *individual* behaviour of a single member of an ensemble, being to this extent in one-to-one correspondence with the system being described, after all. (Such self-interference is also discussed in Appendix[d],[e],[k].) Bohm writes, in the context of the two-

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any (local) hidden variable interpretation of quantum mechanics as it is. But he doesn't seem to notice that Bell's theorem also rules out any *future* hidden variable modification of the formalism, in so far as the hidden variables are to be *local*.

In summary, then, the two mutually inconsistent positions embraced by Ballentine are: (1) The Statistical Interpretation is an interpretation of the quantum-mechanical formalism with no hidden variables – in which case it collapses into the standard interpretation, which is of course incompatible with Einstein's position; (2) The Statistical Interpretation is an interpretation of some future nonlocal hidden variables theory – in which case it is neither an interpretation of quantum mechanics nor compatible with Einstein's position, which hinged on locality and separability.

120 Bohm 1951, p. 126; see also pp. 602-4.

slit experiment:

> [T]he wave function differs from a classical probability function in the important respect that before interference has been destroyed by the actions of a suitable measuring apparatus, the wave function cannot consistently be interpreted in terms of a simple probability... As long as definite phase relations between $\psi_A(x)$ and $\psi_B(x)$ exist [where $\psi_A(x)$ and $\psi_B(x)$ are, respectively, the parts of the wave function that have come from a pair of slits A and B], the electron is capable of demonstrating the effects of interference and acting as if it passed wave-like through both slits simultaneously. A sudden collapse of the wave function would, therefore, at this time represent a real change in the physical state of the electron (from a wave-like to a particle-like behaviour); ... absurd results would follow if such abrupt changes in the wave function could be brought about simply by an improvement in an observer's information about the electron... This means that to the extent that definite phase relations exist between $\psi_A(x)$ and $\psi_B(x)$ [i.e. before measurement has been made], the wave function is in a closer correspondence with the state of the electron than it would be if it were a simple classical probability function, specifying the likelihood that the electron goes through either of the slits. Nevertheless, the degree of correspondence between the wave function and the actual behaviour of the electron is always less than that aimed for by the dynamical variables of classical mechanics.[121]

It is clear that not only the *amplitude* but also the *phase relations* between various parts of the wave function have physical significance. It is the phase relations that give rise to the interference effects that are characteristic of quantum systems, and which are the causes of all the counterintuitive aspects of quantum mechanics (such as those exhibited in the two-slit experiment, in which the phase relations determine the interference pattern obtained on the detecting screen). For this reason, changes in the quantum wave function are not brought about simply by an improvement in the observer's information or *knowledge* of the object,[122] at least if the object is to be interpreted as a classical one, and therefore the quantum wave function cannot be a classical probability function in an ordinary ensemble sense.

The situation as regards the reality of the wave function then seems as follows. On the one hand, the wave function assigns only probabilities to the location on the detector screen where the particle will be detected – in this sense clearly describing an ensemble. On the other hand, by predicting whether interference will take place or not in the case of the individual particle (which has a bearing on where it will be later detected), the wave function describes a single entity under certain experimental conditions. Hence, the wave function seems to encompass in varying degrees *both* of the two mutually exclusive possibilities (a) and (b) described in the beginning of the present sub-section.[123]

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121 Bohm 1951, p. 127.

122 After Bohm 1951, pp. 126-7.

123 These were: (a) the wave function describes a physical property of *each* individu-

Therefore it must be an ensemble of type (ii), but with more to be said. That is a situation not readily explainable in terms of classical probability. The very word 'probability' seems to be used in quantum mechanics in a different sense than the way it is used in classical mechanics and everyday life.[124]

In view of the above, quantum mechanics appears to be a generalization of *both* statistical mechanics and classical mechanics. Hence the attempt by Bohr to make sense of it by way of his notion of complementarity [ensemble of type (ii)(I)]. Hence also the divergence of Bohr's views from those of Einstein, even though both believed that the quantum wave function represents an ensemble in some sense of the term. For Bohr, there was no point in seeking hypothetical 'hidden variables' that were missing in the statistical account given by quantum theory, since such an account was already complete and perfectly consistent without them – if one simply assumed that the classical dynamic properties of objects *did not exist* on the quantum level when unmeasured.

Einstein, on the other hand, believed that one could go deeper, citing the inability of quantum theory to account, e.g. for the definite transformation time of an individual atom, such as its radioactive decay. *Within* the Copenhagen interpretation, one was prohibited from even *asking* for the exact time of decay prior to an observation, the question being held meaningless.[125] But suppose, argued Einstein, that the individual atom nevertheless *does have* a definite disintegration time, contrary to the Copenhagen interpretation.[126] For example, one could arrange things so that a particle produced upon disintegration is detected by a macroscopic instrument such as a particle detector with an automatic registration mechanism. Upon detection, the detector makes a macroscopic mark (using a tripping mechanism) on a strip of paper continuously passing through the detector at constant speed. The position of the mark on this strip will correspond to the time of decay. Yet according to the Copenhagen interpretation, the existence of a definite position for the *mark itself* depends on an observation of the strip. *Prior* to observation, the theory offers only relative probabilities for the position of the mark. Since the theory is claimed to be complete, it follows from the theory that a definite position for the mark cannot be said to exist prior to observation. But that would be an unnatural *physical* interpretation (even if not absurd from a purely *logical* standpoint), since we're now dealing with a system (the mark on a strip of paper) that is in the *macroscopic* sphere. In the macro-

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ally existing system; or (b) it is shorthand for the statistical properties of an *ensemble* (class) of systems; quantum mechanics describes the behaviour of a large number of particles: the wave function never represents a single particle.

124 As e.g. both Gamow (1988, p. 258) and Bell (1987, p. 112), have noted. Bell, for instance, refers to the fact that probability in classical physics is only used 'to take account of uncertainty in initial conditions'.

125 Einstein 1949b, p. 669.

126 Fine calls this a 'preliminary skirmish in Einstein's battle to have the quantum theory seen as an incomplete description of quantum systems' (1986, p. 92).

scopic sphere, says Einstein, it is considered certain that one must adhere to a realistic programme of description in space and time.[127] The difficulty disappears, however, if one accepts (as regards the individual atom) that the description by means of the wave function is an incomplete one, being instead the description of an ensemble of systems [of type (ii)(II)], with the consequence that quantum mechanics is incomplete.[128]

> For if the statistical quantum theory does not pretend to describe the individual system (and its development in time) completely, it appears unavoidable to look elsewhere for a complete description of the individual system; in doing so it would be clear from the very beginning that the elements of such a description are not contained within the conceptual scheme of the statistical quantum theory. Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position

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127 Einstein credits Schrödinger as the originator of the present kind of argument against the Copenhagen interpretation, i.e. argument that relies on thought-experiments which extend the supposedly purely quantum properties of systems into the macroscopic domain (cf. Schrödinger's cat).

128 Einstein regarded a physical theory as incomplete unless every element of the physical reality described by the theory had a counterpart in the physical theory. Suppose, for the sake of argument, that a particle really does possess both a definite energy and disintegration time as simultaneous elements of physical reality. In that case quantum mechanics clearly fails to satisfy this criterion, since the wave function can specify, at most, only one of these components with complete precision. (We shall go into more detail in Chapter 3.)

It may be worth pointing out, in connection with the above discussion regarding the disintegration time of an individual atom, that two questions are easily conflated: Qn 1: Does a particle possess both a definite energy and disintegration time as simultaneous elements of physical reality?; and Qn 2: Can energy be measured reproducibly in an arbitrarily short time? Einstein is concerned with the former question. As regards the latter, Aharonov & Bohm showed in 1961 on the basis of theoretical considerations that energy can be measured reproducibly in an arbitrarily short time. For some discussion, see Jammer 1974, pp. 148-50. Schulman (1997, p. 246) writes, 'In 1930 you weren't supposed to ask what happened; in 1986 you could point [on the basis of experiment] to the moment at which a single atom made the transaction.'

In their 1961 *Physical Review* paper, Aharonov & Bohm distinguished between two kinds of time in quantum mechanics – an 'outer' time and an 'inner' time. The former is simply the conventional time parameter t – the time imposed on the system from outside the system itself, e.g. as read from a laboratory clock. The 'inner' time of a system, in contrast to the 'outer' time, is the time that is subject to the indeterminacy relation $\Delta E \Delta t \geq h/4\pi$, where ΔE is the indeterminacy of the energy of the system. Δt is, in effect, the lifetime of states in that system. The authors showed that there is no indeterminacy relation between the *duration* of measurement and the *energy transfer* to the observed system. The energy-time uncertainty relation are often misinterpreted and the two kinds of time conflated.

The fact that energy can be measured reproducibly in arbitrary short time does not of course resolve the issue between Einstein's realist interpretation and the Copenhagen interpretation, since that concerns the status of *unobserved* systems. Moreover, given hidden variables, the distinction between 'inner' and 'outer' time is an artefact of the standard interpretation. In that case, the inner time is then just the average time of e.g. decay of an ensemble of identically prepared unstable systems, no different in principle from that appearing in a classical life-expectancy function.

> to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult.[129]

It was for such reasons that Einstein always clung to something like the classical ensemble conception of the notion of probability (essentially, the *ignorance* interpretation),[130] notably first in 1905 when he proposed his light-quanta hypothesis and tried to reconcile it with Maxwell's classical theory of electromagnetic waves. He initially thought he had managed both the reconciliation and the retention of the classical concept by interpreting $\overline{\varepsilon^2}$,[131] which in electromagnetic theory is proportional to the radiant energy in a unit volume, as a measure of the *average number of photons* per unit volume: that is, $\overline{\varepsilon^2}$ is a probability measure of photon density. However, he was mistaken in this.[132] Though Einstein continued to hold an ensemble interpretation to the end of his life, nowhere, as Fine emphasizes, did he spell out just what this ensemble interpretation amounted to.[133]

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129 Einstein 1949b, p. 672.

130 Though it would be a mistake to regard Einstein as a hidebound classical thinker in his conception of quantum mechanics. See e.g. Fine 1986, Ch. 6; also §3.5 of the present work.

131 $\overline{\varepsilon^2}$ is the average value over one cycle of the square of the electric field strength of the wave. On the wave picture, the intensity of the radiation, I (the average value of the Poynting vector) is proportional to $\overline{\varepsilon^2}$ (Resnick 1972, pp. 176-7.)

132 The connection between the density of photons in a field of electromagnetic radiation and the square of the electric field vector is analogous to the connection between the probability density of electrons and the absolute square of the amplitude of the associated matter wave. However, there is a difference. It is that in the case of the electromagnetic field, the *intensity* of the wave (defined as the mean over time of the square of the wave amplitude), directly gives the probability density, whereas in the case of matter waves, one needs to take the absolute square (obtained by multiplying the amplitude by its complex conjugate). The difference arises because the electric field vector is real, whereas the quantum wave function is complex.

Moreover, it can be shown (Dirac 1935, p. 9) that the wave function gives information about the probability of *one* photon being in a particular place, not about the probable *number* of photons in that place. See Appendix(d) for details.

The lesson is, I think, that even when we go to an ensemble and consider interference phenomena involving electromagnetic radiation (when a large number of photons are involved), there is nothing straightforwardly *classical* about what is going on beneath the possibly classical results obtained. That is because even in the case of an ensemble of photons, each photon interferes only with itself, never with another photon. Averaging owing to the large number of photons involved simply tends to mask the nature of the utterly non-classical underlying process.

Newton's attempts to understand the nature of partial reflection of light by two or more surfaces are instructive in this regard. Even today, writes Feynman, 'we haven't got a good model to explain partial reflection by two surfaces; we just calculate the probability that a particular photomultiplier will be hit by a photon reflected from a sheet of glass'. (Feynman 1985, p. 24.)

133 Fine 1986, p.41.

Max Born followed Einstein's early lead by proposing in 1926 a similar uniting probability conception of the wave-particle duality of matter (essentially also an 'ignorance' interpretation), in which, as we have seen, $|\Psi|^2$ is the probability density of particles of matter. Born had been impressed by the corpuscular aspects of particle collision experiments, and for that reason the corpuscular or *particle* aspect of matter was the primary reality in his initial conception, in contrast to Schrödinger's own interpretation at that time of the *wave* as the primary reality.[134] In Born's initial conception, the quantum probability wave was a kind of 'phantom field' whose waves guided the corpuscular particles in their paths, in the sense that the intensities of the waves (squared amplitudes) determined the probabilities of the presence of particles. 'Just as the intensity of light waves was a measure of the density of light quanta, Born argued, "it was almost self-understood to regard $|\Psi|^2$ as the probability density of particles".'[135]

Born's initial interpretation of the wave function underwent a conceptual change soon after he proposed it, because the corpuscular ensemble interpretation broke down when applied to individual particles rather than swarms of them.[136] It did so because it was unable to account for the observed interference effects when the wave fields were superposed: the mathematical interference is manifested in a physical distribution of particles on a detector-screen (they are distributed in an interference pattern) – suggesting to Born that the wave function is physically real in some sense, and not just a representation of our knowledge, at least in any ordinary way. In Born's revised conception, both the wave and particle aspects of matter have the same ontological status, and waves and particles are equally 'unreal'/'real'.[137] But even though Born had to revise his

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134 Even as late as 1953 Schrödinger continued to favour a wave interpretation of reality, writing, 'The wave v. corpuscle dilemma is supposed to be resolved by asserting that the wave field merely serves for the computation of the probability of finding a particle of given properties at a given position if one looks there. But once one deprives the waves of reality and assigns them only a kind of informative role, it becomes very difficult to understand the phenomena of interference and diffraction on the basis of the combined action of discrete particles. It certainly seems easier to explain particle tracks in terms of waves than to explain the wave phenomenon in terms of corpuscles.' (Schrödinger 1953, p. 6.)

135 Jammer 1966, p. 285.

136 Corpuscular ensemble interpretations are familiar in statistical mechanics and the kinetic theory of gases.

137 Born initially supposed that the intensity (amplitude squared) of Ψ at any space-time point was proportional to the probability of a particle being situated at that point, and that this gave information about the number of particles situated at that point (essentially Einstein's statistical ensemble conception of the relation between photons and electromagnetic radiation). Such a view is subject to two difficulties:

(a) It was found that the wave function could only give information about the probability of *one* photon being in a particular volume and not about the probable *number of photons* in that volume (for details, see Appendix[d]); and

(b) it presupposed that the particle always *has* a sharply defined position, even when initial interpretation, the *probabilistic* basis of his interpretation has never changed, and remains orthodoxy today. He was eventually awarded the Nobel Prize for it (for 'his fundamental work in quantum mechanics, and especially for his statistical interpretation of the wave function', as the official declaration stated).

Unlike Einstein, Born regarded probability in relation to the wave function as more than a useful mathematical fiction. He believed that probability itself had to be endowed with some kind of physical reality because it propagated in space as a wave and evolved in time in accordance with Schrödinger's equation. On the other hand, though, it did not transmit energy or momentum. Since according to classical physics, only that which can transmit energy and momentum is real, the probability wave was not quite real either, but possessed an intermediate kind of reality, occupying a sort of 'no-man's land' in ontological space.[138] Born wrote in 1935:

> Experiments show that the waves have objective reality just as much as the particles – the interference maxima of the waves can be photographed just as well the cloud-tracks of the particles. There seems to be only one possible way out of the dilemma; a way I have proposed which is now generally accepted, namely the *statistical interpretation* of wave mechanics. Briefly it is this: *the waves are waves of probability*. They determine the 'supply' of the particles, that is, their distribution in space and time. It follows that the waves, apart from their objective reality, must have something to do with the subjective act of observation.[139]

Born's probabilistic interpretation of matter waves and the wave function provides a much-needed link between the wave and particle pictures of matter. (Whether that link is sufficient to make us desist from seeking a more satisfying picture of the microworld is another matter. That is just the point of difference between Einsteinian realists and proponents of the standard interpretation.)

For Werner Heisenberg too, who had quickly accepted Born's interpretation, the waves were more than just a mathematical fiction. Heisenberg, a recipient of a classical education, conceived of the probability waves, as he later explained (in 1961), as

> a quantitative formulation of the concept of... possibility, or in the later Latin version, *potentia*, in Aristotle's philosophy. The concept that events are not determined in a peremptory manner, but that the possibility or 'tendency' for an event to take place has a kind of reality – a certain intermedi-

unobserved. In Born's revised view, owing to the ideas of Bohr and Heisenberg, the particle only has a sharply defined position *immediately after a position-observation has been made*. Prior to such observation, it was supposed to be meaningless to speak of the particle possessing a position at all. (After d'Abro 1951, p. 652.)

138 After Jammer 1966, p. 286.

139 Born 1935, p. 157.

> ate layer of reality, halfway between the massive reality of matter and the intellectual reality of the idea or the image – this concept plays a decisive role in Aristotle's philosophy. In modern quantum theory this concept takes on a new form; it is formulated quantitatively as probability and subjected to mathematically expressible laws of nature.[140]

The new probabilistic basis of quantum mechanics resulted in a new conception of the laws of nature. Laws determined not the occurrence of an event, but the *probability* of its occurrence. As Born famously put it in 1926,

> The motion of particles conforms to the laws of probability, but the probability itself is propagated in accordance with the law of causality.[141]

1.3.3 Non-separability of quantum systems

Another characteristic feature of the description of quantum mechanical objects is that of non-separability, or *entanglement*. Quantum mechanics predicts an essentially non-classical correlation of the observable properties of separated objects that have once been part of a single system. Until the occurrence of a pair of incompatible measurements on the system (which breaks the entanglement), two (or more) objects that have once interacted remain described by a single wave function, and are said to be in an 'entangled' state. The wave function describing the state of the larger system of both objects is one that is a function of both sets of coordinates and it cannot be written as a product of the separate functions of those coordinates. This has physical consequences. For example, a measurement of the spin along some arbitrarily selected axis of one member of a pair of widely separated spin-half particles in an entangled state ensures that the spin of the other member measured along the same axis will be found to be perfectly anti-correlated to that of the first (e.g. if, say, the x axis spin of the first particle measured at location **A** is 'up', then the x axis spin of the second particle will be found 'down' at location **B**, and vice versa). The combined spin-state of both particles is reduced instantaneously from the original entangled state (in which the formalism entails that neither particle by itself has a well-defined state of spin) to a disentangled state (in which both spins are well-defined). That is, a measurement of the spin of *one* member of the pair collapses the wave functions

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140 Heisenberg 1961, cited in Jammer 1966, p. 287. In his book *Physics & Philosophy* (1958, p. 160), Heisenberg wrote, 'In the experiments about atomic events we have to do with things and facts, with phenomena that are just as real as any phenomena in daily life. But the atoms or the elementary particles themselves are not as real; they form a world of potentialities or possibilities rather than one of the things or facts.'

This kind of view, in which matter has only a *potential* to show its (classical) properties when placed in an appropriate experimental situation, appears to have been first put forward by Bohm, even though it tends to be associated with Heisenberg. See Bohm 1951, pp. 132-3, 138-9, *passim*. See also Bohm & Hiley 1993, p. 18n, who express a similar opinion.

141 Born 1926, p. 804.

of *both*.

The collapse and the ensuing correlation of observables are both nonlocal, given the standard interpretation. According to that interpretation, the act of measurement actually brings the property of a well-defined spin into existence (in the jargon, it 'actualises the potentiality for the realization of the property'), not only for the particle whose spin is measured but also for the distant particle. Furthermore, any measurement of the value of some property of either member of the pair of entangled particles, say the x axis spin, destroys the value of any previously measured and therefore known *non*-compatible property of the particle being measured, such as its y axis spin (presumably, it 'de-actualises the potentiality...'). Analogous considerations apply to properties such as position and momentum.

The correlation appears to be contrary to at least one of two central tenets of Einsteinian local realism as held by Einstein himself in 1935 (here extrapolated to the spin case):

(a) Both particles possess definite spin components along every axis as simultaneous elements of physical reality.[142]

(b) The reality of the physical properties of one member of the pair does not depend on what is done to the other when they are spatially separated.

The existence of a spin-correlation between pairs of entangled particles emitted from a common source might suggest that we can use it to send information between two distant locations at a speed faster than light, by having in place a constant stream of such pairs, and 'modulating' the sequence of the measured values of the spins at one location, which modulation must be instantaneously reflected in the sequence of the measured values of the spins at the other location. A non-random selection of spin axes for measurement could constitute such a modulation owing to the entanglement.[143] That's because, as we saw above, a selection of a spin axis, e.g. x, y, or z, for a measurement of spin at **A** brings the property of a well-defined spin into existence not only at **A** but also at the distant location **B** along the selected axis. By the same token it destroys the value of

142 Although this tenet is really an intuitive postulate, it was derived by Einstein, Podolsky and Rosen in their famous 1935 paper from other assumptions (some of which are questionable, however). The paper attempted to show that quantum mechanics is incomplete. We shall take a close look at the 'EPR' argument in Chapter 3.

143 E.g. certain sequences of selected detector settings might stand for the various letters of the alphabet, or simply for 'yes' and 'no'. For simplicity, the detector at **B** may be left at a fixed setting agreed upon beforehand. We are assuming an idealized experiment in which there is a constant stream of singlet-state particles from a source somewhere between **A** and **B**, efficient detectors, and that the message is sent repeatedly.

any previously measured and therefore known non-compatible property of the pair, in this case their spins along a non-compatible axis, not only at **A** but also at **B**. To put it another way, use is made of the fact that each member of a particle-pair arriving at the spin detector at the distant location **B** somehow 'knows' both (i) the relevant detector setting at **A** (i.e. the axis along which the spin of the other member of the pair was measured at location **A**) and (ii) the value of the spin obtained (either 'up' or 'down' along that axis) – and instantly adopts the appropriate spin to reflect such settings so as not to violate the law of conservation of angular momentum.

It turns out, however, that the connection between the entangled objects is not of a kind that allows a physical signal to be sent utilizing the connection.[144] The reason is that each pair is emitted from the common source with *individually random,* though correlated, spin.[145] Consequently, when a 'message' is encrypted into a sequence by varying the spin-detector settings in the above way, all that is achieved is to alter one random sequence of spin-readings into another random sequence.[146] (The knowledge that certain sequences of selected detector settings stand for the various letters of the alphabet is not sufficient.) There is no way to extract the information about the variation in the settings (constituting the message) from examining only the *one* record of readings. Only by *comparing the two records* can one tell that there have been variations in the detector settings: only then can the 'message' be read.[147] The would-be superluminal communicators at **A** and **B** must get together and compare notes – which means that they are unable after all to communicate at speeds faster than light.[148] Pagels writes,

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144 See e.g. Shimony 1989, p. 388; Home 1997, p. 258.

145 We are using here for descriptive convenience a semi-classical picture in which it is possible to talk of the spins of the individual particles, even if these are random. But such a picture can be misleading, for the standard interpretation the spin state of the two particles is irreducibly a state of the two considered as a *composite* system. As Lockwood notes (1996, p. 162), the state can't be equated with *any* combination of spin states of the two particles considered individually (which would always be of the form $|\psi_1\alpha\rangle|\psi_2\beta\rangle$, say $|\psi_1\uparrow\rangle|\psi_2\downarrow\rangle$, as opposed to the required form as depicted in $|\psi_0\rangle = 1\sqrt{2}\left(|\psi_1\uparrow\rangle|\psi_2\downarrow\rangle - |\psi_1\downarrow\rangle|\psi_2\uparrow\rangle\right)$). The notation $|\ \rangle$ refers to Dirac kets, signifying a wave function, and $|\psi_1\alpha\rangle|\psi_2\beta\rangle$ is the tensor product of the two states. See Appendix(d) for details.

146 In the technical language of cryptology, the information about the detector settings (selection of spin-axes for measurement) contained in the cross-correlation of the two spin sequences appears to correspond to encryption using a Vigère cipher using a random key. A consequence of each pair of particles being emitted with random spin from the original singlet state is that the 'key' for deciphering the ciphertext (the message) at one location is knowledge of the entire sequence of measurement results (the ciphertext or message) at the other location.

147 For discussion, see Pagels 1982, pp. 174-6, 183-4. See also Maudlin 1994, Ch. 6; Home 1997, pp. 258-65.

148 'And let no one use the Einstein-Podolsky-Rosen experiment to claim that information can be transmitted faster than light...', is how J.A. Wheeler put it in a lecture to

> We conclude that even if we accept the objectivity of the microworld then Bell's experiment does not imply actual nonlocal influences. It does imply that one can instantaneously change the cross-correlation of two random sequences of events on other sides of the galaxy. But the cross-correlation of two sets of widely separated events is not a local object and the information it may contain cannot be used to violate the principle of local causality.[149]

Pagels is right in that the would-be superluminal communicators are unable to communicate faster than light, for the reason he gives. Albert, too, emphasizes that the cross-correlation cannot possibly be exploited to transmit a detectable signal, or to carry information, nonlocally, between any two distant points.[150] Quantum mechanics possesses *signal locality* in this sense.[151] Accordingly, I shall characterize quantum mechanics as a *weakly nonlocal* theory, to distinguish it from hypothetical *strongly nonlocal* theories that might countenance actual superluminal communication. But contra Pagels, it doesn't follow that the cross-correlation does not violate the principle of local causality. Whether it does or doesn't depends on (a) our definition of 'local causality' and (b) our interpretation of quantum mechanics. Take (a). There are many who would say that, *given* the objectivity of the microworld, the instantaneous change in the cross-correlation of two random sequences of events on other sides of the galaxy *ipso facto* entails violation of local causality, thereby entailing some kind of nonlocal 'influence', even if the two sequences do need to be brought together to establish the cross-correlation (and thus the violation etc.). As for the fact that the violation can't be used for superluminal communication, it's argued that that's irrelevant: the change in the cross-correlation is sufficient to establish violation of local causality. That then leads to (b) our interpretation of quantum mechanics. *Do* we accept the objectivity of the microworld, and if so, in what precise sense? Quantum nonlocality, signal locality and local causality are closely connected notions. Accordingly, even though quantum mechanics is only a weakly nonlocal theory in every known interpretation (in the above sense), the *degree* of weakness ascribed to the nonlocality may be said to vary from interpretation to interpretation in proportion to the degree of objectivity each ascribes to the microworld, i.e. just to the degree that quantum mechanics violates local causality in the interpretation.

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the American Association for the Advancement of Science in 1979. (Gardner 1981, p. 189.)

149 Pagels 1982, p. 176. The principle of local causality, in its usual form, states that all the causes of an event lie in its past light cone. (Maudlin 1994, p. 90.) However, this presupposes the absence of backward causation. In particular, it presupposes that the causes of an event cannot lie in *both* its past and future light cones – a possibility that we'll examine in the second half of the thesis.

150 Albert 1992, p. 72.

151 At least, it does so in any interpretation in which it is impossible to distinguish between non-orthogonal eigenstates of a non-Hermitian operator – which isn't permitted in the standard treatment of measurement. (Home 1997, p. 260.)

Home puts it as follows:

> [W]hatever nonorthodox approach we adopt to describe individual quantum events realistically, either by introducing the notion of so-called hidden variables (which may or may not have ontological significance) or by providing a dynamical description of wave function collapse as an actual physical process, an inevitable action at a distance (entailing superluminal causal influence) is implied by such a scheme.[152]

The interpretation with the weakest nonlocality is quantum mechanics in Bohr's original Copenhagen interpretation. It would be hard to argue that quantum mechanics in this interpretation violates local causality, as according to it a quantum-mechanical system has no intrinsic dynamical properties whatsoever. It follows that there are no dynamical properties to be *changed* nonlocally by a measurement (see §3.2). The class of interpretations with the strongest (weak) nonlocality is quantum mechanics in expressly realist and expressly nonlocal interpretation such as David Bohm's causal interpretation. This interpretation does appear to violate the principle of local causality (see §3.5). The standard interpretation is somewhere in between. Some investigators, such as Pagels, and Herbert[153], argue that its nonlocality does not violate local causality, while others, such as Maudlin and d'Espagnat,[154] argue that it does – even though the violation can't be used for superluminal communication (in the sense identified by Pagels above).[155] However, the difference of opinion seems to be more verbal than real, reflecting little more than different usage of terms such as 'superluminal signalling'/'–transmission of information'/ '–influence'/ '–causal connection', and 'local causality', together with some particular stance on the Einstein-Bohr debate. Bell himself briefly considers the possibility of superluminal messages given his own theorem together with the assumption that quantum field theory is embedded in a certain way in a theory of hidden 'beables'.[156] Could we *then* signal faster than light? The answer, according to Bell, would depend, among other things, on what *we* as humans could do – e.g. to what precise extent could we control the beables? The answer, then, depends on precisely what is meant by 'locally causal'. In the particular illustration he gives, faster than light signalling is not possible even given the spacelike EPR/Bell correlations. Even so, he else-

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152 Home 1997, p. 262.

153 Herbert 1988, pp. 159-181. Herbert's reason for maintaining that local causality is not violated is essentially the same as that of Pagels.

154 Maudlin 1994, Chs 4-6, e.g. pp. 98, 154, 186; d'Espagnat 1995, pp. 124-7.

155 Indeed, Maudlin (1994) mounts a determined attempt to show that quantum mechanics in even Bohr's original Copenhagen interpretation entails violation of local causality. The attempt fails, as I show in §3.2.

156 Bell 1987, Ch. 7. 'Beables' refer to hypothetical unobservable elements of a realist description. The contrast is with 'observables'. The idea is, as Bell puts it, that future developments might make it again 'possible to say of a system not that such and such may be *observed* but that such and such *be* so. The theory would not be about "*observables*" but about "*be*ables"'. (Bell 1987, p. 41.)

where notes that

> It is as if there is some kind of conspiracy, that something is going on behind the scenes which is not allowed to appear on the scenes.[157]

We shall take a closer look at EPR/Bell and the question of the violation of local causality in Chapter 3.

Within the framework of the standard version of the Copenhagen interpretation, it remains a mystery how a pair of objects remain in contact, each 'knowing' how to respond to changes in the state of the other, even when separated by spacelike distances.

The complementarity and entanglement exhibited by quantum systems seem to require that we give up the notion that any object has, by itself, any *intrinsic* properties at all. It would seem that the classical properties of a given system exist only in an imprecisely defined form, and in a more accurate description are not properties at all but mere *potentialities*, a view proposed by Bohm in his 1951 exposition of the standard interpretation. The incompletely defined potentialities of each object are more definitely realized in interaction with an appropriate classical system such as a measuring apparatus. For example, consider two non-commuting observables such as the position and momentum of an electron.

> We say that, in general, neither exists in a given system in a *precisely* defined form, but that both exist together in a roughly defined form, such that the indeterminacy principle is not violated. Either variable is potentially capable of becoming better defined at the expense of the degree of definition of the other, in interaction with a suitable measuring apparatus. We see then that the properties of position and momentum are not only incompletely defined and opposing potentialities, but also that in a very accurate description, they cannot be regarded as belonging to the electron alone; for the realization of these potentialities depends just as much on the systems with which it interacts as on the electron itself. This means that there are actually no precisely defined 'elements of reality' belonging to the electron. Thus, we contradict the assumptions of... Einstein, Rosen and Podolsky.[158]

Bohm emphasizes that such a view contradicts the classical assumption that the universe can correctly be regarded as made up of distinct and separate parts that work together according to exact causal laws to form the whole. The difficulty is that in quantum theory none of the properties of these parts can be defined, except in interaction with other parts. Moreover, different kinds of interactions bring about the development of different kinds of 'intrinsic' properties of the so-called 'parts'.[159] In general then, as Hermann Weyl put it, 'the whole is always more, in the sense of being capable of much greater variety of wave

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157 Cited in Home 1997, p. 258.

158 Bohm 1951, p. 620.

159 Bohm 1951, pp. 139-40.

states, than the combination of its parts'.[160]

The wave function appears to be an abstraction, describing the propagation of correlated potentialities. It provides 'a mathematical reflection of certain aspects of reality, but not a one-to-one mapping'. To obtain a description of all aspects of the world, one needs to supplement the mathematical description with a physical interpretation in terms of incompletely defined potentialities.[161] Thus, quantum mechanics seems to have radical implications for our traditional way of modelling the world. In particular,

> It seems necessary, therefore, to give up the idea that the world can correctly be analysed into distinct parts, and to replace it with the assumption that the entire universe is basically a single, indivisible unit.[162]

Such a view represents a radical departure from virtually all traditional philosophical conceptions of the nature of reality. (We shall return to quantum entanglement and its philosophical implications in Chapter 3.)

1.4 A procrustean choice

Procrustes, you will remember, stretched or chopped his guests to fit the bed he had constructed. But perhaps you have not heard the rest of the story. He measured them up before they left next morning, and wrote a learned paper 'On the Uniformity of Stature of Travellers' for the Anthropological Society of Attica.

(A.S. Eddington, The Tarner Lectures 1938)

The difficulty of a straightforward realist interpretation of quantum mechanics lies in the procrustean nature of the choice awaiting the would-be realist, necessitated by the peculiar behaviour of quantum systems. The realist is of course always free to suppose that quantum systems possess the full complement of classical properties, instantiated by 'hidden variables' of some kind. But if he does he is liable to find himself publishing learned papers in which special thanks will need to be extended to Procrustes as regards the methodology. Some of the reasons for this we've already seen; for others, see Chapter 3. Take David Bohm. He developed a new deterministic realist theory, Bohmian Mechanics, which was further developed by Bell. The development of the Bohmian model was valuable because of the insights gained from it concerning both what is and what is not possible. But to actually *replace* the standard model by the Bohmian one would not be sound methodology. *Then* special thanks would need to be extended to Procrustes as regards the methodology. And as we shall see below, the standard Copenhagen theorist is no better off. Nor are his non-standard successors, as will become evident in §3.5. To make matters worse, the procrus-

160 Cited in Barrow & Tipler 1986, p. 503.
161 Bohm 1951, pp. 621-2.
162 Bohm 1951, pp. 139-40.

tean choice to be made is not even unique. There are many different major interpretations of quantum mechanics in each of the two main categories (Copenhagen and realist), each of which represents a serious attempt to describe what is 'really' going on. It would seem that no one of these interpretations can claim experimental refutation of the others because the positions are experimentally indistinguishable, at least to date. That is because all have been carefully constructed so as to be consistent with the available experimental data.[163] Even the Copenhagen interpretation itself is not a single, clear-cut set of ideas or philosophical positions, as e.g. Jammer points out, but rather a common denominator for a variety of related points of view.[164]

In view of the present experimental equivalence of the competing interpretations, is there any reason to favour any one interpretation over the others?

The advocates of the Copenhagen interpretation remind us that experiment shows that whatever the nature of deep reality may be, it *cannot be local,* locality being ruled out by quantum entanglement – that characteristic feature of quantum mechanics that enforces its departure from classical thought. They also point out that special relativity forbids faster-than-light transmission of signals and information.[165] Therefore it is realism that must be given up.

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[163] Herbert 1985, p. 28.

[164] Jammer 1974, p. 87.

[165] Tim Maudlin disagrees. He argues that this claim is often made without justification and accepted without demur (1994, p. 99). According to him, the choice is not between relativity and superluminal signalling as such. There is nothing about superluminal signalling *per se* that's incompatible with the relativistic account of *space-time structure* (by which he seems to mean its metrical structure [see 1994, pp. 230-1]), provided that one is willing to countenance signal loops (1994, p. 115), and more generally, causal loops, in the sense of effects preceding their causes in some Lorentz frames (p. 101). [In a slip, Maudlin writes 'causes preceding their effects', but his intent is clear]). The choice is thus between relativity and superluminal signalling of a certain *kind,* namely one that countenances causal loops, and so possible causal paradox (p. 113).

The *important* question as regards relativity, according to Maudlin, is whether the superluminal transmission phenomenon being investigated would force us to pick out a particular Lorentz frame as holding a privileged position in nature. If so, *then* (and only then) a fundamental relativity principle would be violated (p. 102). The answer to *that,* however, will depend on the details of the superluminal transmission, on the exact connection between the emission of the signal and its reception (p. 102). He proceeds to illustrate just what he means by examining three purely hypothetical cases of transmission of superluminal signals. All involve relaxing the principle of the constancy of the speed of light. He claims that at least one of these would not violate Lorentz invariance, in the sense of 'intrinsically' preferring some Lorentz frame over others (even though, admittedly, even in that case the signal propagation is isotropic only in the rest frame of the emitter).

It is not clear, though, that relativity *per se doesn't* rule out superluminal signalling. That's because special relativity (SR) is based on two postulates put forward by Einstein: the *relativity principle,* which states that all inertial systems are equivalent for the carrying out of all physical experiments, and the *principle of the constancy of the speed of light,* i.e. the invariance of the speed of light in all reference frames

As for the Einsteinian realists, they remind us that Schrödinger has given a powerful argument against the Copenhagen interpretation, showing that it entails an absurd consequence and so it is *that* interpretation which must be given up. Schrödinger imagined a closed steel chamber containing a live cat, a Geiger counter, a hammer, a sealed flask filled with deadly hydrocyanic acid, and a small amount of a radioactive element, the probability of decay of one atom of which is exactly 0.5 in a given period, e.g. one hour. When a radioactive decay occurs, the Geiger counter closes a circuit triggering a device which releases the hammer which breaks the flask, and the fumes then kill the cat.[166]

Orthodox quantum mechanics combines the wave function representing the radioactive atom and the wave function representing the cat into a single wave function representing them both at once. Now, the fact that the atomic wave function is indeterminate between decay and no decay implies that the wave function representing the entire coupled system of (unobserved) cat and atom after one hour is also indeterminate between the cat's being alive and dead. If

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(Rindler 1977, pp. 23-4.) Rindler writes (p. 24): 'The physics based on Einstein's two postulates is called special relativity (SR).' The latter postulate is the relevant one for the present purposes. Maudlin's claim that SR doesn't forbid faster-than-light signalling depends on relaxing the principle of the constancy of the speed of light. But since that's one of the two postulates of SR, then his claim involves abandoning SR, at least in the usual form. (I ignore general relativity [GR] here because most of Maudlin's discussion and examples depend on presupposing SR, not GR.)

It is true, however, that *once* the Lorentz transformations have been derived on the basis of the two postulates, the constancy-of-the-speed-of-light postulate can be dropped. Everything is contained in the transformations themselves. The question *then* is: are there *Lorentz-invariant* ways of superluminal signalling, i.e. such signalling that doesn't entail a preferred reference frame? I say no. At the very least, I claim there is no physically realistic model of such signalling. Maudlin says, in effect, maybe, and refers one to Fleming's 'hyperplane dependent quantum theory'. However, he is forced to concede that Fleming's proposal entails a 'radical new conception of the world' (Maudlin 1994, p. 209), involving 'a fundamental rejection of the ontological foundations of both common sense and of classical physics' (p. 212). Moreover, there is a sense in which the original problem of Lorentz non-invariance reappears in another guise (pp. 209, 220).

In any case, Maudlin is forced to admit that '... so far as we know, so far as our theories inform us about what can be controlled or observed, faster-than light telegraphs cannot be built' (p. 84). That is, faster-than-light signalling is physically impossible. As Maudlin puts it, *nature* does not allow it (p. 116). In particular, he emphasizes that quantum theory entails that energy and signals cannot be sent using the EPR/Bell quantum connection. Or, as it is often said, 'there is no Bell telephone'. (p. 84). As far as *signalling* goes then, Maudlin is arguing an empty case, and he admits it (p. 116).

Maudlin goes on to argue that even though the violation of Bell's inequality (see §§3.3-3.4) doesn't entail superluminal signalling, it *does* entail superluminal transmission of information. He claims that there *must* be such transmission to explain the violation. The claim fails, as I show in §3.2. (For the view that the nonlocal influences cannot possible be exploited to carry information, see also Albert 1992, p. 72.)

166 Schrödinger 1935. The paper, prompted by the EPR argument (see Chapter 3), appeared in three parts. See also Schrödinger 1936.

this wave function is indeed a complete description of the state of the cat in the chamber, it follows that the cat is neither alive nor dead.[167] Suppose for example that the entire coupled system is represented by the wave function ψ_1, denoting the state 'cat is alive' and ψ_2 denoting the state 'cat is dead', then the state of the system at the end of one hour is, according to quantum mechanics, one of superposition, represented by the (normalized) wave function

$$\psi = 1/\sqrt{2}(\psi_1 + \psi_2).$$

If the cat were still alive at the end of one hour (i.e. if the atom had not decayed) it would be in the state $\psi = \psi_1$, whereas if the cat were dead (if the atom had decayed) it would be in the state $\psi = \psi_2$. Both states are quite different from the superposed state $\psi = 1/\sqrt{2}(\psi_1 + \psi_2)$, according to which the chamber contains a cat that is neither alive nor dead but in a *superposition* of the two states. Yet, according to the standard interpretation it is this latter state that the cat must be in before we actually open the chamber and take a look. When we do, the act of observation in some unexplained fashion collapses the wave function of the combined cat-atom system into either $\psi = \psi_1$ or $\psi = \psi_2$, i.e. an alive cat or a dead cat. (The 'second category' probabilistic laws [§1.3.1] take over.) Yet it also seems absurd to suppose that *until* we look the cat is neither alive nor dead. The question of whether Schrödinger's cat is alive or dead is a memorable illustration of the measurement problem of the standard interpretation. It is the quantum-mechanical version of Berkeley's question about the tree in the quad, with a peculiarly quantum-mechanical twist in the tail in the answer. Moreover, as Healey points out,[168] if we find a dead cat, it is our curiosity that killed the cat.

Schrödinger's thought experiment involves a characteristic feature of *every quantum-mechanical measurement*, as for example Jammer points out.[169] The only essential differences are:

(a) In the majority of quantum mechanical measurements the selection between possibilities is between more than two states with mutually exclusive properties, whereas in the case of Schrödinger's cat the experimental set-up has been deliberately chosen to keep the possibilities to only two alternative states with mutually exclusive properties; and

(b) Schrödinger's cat manages to extend the quantum-mechanical superposition of states to the macroscopic domain, bringing out in a dramatic way the peculiar nature of quantum reality according to the orthodox interpretation.

167 After Healey 1998, p. 83.
168 Healey 1998, p. 83.
169 Jammer 1974, p. 217.

Regarding difference (b), the distinction between classically describable and essentially quantum-mechanical systems is not so much a matter of the *accuracy* with which we can make an observation, as Bohm points out,[170] but rather whether its interesting properties depend critically on the quantum properties of matter. In the case of Schrödinger's cat, the most essential and striking feature of the macroscopic cat, namely its being dead or alive, is made to depend critically on the quantum properties of subatomic matter lacking objective definiteness of eventualities.

Something like Schrödinger's thought experiment was actually carried out in 1996 by putting a beryllium atom ion into a superposed state of spin up and spin down, in which state it was also in a superposition of two macroscopically distinguishable locations, the latter being analogous to Schrödinger's cat being in a superposition of two macroscopically distinguishable states, dead and alive.[171]

Questions raised by Schrödinger's thought experiment include: Just *when* does the measurement occur and *who* is the observer whose observation brings about the wave function collapse? Is it the human observer, who opens the cavity and takes a look? Or is it the cat, who feels the effect of the poison if the atom decays? Perhaps it is it the geiger counter, which records the atomic decay event, irreversibly amplifying it to macroscopic dimensions. Or does the collapse occur at different times and perhaps in different ways for different observers? There is an essential ambiguity as to how the collapse comes about and when it takes place.

The ambiguity exists owing to the essential ambiguity in quantum theory's account of the two possible modes of time evolution of a quantum system, discussed in §1.3.1. As we saw, the wave function can change over time in two very different ways, depending on whether the system is observed or unobserved. When unobserved, it evolves *continuously and deterministically* as a solution to the time-dependent Schrödinger equation (which we called a 'first category law' in §1.3.1), a process known as 'Schrödinger evolution', also 'unitary evolution' or '**U**', or 'process 2' (this last by von Neumann)[172], or it can undergo a *discontinuous change*, a 'collapse' or 'reduction' or 'R', or 'process **1**', according to probabil-

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[170] Bohm 1951, p. 165. Other well-known macroscopic phenomena that crucially depend on the quantum properties of matter are superfluidity, superconductivity and the specific heat of solids at low temperatures.

[171] A measurement destroyed the superposition and left the ion in just one place. But by repeating the experiment very many times and making the measurements at different times, an interference pattern was built up which revealed the superposition of both the spin up and down states and of the two locations. The important point, according to one of the experimenters (Chris Monroe), is that the two locations were separated by a distance of 80 nanometres, a distance over which classical physics works perfectly well. (See review in Ward 1996.)

[172] For a discussion, see von Neumann 1952, pp. 351-8, 417-45.

ity laws as a result of a measurement ('second category law'). The first type of process is thermodynamically reversible, and the second type of process is thermodynamically irreversible.[173] The possibility of the discontinuous change is provided for in the formalism by the reduction or projection postulate.[174] The ambiguity arises when we ask which one of the two laws is being obeyed at any given moment. The answer depends on whether or not a measurement is being carried out at that moment. But it is not at all clear what constitutes 'measurement' in quantum mechanics, as the term is nowhere defined in the formalism.

John von Neumann, who gave an axiomatic formulation of quantum mechanics in 1932, which aimed to represent the world in wholly quantum terms,[175] was forced to regard the two processes of time evolution as mutually irreducible.[176] There is however, no hard and fast dividing line between the two. If we consider the second process, the collapse, we might say that it occurs somewhere in the experimental apparatus itself, or we might say that the experimental apparatus is itself part of a larger system or chain which includes the human observer, and that the collapse occurs in the consciousness of the observer. This is von Neumann's answer to the question of where to draw the line. The so-called 'von Neumann chain' terminates in the 'subjective perception of the observer' – in the observer's consciousness.[177] It's hard to do better than Her-

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173 Von Neumann 1952, p. 418.

174 Although this postulate is often associated with von Neumann, it was already used by Dirac at the 1927 Solvay Congress, and in his 1930 text, *The Principles of Quantum Mechanics*. (Whitaker 1996, p. 195.)

175 Von Neumann objected to Bohr's division of the world into a classical part (measuring device) and a quantum-mechanical part (measured system). In his 1932 book *Mathematische Grundlagen der Quantenmechanik*, von Neumann represented the world in entirely quantum-mechanical terms, writing wave functions not only for the measured systems but also for the measuring apparatus: corresponding to the measured property eigenfunctions $\phi_1, \phi_2, \ldots\phi_n$, he had apparatus eigenfunctions $a_1, a_2, \ldots a_n$. But his representation needed to postulate a collapse of the wave function. Moreover, a natural location had to be found for the collapse. (In this thesis von Neumann's book is always cited in the 1952 English translation.)

176 Among other differences, Schrödinger evolution is thermodynamically reversible, whereas reduction is thermodynamically irreversible, as already noted above. The measurement process is irreversible in that re-establishment of definite phase relations between the eigenfunctions of the measured variable is extremely unlikely. In that regard, there seems to be a close connection between entropy and the measurement process, as noted by Bohm (1951, p. 608.) Bohm goes on to say that because the irreversible behaviour of the measuring apparatus is essential for the destruction of definite phase relations, and because the destruction of definite phase relations is in turn essential for the consistency of the quantum theory, thermodynamic irreversibility enters into the theory in an integral way (p. 609). However, that does not necessarily imply that there is anything fundamentally time asymmetric about quantum mechanics – just as the fact that cups of tea do not spontaneously boil does not imply that the classical laws of physics are time asymmetric. The irreversibility may arise simply because certain conditions are very hard to arrange in the world. For a discussion, see §2.5.

177 For discussion, see von Neumann 1952, pp. 417-45; Jammer 1966, pp. 370-4; Stapp 1993, pp. 138-40; Goswami 1997, pp. 214-15, 526-7.

bert's description of Von Neumann's dilemma:

> On each side of the wave function collapse, von Neumann erects impeccable mathematical structures familiar to quantum physicists – the world expressed as proxy waves. However, separating the two sides of the argument – the world unmeasured and the measured world – is a logic gap in which von Neumann effectively writes, "And then a miracle occurs".
>
> Von Neumann could not find a natural place to locate his "miracle". Everything, after all, is made of atoms: there's nothing holy about a measuring instrument. Following the von Neumann chain, driven by his own logic, in desperation von Neumann seized on its only peculiar link: the process by which a physical signal becomes an experience in the human mind. Von Neumann reluctantly came to the conclusion... that human consciousness is the site of the wave function collapse.[178]

This seems to give consciousness (or mind) a privileged status – a being with a consciousness has a different role in quantum mechanics than an inanimate measuring device. The idea was embraced by Eugene Wigner, who tried to extend it, and with whose name the 'consciousness interpretation' of quantum mechanics has come to be associated.[179] In particular, Wigner postulated that not only must consciousness be capable of being *acted upon* by matter, but it must also be capable of *acting back* on matter (in conformity with the law of action and reaction). If so, the equations giving the time variation of the wave function (i.e. the equations of motion) must be *non-linear* whenever conscious beings enter the picture. Some big questions that arise are: (a) how would the postulated non-linearity affect the consistency of quantum theory, in which the linearity of Schrödinger's equation seems essential?;[180] (b) what is the relation between the mind and the body (the latter on Wigner's picture being the 'substrate', the physico-chemical properties and conditions of the brain that gives rise to consciousness)?; and (c) just *whose* consciousness is it that does the collapsing – ours – or the cat's? Consider this last question. Suppose that a human, known as 'Wigner's friend' in the literature,[181] has taken the place of the cat in the box. Is the wave function collapsed by his consciousness, or by ours later on when we open the lid to ask him whether he is dead or alive?[182] Wigner later retreated

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178 Herbert 1985, p. 148.

179 See Wigner 1962; 1967, Ch. 13 & 14. Wigner suggested that the linearity of quantum mechanics might fail for conscious observers, leading to a resolution of the alternatives.

180 See Appendix(a) on the essential linearity of quantum mechanics.

181 See Wigner 1967, pp. 173-81; for a discussion, see e.g. Hughes 1989, pp. 294-5.

182 Penrose points out an apparent circularity in this kind of viewpoint. Conscious minds are products of evolution owing to mutations. The latter are presumably quantum events, and so must exist (on the consciousness interpretation) in linearly superposed form until they are collapsed by conscious minds – the evolution of which, however, depends on all the 'right' mutations actually having taken place. Thus, it is our presence which brings into existence our past (Penrose 1989, p. 295.) Although Penrose himself finds this kind of circularity repugnant, something like it has been embraced, as he notes, by J.A. Wheeler in the latter's 'participatory universe' picture (see §1.2[e]).

from this idea as he came to the conclusion that quantum theory can't be used for macroscopic systems (of which the brain is one). He calculated that the interaction of macroscopic systems with the environment, such as thermal photons, is sufficient to destroy certain quantum-mechanical interference effects in macroscopic bodies, with the consequence that the idealization of a macroscopic body as an isolated system is not a good approximation.[183]

Nor need we draw the line at the observer. In von Neumann's formulation of quantum mechanics the collapse of the wave function is introduced as an additional postulate, not derivable from the laws of quantum mechanics. But there is little justification for the postulate, save that it seems necessary. Why isn't the observer, too, in a superposed state just like the cat? Why isn't the observer's state one of having seen the cat alive *and* having seen the cat dead?:

$$\psi_{\text{cat-human system}} = (\psi_{\text{cat alive}} \times \psi_{\text{human sees cat alive}}) + (\psi_{\text{cat dead}} \times \psi_{\text{human sees dead cat}})$$ [184]

This is of course just the measurement problem discussed in §1.3.1. The story gets even more weird. According to the physicist Frederick Belinfante, if the Copenhagen interpretation of quantum mechanics is correct, then quantum mechanical devices can be used not only to kill cats but also to bring dead cats back to life. Belinfante predicts (with tongue firmly in cheek) that quantum theory could one day become an important branch of veterinary science.[185] This may not be quite as absurd as it sounds. Experiment shows that a collapsed wave function can be put back together again by using a 'quantum eraser' – a very peculiar form of quantum necromancy.[186]

Schrödinger himself vehemently rejected the possibility of a cat that is neither wholly alive nor wholly dead. He argued that states of a system which could be told apart by a macroscopic observer – such as whether the cat is dead or alive – must be distinct from each other whether actually observed or not. That of course does not constitute proof. It is rather an expression of Schrödinger's strong intuitions about the interpretation of the wave function – and in particular, repugnance at the discontinuity in Born's probabilistic interpretation. Schrödinger concluded that the quantum-mechanical description of physical reality is not complete.

Schrödinger believed that his investigation of the mathematical ramifications of seemingly straightforward cases such as that of the cat in the box showed that

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183 According to Wigner, even the 2.7K cosmic background radiation would be sufficient to grossly disturb the vibrational modes of say a cubic centimetre of tungsten located in intergalactic space. The radiation deposits on to the cubic centimetre of tungsten would be of the order of 2.3×10^{13} photons per second. (Stapp 1993, p. 130.)

184 Barrow & Tipler 1986, p. 467.

185 For brief details, see Herbert 1985, p. 151.

186 See Horgan 1992, p. 76.

the conceptual situation was even more complicated than the one envisaged by Einstein (and dramatised in the famous EPR thought experiment). It suggested to him not only that quantum mechanics was incomplete, but also that there existed a serious flaw in its theoretical foundations.[187]

A possible source of this flaw might lie, according to Schrödinger, in the role played by the *time* variable in quantum mechanics, in particular in its theory of measurement – which applies the non-relativistic theory beyond its proper range of applicability.[188] Be that as it may, it certainly seems true that for all the elements necessary to achieve a successful interpretation of quantum mechanics, one needs to go to some relativistic formulation of quantum mechanics for the purposes of such interpretation. By 'successful', I mean an interpretation which doesn't simply replace one interpretative mystery by another, in the way that for example Bohm's causal interpretation eliminates wave function collapse by replacing it with an explicit nonlocality and other ills (§3.5).

We shall turn to relativistic quantum theory in Chapter 6. In the next chapter, though, our task is to look at the role of time in the non-relativistic quantum theory.

Summary

In this chapter we have gone back to the basics of quantum mechanics. The reason is two-fold. First, to gain a clear understanding of the essential elements of the quantum-mechanical picture of the physical nature of matter, and just how this picture differs from the classical one. Second, to pin down the exact nature of the interpretational problem of quantum mechanics, with all its elements clearly displayed. Through going back to the basics, I've tried to show that the standard interpretation of quantum mechanics is philosophically unsatisfactory in two main ways:

(1) It is unsatisfactory *internally* owing to its measurement problem ('internally' in the sense that the measurement problem is an 'in-house' problem of the standard interpretation).

(2) It is unsatisfactory *externally* owing to its failure to come to proper grips with the quantization exhibited by quantum systems. The standard interpretation is not only ambiguous regarding the ontological status of the wave function, and wave-particle duality in general (wanting to have things both ways), but

187 Jammer 1974, p. 221.

188 Schrödinger 1935a. (Reprinted in English translation in Wheeler & Zurek 1983: see pp. 166-7 for the relevant remarks.) Von Neumann was also of the opinion that the non-relativistic character of quantum mechanics was its 'chief weakness' (1952, p. 354). Einstein himself bluntly rejected such an idea in a letter to Schrödinger of June 7, 1935 (Fine 1986, p. 73).

its doctrine of completeness acts to suppress investigation of the origin and significance of quantization – which is simply taken as a given.

I have argued that the philosophical unsatisfactoriness of the standard interpretation has its origin in the apparent wave-particle duality exhibited by matter and radiation, which duality itself stems from quantization.

Take (1), the 'internal' unsatisfactoriness. A consequence of the duality, which is taken as a given in that interpretation, is the irreducible role of probability in describing the time evolution of a quantum system: it is as if one were describing an *ensemble* of systems. At the same time, however, the standard interpretation maintains that the wave function provides a complete description of even an individual quantum system (§1.3). The latter claim has the consequence that the standard theorist is faced with the problem of accounting for the origin and nature of the collapse of the wave function – which is nowhere described in the formalism. The formalism, in the absence of the *ad hoc* projection postulate, predicts that measurements generally ought not to have determinate results, which would be false. This is the measurement problem of the standard interpretation, highlighted in the 'Schrödinger's cat' thought-experiment.

A related problem of that interpretation is that the collapse is nonlocal, as is especially evident in the EPR-type of experiment. For example, we've seen that the combined spin state of a pair of widely-separated singlet-state particles is reduced instantaneously from the original singlet state – in which the formalism requires that neither particle has a well-defined state of spin – to a 'disentangled' state in which both spins are well-defined. (The collapse is however, only 'weakly' nonlocal, in the sense that it can't be exploited even in principle to send readable messages). The nonlocality in the standard interpretation is not confined to the EPR-type of experiment. It is equally a feature of the two-slit experiment, and measurement in quantum mechanics in general, as shown for example by Wheeler's 'delayed-choice' type of experiments.

Now take (2), the 'external' unsatisfactoriness of the standard interpretation, as I've called it. The quantum wave function holds a central place in quantum theory, with much of the philosophical investigative work in quantum mechanics revolving around its interpretation, as we saw in §1.3 and §1.4. But even more central – and basic – is the *quantization* exhibited by quantum-mechanical systems, examined in §1.1. It is the quantization that is responsible for the difference between classical and quantum-mechanical systems – and therefore the root cause of the interpretative problem in all its manifestations. The wave properties of matter are inferred from the quantization, and not the other way around. The Copenhagen doctrine of complementarity is founded on the quantization. Planck's constant h is the quantitative and qualitative expression of this quantization. Yet the quantization is not well-understood. In the standard interpretation it is simply taken as a given. It seems clear that to make progress in interpreting quantum mechanics, we need to engage with the quantization (and therefore h) more closely. To my mind, h is the key to interpreting quantum mechanics. But the standard interpretation would deny this, as it takes quantum mechanics as complete.

Consider again my claim that h is the key to interpreting quantum mechanics. We have seen that de Broglie's relation $\lambda = h/p$, constructed around h, connects in a single equation two apparently incompatible states of matter, namely being a *wave* (having the wavelength λ) and being a *particle* (having the momentum p). We have also looked at the relation of de Broglie waves to the wave function in the standard interpretation, noting that Schrödinger's equation simply specifies the laws of de Broglie wave motion which the particles of any microscopic systems obey. Furthermore, when the wave function collapses in the standard interpretation, it is the superposition of de Broglie waves (the wave packet) which collapses. A thorough appreciation of these points is essential for understanding the project of this thesis. That's because an obvious way to shed light on the question of the ontological status of the wave function and its collapse, and on whether it provides a complete description of quantum-mechanical systems, is to determine the ontological status of the de Broglie waves themselves. In other words, the task is to see if they are an artefact of the standard interpretation. If so, the collapse of the wave function is an artefact, and quantum mechanics is incomplete.[189] One way to determine the ontological status of de Broglie waves is to see if the work they do can be done by some other process – in the present case by advanced action. (We turn to that task in the second half of the thesis.) In this connection, recall that we saw in §1.1 how, by interpreting quantum mechanics in terms of advanced action, we are also interpreting h in a certain way.

An aim of the present chapter has been to show that the standard interpretation of quantum mechanics is philosophically unsatisfactory – which is the first of the four claims this thesis argues. However, the understanding gained in the present chapter regarding the central role of h in the interpretative problem of quantum mechanics will also be of relevance in the later chapters when we come to look at Price's advanced action interpretation of quantum mechanics.

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189 Even though there are other, more abstract mathematical formalisms of quantum mechanics than that of Schrödinger, they, too, contain the equivalent of the wave function, namely the state vector. If one can show that the basis of the Schrödinger formalism is an artefact, that would be strongly suggestive that the state vector of the other formalisms, too, is an artefact.

2

The Role of Time in Quantum Mechanics

Time is shorthand for the position of everything in the universe.
(Julian Barbour, 1999)

Schrödinger's suggestion (end of last chapter) that there was a serious flaw in the foundations of quantum mechanics, possibly connected with the role of time in it, brings us to time and quantum mechanics. What, exactly, is the role of time in quantum theory, and in particular, in the theory's account of measurement? In this chapter, we shall concern ourselves with just two aspects of this question: (1) the way time and time reversal enter into the quantum-mechanical formalism; and (2) the implications of the discontinuous change in the wave function (the collapse) for the time reversal invariance ('T invariance') or otherwise of quantum theory. Does the collapse render quantum mechanics a T non-invariant theory? In the context of (2), we shall also touch on the role of thermodynamic asymmetry in the matter of the T invariance of quantum mechanics. We also note and store away for investigation in later chapters (Chapters 5 & 6) Schrödinger's worry that perhaps the quantum theory of measurement applies the non-relativistic quantum theory beyond its proper range of applicability.

2.1 How do time and time reversal enter into quantum mechanics?

Time enters into the formalism of quantum mechanics as a parameter as in classical pre-relativity physics, where it is a number with a well-defined value. Its role is to specify how the wave function Ψ develops in space, and to specify the mathematical apparatus of the theory needed to extract predictions from it (in particular the energy operator). The time of quantum mechanics is a Newtonian time, and time intervals are physically measurable quantities. The measure of time is quantified by the variable t. The theory presupposes the existence of macroscopic measuring apparatus such as laboratory clocks and rulers.

Time reversal enters into the formalism of both quantum mechanics and classical mechanics in a natural way from the role of time in those theories as a parameter. It is assumed that the variable t is additive, and hence can be assigned an algebraic sign (time intervals Δt can be added or subtracted to give longer or shorter intervals). When this assumption is coupled with the assumption that the choice of an origin $t = 0$ for a time interval is arbitrary (which assumption is connected with the fact that we cannot measure an instant of time but only intervals of time), it follows that the assignment of a sign for t (say positive for t later than $t = 0$ and negative for t earlier than $t = 0$), is purely conventional.

The conventional character of the sign of t enables the introduction of a 'reverse-time' variable t', such that $t' = -t$, and $\Delta t' = -\Delta t$. Kinematically, t' and t have equal standing, because the time interval $\Delta t = t_2 - t_1$ can be expressed just as well in terms of t', i.e. $\Delta t' = t'_2 - t'_1$, without altering its algebraic properties.

Another way of explicitly expressing the above relation between t and $-t$ is to introduce the classical *time reversal transformation* T, where

$$T: \quad t \to t' = -t,$$

which is to be read as: 'Under T, t transforms to $t' = -t$.' (T operates on a description of a system, the properties of which are a function of time. E.g. in the Schrödinger equation, it operates on the wave function – a solution of the equation.) In what follows, we shall refer to the reference system using t as a variable as the 'standard system', and the one using t' as the variable as the 'transformed', or 'time-reversed' system'.

The corresponding quantum-mechanical transformation is of the same form, but it has an additional element not present in the classical transformation, as we shall see in §2.3. To avoid non-essential complexity, we shall stay for the moment with the simpler classical time reversal transformation to make the basic notions clear.

As a consequence of the above transformation, the corresponding transformation for the velocity variable v is

$$T: \quad v \to v' = -v,$$

where the transformation T may be said to reverse the velocity. In fact, the transformation T is often referred to simply as 'velocity reversal'.[1]

The transformation T is *discrete*. There can be no smooth, continuous transition from forward in time to backward in time. (T shares its discreteness with two other related transformations, known as C and P [described in §2.4 below].

[1] The above discussion of time reversal is after Sachs 1987, pp. 3-5.

Just as there can be no smooth, continuous transition from forward in time to backward in time, there can be no smooth, continuous transformation from positive charge to negative charge, or from a left-handed system to a right-handed system.)

It has been recently argued by William Unruh that the way in which time enters into quantum mechanics differs from the way it enters into classical mechanics, owing to the role of time in specifying the boundary conditions of a system. It will be useful to take a quick look at Unruh's argument now, especially as we shall have to touch on the issues it raises in the next sub-section (where I consider the question of whether the collapse of the wave function renders quantum mechanics T non-invariant).

In classical mechanics the equations of motion specify how the values of specified variables at any one time are related to those at any other time. To solve the equations, one either fully specifies the variables (the state of the system) at some one time, or one partially specifies them at two separate times. For example, one might specify both the position and the momentum of a particle at some time, or the position of the particle at two different times. Either way, the specification has the effect of completely particularizing the theory, and is therefore sufficient to specify the outcomes of all experiments at all other times (everything else being the same). Later-obtained information about the position or momentum is in principle redundant.

In quantum mechanics, on the other hand, things are different, argues Unruh: the role of time in specifying the boundary conditions differs from that in classical mechanics, and the difference is not simply a matter of detail. In quantum mechanics, later knowledge is far from redundant, as it actually replaces earlier knowledge. '[N]ew knowledge changes the results of the theory in a way completely unexpected in classical physics.'[2] That is possible because quantum mechanics is a theory of 'insufficient cause'. Given two identical states (causes), the outcomes (effects) can differ. The complete specification of the system at one instant of time is generally not sufficient to completely specify the outcomes of experiments at other times, even though the wave function at later t is completely determined by its state at earlier t. To see this, recall that the wave function Ψ is supposed to specify completely the state of the system. But we've seen that when the system's wave function Ψ is given, and provided that Ψ is not an eigenfunction of the operator $\hat{O}$, the observed value of any observable O cannot be predicted, but fluctuates about some mean. Consequently, after each measurement we need to discard our earlier knowledge about the state of the system and replace it by the new knowledge. Analogous considerations apply when the wave function is an eigenfunction of the operator $\hat{O}$ (see Appendix[e]).

2 Unruh 1995, p. 53.

I agree with Unruh that later knowledge replaces earlier in quantum mechanics, and that this is a point worth stressing. However, I don't think that it shows that time plays different roles in (non-relativistic) classical and quantum mechanics. The *roles* seem to be the same in both theories. Only the *theories* differ, one theory being fully causal, and the other a theory of 'insufficient cause' of a certain very specific kind.[3] Consequently, the ensuing phenomena may be expected to look very different even though time plays the same role in both theories.

2.2 Time reversal invariance in classical & quantum physics

Let us take a closer look at *T* invariance in physics, starting with classical physics, taking Newtonian mechanics as the paradigm case. Just what are we talking about when we refer to *T* invariance? We need to define our terms and make some distinctions.

(1) *Invariant* means constant or the same. '*T* invariance' refers to the essential sameness of that which is invariant in both the forward and backward senses of time.

(2) *Theories* are *T* invariant/-non-invariant, whereas the systems or processes described by the theories are reversible/irreversible. The statement of *T* invariance is primarily about the *equations* (the laws or the theories), not about the actual processes themselves (i.e. the playing out of the laws in the processes), since the latter also critically depend on the particular boundary conditions that happen to obtain.[4]

(3) The *T* invariance/-non-invariance of theories is determined by the *reversibility/irreversibility* of the processes they describe. If all processes that are possible in the forward time direction pursuant to the laws of the theory (for given initial states) are reversible, that is, also possible in the reverse time direction or *backward* pursuant to the same laws, the theory is *T* invariant. The fact that the processes are reversible in this way is just what is *meant* by saying that the theory is *T* invariant. By the same token, if the processes or some of them are irreversible, the theory is *ipso facto T* non-invariant. How can we tell whether the processes described by a theory are reversible? The test of reversibility is whether the theory provides *identical algorithms* (or instruction

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[3] Unruh's point depends not just on the indeterminacy of quantum mechanics, but also on the precise nature of the indeterminacy. Not just any kind of indeterminacy will do. The indeterminacy must be *rule-like*, to ensure consistency with what is observed. In particular, the precise nature of the indeterminacy must be consistent with both the reduction postulate of orthodox quantum mechanics and its principle of complementarity, as expressed by the indeterminacy relations. See Appendix(e).

[4] Sachs 1987, p. 6.

sets) for calculating future/past physical situations of the world from its present physical situation.[5] If it does, and if the theory entails that a certain process can happen in the forward time direction, then it will also entail that the process can happen in the backward time direction.[6]

(4) We must also specify what it means for something to happen in the *reverse time direction*, or *backwards*. Take a theory *H*. Any physical process described by the theory is some sequence $S_I \ldots S_F$ of instantaneous states of *H*. For that process to occur backward is for the sequence $S_F \ldots S_I$ to occur.[7]

(5) When talking of the reversibility/irreversibility of a process, we must distinguish two senses of 'reversibility' – *reversibility in principle* and *reversibility in practice*, the latter entailing the former, but the former not entailing the latter. A process might be reversible in principle but nonetheless remain *effectively* irreversible, owing to the practical impossibility of setting up the requisite initial conditions to bring about a reversal. In that case, the irreversibility would be, as I shall designate it, merely 'factlike', as opposed to 'lawlike', and the theory describing the process could still be *T* invariant. *T* invariance is defined in terms of reversibility in principle.

(6) We must also specify what is meant by 'reversibility in principle'. There are two senses of 'reversible in principle' for systems described by physically realistic theories – which I shall designate the strong sense and the weak sense:

(i) The strong sense of reversibility in principle applies to physically realistic systems or processes described by (classical) *deterministic laws* of a non-statistical nature. It is as follows. If a system, including its environment, is *T* reversed, i.e. if we apply $T\colon\ t \to t' = -t$ to the system, the deterministic nature of the relevant laws (together with the existence of identical algorithms for inferring forward and backward), ensures that the system returns to its earlier state without fail.[8] For example, if the system went

[5] In the case of a physical theory, the instruction sets consist of the *laws* of the theory and (more or less) mechanical *procedures* for applying the laws.

[6] This algorithmic criterion of *T* invariance is after Albert 2000, pp. 11-14.

[7] After Albert 2000, p. 11.

[8] In the general case, mere determinism isn't enough. It turns out that one can imagine theories which, although deterministic, are not *T* invariant. For example, Albert (2000, p. 12) gives an example of a physically unrealistic theory that fails to provide identical algorithms for inferring toward both the future and the past. He also gives an example of a physically unrealistic theory that provides an algorithm for inferring toward the future but not toward the past, for which there is *no* algorithm. In both cases, the 'theories' will deny that whatever can happen to a collection of particles in the forward direction can also happen backwards. They distinguish between the future and the past.

John Earman, too concludes that 'there are conceivable circumstances in which the from state A to state B, then the system, after *T*, goes back from state B to state A. The determinism together with the existence of identical algorithms for inferring forward and backward allows the replication in principle of any process, whether forward or backward in time, given the appropriate boundary conditions. Newtonian mechanics is reversible in this sense, as is classical electrodynamics.[9] (If we know the state of such system at any instant, the relevant law enables us to determine from this state the series of states the system passes through over all time, both forward and backward. Classical determinism of physically realistic theories is thus to be understood as a temporally symmetric relation between states.)

(ii) The weak sense of reversibility in principle applies to physically realistic systems or processes described by intrinsically *probabilistic laws*, such as quantum mechanics in the standard interpretation.[10] It is as follows. If we apply $T\colon t \rightarrow t' = -t$ (or its quantum-mechanical version if the system is a quantum-mechanical one [see §2.3]) to such a system, the system need not, and generally does not, return to its earlier state owing to the probabilistic nature of the relevant laws. However, returning to the earlier state must be an option – a *physically* possible process – if a system is to be reversible in the weak sense. It must not be *forbidden* by the theory. For example, if a system went from state A to state B, then the system, following *T*, need not, but may, return to state A. The essential thing in that regard is the intrinsically probabilistic nature of the relevant laws and not anything to do with reversal as such. It follows from the probabilistic nature of the laws that if a system described by such theory is intrinsically indeterministic in the one time direction, it must be so also in the other. However, the mere fact of intrinsic indeterminacy in both time directions isn't sufficient to establish the *T* symmetry of the theory. For that, the theory must pro-

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most natural and plausible description would violate time translation invariance; and that... the same circumstances would also violate periodicity without necessarily violating determinism' – though he concedes that such circumstances are far-fetched. (Earman 1986, p. 135.)

It is the logical possibility of such circumstances, even if they are far-fetched, that necessitates Albert's algorithmic criterion of *T* invariance. Quite generally, as Albert puts it, deterministic theories 'can allow that whatever can happen forward can also happen backward *only* if the theory offers us identical algorithms for inferring toward the future and the past, and (equivalently) it turns out that deterministic theories can *deny* that whatever can happen forward can also happen backward only if they *fail* to offer us identical algorithms for inferring toward the future *and* the past'. (Albert 2000, pp. 12-13.)

[9] Though for an argument for the contrary view concerning the latter, see Albert 2000, pp. 14-21.

[10] By 'law', physicists generally mean the mathematical formalism of the theory together with its interpretation. The physicists' usage is flexible, sometimes even cavalier – but by the same token heuristic-friendly. In contrast, philosophers tend to have a more formal (and rigid) understanding of 'law'.

> vide *identical* algorithms for inferring probabilities toward both the future and the past. If such algorithms exist, it of course follows that identical probabilities must apply in both temporal directions, everything else being the same.

Another frequently encountered expression in the context of the discussion of time is 'time-symmetric/-asymmetric'. That is a generic expression that indicates a symmetry/-asymmetry of *some* kind in the two temporal directions. Usually the symmetry/asymmetry indicated refers to the boundary conditions. In that case it refers to a 'factlike' condition rather than a lawlike one, the existence of which is generally closely associated with thermodynamics. (See [5] above regarding my usage of 'factlike'.) Practically all the phenomena of nature seem to be asymmetric in time in this sense, as e.g. Davies points out.[11] For example, we remember only our past. There is of course no inconsistency between such factlike macroscopic asymmetry (traceable to boundary conditions) and a lawlike microscopic symmetry. However, the expression 'time symmetric/-asymmetric' can also be used to refer to a 'lawlike' condition claimed to exist between the two temporal directions. In what follows, I shall mostly speak of T invariance/–non-invariance, and reversibility/irreversibility, avoiding the less explicit expression save where its use seems appropriate.

For greater completeness yet another expression needs to be mentioned that one sometimes encounters: 'isotropy/anisotropy of time', favoured by some philosophers. It is supposed to characterize the structural sameness/non-sameness of time in the two temporal directions. It is taken to be clarificatory, but to my mind its clarificatory qualities have been overstated and it will not be used here.[12]

To get a clearer appreciation of the physical sense of T invariance, we shall need to consider some specific examples. Before we do, though, it will be useful to take a quick look at mirror reflection invariance, which is closely related to time reversal invariance, the latter being the temporal counterpart of the former.

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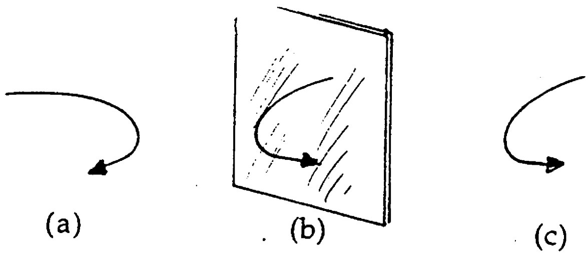
11 Davies 1974, p. 3.

12 It is used, for example, in Horwich 1987. In Ch. 3, Horwich is concerned with the 'structure of time'. He argues that 'time itself' is isotropic, meaning, according to him, that it has no privileged direction. Yet, he says, 'we should be open to the idea that time is anisotropic, despite [its] having no *privileged* direction' (p. 37). Unsurprisingly, Horwich feels the need to clarify the meaning of the clarificatory expression 'time is anisotropic'. He attempts to do so by appealing to another philosophical notion, namely the 'intrinsic' dissimilarity of the past and future directions of time, the 'intrinsic' properties of an object apparently being those expressible by predicates composed of 'natural' predicates. Next, the entire notion of anisotropy needs to be 'sharpened up' by contrasting it with other conceptions of anisotropy, defined in terms of notions such as the 'objectivity of time order' and 'nomologically irreversible processes'. It's hard to avoid the impression that for Horwich it's 'turtles all the way down...'

Take the elliptical orbit of a particle moving under the influence of a central attractive force. (For simplicity we ignore friction and other perturbations.) The orbit is a solution of the equations of motion for a certain set of initial conditions. View the orbit, or a segment of the orbit in a mirror placed at right-angles to the plane of the orbit, as in the diagram below. (The mirror changes a right-hand axis coordinate system into a left-hand axis coordinate system, or vice versa.)[13] If the equations of motion are invariant under reflection, the reflected orbit, too, ought to be a solution of the equations of motion. Even though the invariance is a mathematical fact, we can 'test' such invariance by performing a real experiment in which we take as our initial conditions those of the reflected orbit segment, i.e. as seen in the mirror – which are clearly different from those of the actual orbit segment. (E.g. the coordinates of 'start' and 'finish' will be different, as will be the velocity.)



If the experiment yields an identical orbit or orbit segment, save for the direction of travel, to the original one (after the appropriate translations of the coordinates) every time the experiment is repeated, then the mirror reflected orbit, too, can be seen to be a *solution* of the equations of motion. Moreover, the original orbit and the reflected orbit are *superposable*. At each point along its orbit, in either orbit, the particle has exactly the same kinetic energy, potential energy and velocity (save for the sign). *The reversal of motion in the orbits is simply a consequence of the change in the initial conditions.* The concept of invariance under reflection can be readily extended to include other phenomena such as electric currents and magnetic fields.[14]

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13 Note that, contrary to what we might at first think, the mirror doesn't really reverse left and right, but front and back. That is, a mirror transposes only the coordinates *normal* to its plane. It only *looks as if* it transposes left and right because when we look into a mirror, we appear to be located or imagine ourselves to be located behind the pane of the glass (the same distance behind it as we are actually in front of it) and also turned around so that we are facing our real selves. This interchanges handedness. View a right-hand glove in a mirror. The mirror reflection is a left-hand glove, and one cannot be transformed into the other without turning it inside out. Handedness is an extrinsic property the glove acquires when embedded in 3-space. Intrinsically, it has no handedness.

(Mirror-imaging is related to but not identical to the parity operation *P*, described in §2.4. A mirror inverts [transposes] only the coordinates normal to its plane. This must be followed by a rotation through π about the normal for the parity transformation [Bjorken & Drell 1964, p. 72]. *P* changes a right-hand axis into a left-hand axis, or vice versa, and rotates the system by π about the normal.)

14 For details, see Sachs 1987, pp. 20ff. As for the well-known breakdown of reflection invariance, we shall come to that in §2.4.

Now consider the time reversal transformation T: $t \rightarrow t' = -t$. As already noted, the above experiment tests explicitly only for reflection invariance, or P, not for T invariance. However, a practical 'mirror' of the time variable for classical mechanical systems such as the one in our illustration above can be created by taking a movie of the system, with sufficiently high resolution to enable measurements of the motion when the movie is projected onto a screen.[15] Play it backwards. The backward-run movie models the original system in time reverse, and enables measurements of the motion as a function of the reversed time variable t'. It also illustrates the *physical content* of T invariance in the strong sense. (There is no special significance to the use of a movie, or the fact that movies are discrete, i.e. constituted of many stills. The important idea is simply the reversal of all motion and potentials.) Again, the reversal of motion is simply a consequence of the change in the initial conditions. It is evident that the trajectory shown in the backward-run movie is identical to the mirror-reflected one.[16] It follows that the very same (idealised) experiment described above (to model the invariance of the laws of motion under space reflection P) also models the invariance of the laws of motion under time reversal T.

The classical, deterministic laws of motion are invariant under T in the strong sense. These, and other T invariant laws have the property that when the direction of time is inverted, the equations that describe them remain unchanged. Maxwell's equations of electromagnetism are an example.[17] Apply T to a retarded wave, and you get an advanced wave. An advanced wave is *physically possible,* though we don't seem to see any.[18] (I shall have more to say about whether we do see them or not in Chapters 4 and 5, in the context of Price's advanced action interpretation of quantum mechanics.)

What about the apparent irreversibility of complex classical systems ('complex' in the sense of having many degrees of freedom)? Take a movie of a rampaging bull in a china shop. If the movie is run backwards, it certainly shows a *practically* irreproducible situation. However, that has nothing to do with the invariance of the underlying laws, but only the practical irreversibility of the initial conditions owing to their sheer complexity. Even though each element of the system is exactly reversible, the likelihood of exactly reproducing *all* the initial conditions necessary to bring about the situation shown in the backward-

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15 Sachs 1987, p. 21.

16 Make a movie showing at once the system and the reflected system. Play the movie backwards. The system and reflected system are interchanged in the backward-run movie. The equivalence of the reflected and time-reversed orbits is also evident from the fact that reflection is effectively the same as velocity reversal, which in turn is the same as T reversal for classical systems.
(T: $v \rightarrow v' = -v$.)

17 For a dissenting view, though, see Albert 2000, pp. 14-21.

18 Davies 1995, p. 209.

run movie is very small – exponentially smaller the greater the complexity of the system.[19] The same goes for our movie of the particle moving under the influence of a central attractive force when frictional and other perturbative factors are included, which would create an asymmetry in the actual trajectory of the particle and our movie of it (e.g. the particle would slow down whereas in the movie it would speed up). As Einstein wrote to his friend Michele Besso, the apparent irreversibility of complex systems is an *illusion* produced by improbable or hard-to-set-up boundary conditions.[20]

We now move to the second sense of T invariance – the 'weak' sense. It is evident that no intrinsically probabilistic theory can be T invariant in the above strong sense for the reasons stated in (6)(ii). If we apply T to such a system, the system need not, and generally does not, return to its earlier state owing to the probabilistic nature of the relevant laws.

Quantum mechanics in the standard interpretation falls into this category. Although Schrödinger's equation is deterministic, quantum mechanics is an intrinsically probabilistic theory according to the standard interpretation, owing to its wave-particle link, according to which $|\Psi|^2$ gives only the probability of some observation.

Given the intrinsic indeterminism of the theory in the forward time direction, it must be intrinsically indeterministic also in the backward time direction.

The question then arises: does the indeterminism render quantum mechanics a T invariant theory? Let us check our criterion of T invariance for intrinsically probabilistic theories in (6)(ii).

We noted that if we apply T to such a system, the system need not, and generally does not, return to its earlier state owing to the probabilistic nature of the relevant laws. So the system is not reversible in the strong sense. However, for intrinsically probabilistic theories, that doesn't automatically rule out T invariance, provided that returning to the earlier state remains an option – a physically possible process – for the system, even if an unlikely one. It must not be forbidden by the theory. This is the *weak* sense of reversibility. However, the actual test of reversibility (and so of T invariance) for an intrinsically probabilistic theory is whether the theory provides identical algorithms for inferring probabilities toward both the future and the past. If such algorithms exist, the theory is T invariant. (The existence of such algorithms *ipso facto* guarantees that the system is not forbidden from returning to its earlier state, thus satisfying our condition of reversibility in the weak sense.)

The physical content of reversibility in the weak sense may again be illustrated by taking a movie of some appropriate process of interest, describable by

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[19] See Sachs 1987, p. 26.

[20] Cited in Prigogine & Stengers 1984, p. 257; see also Pais 1982, p. 68.

the laws of quantum mechanics. It is of course impossible to take a movie of the quantum wave function itself, since it is unobservable. Nor is it feasible to take a movie of the 'trajectory' of a quantum particle, since each observation of its position alters the system. Indeed, according to the standard interpretation, an unobserved quantum particle does not have a classical trajectory. However, we can limit the number of stills in our movie to show just the initial emission event(s) and the final absorption event(s) – which can be flagged by our macroscopic measuring apparatus.

The process shown in our 'movie' is *reversible* in the weak sense if the backward-run movie shows a *possible* physical process. (It doesn't need to be a *likely* process.) However, the mere fact of reversibility in this sense isn't sufficient to prove that quantum mechanics is T invariant. It shows merely that T invariance is not ruled out. The real test of T invariance is whether quantum theory provides identical algorithms for inferring probabilities forward and backward. I show that it does in §2.5. There we shall also see just how the collapse of the wave function enters into the picture.

First, though, we shall need to take a brief look at an important feature of time reversal in quantum mechanics, connected with the complexity of Schrödinger's equation. When the time-reverse of the wave function is taken, it is necessary not only to replace t by $-t$, but also to replace the wave function by its complex conjugate.

2.3 The quantum-mechanical time reversal operator

Two conditions must be imposed on any candidate for the time reversal transformation T in quantum mechanics: (1) that it be a kinematically admissible transformation, in that it is consistent with the commutation relations; and (2) that it conform to the requirements of the correspondence principle, in that the operators representing classical kinematic observables must transform under T in a way corresponding to classical motion reversal.[21] Given these conditions, the fact that Schrödinger's equation is *complex* necessitates that T in quantum mechanics must include the operator K, which takes any complex number into its complex conjugate.

For this reason, and having regard to the fact that the commutation relations are invariant under any unitary transformation, the transformation T in quantum mechanics may be written as

$$\hat{T} = UK,$$

where U is the linear transformation T: $t \to t' = -t$. ($\hat{T}$ is known as 'Wigner's

21 Sachs 1987, p. 34.

time inversion operator'.)[22]

The effect is that not only is t replaced by $-t$, but also the wave function is replaced by its complex conjugate, so that

$$\hat{T}\Psi(t, \mathbf{r}) = \Psi^*(-t, \mathbf{r}).$$

This is an 'antiunitary' transformation and $\hat{T}$ is an antiunitary operator.[23] It has

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[22] Wigner writes, 'the transformation $t \rightarrow -t$ ... transforms a state φ into the state $\boldsymbol{\theta}\varphi$ [where $\boldsymbol{\theta}$ is $\hat{T}$] in which all velocities (including the "spinning" of the electrons) have opposite directions to those in φ. (Hence, "reversal of the direction of motion" is perhaps a more felicitous, though longer, expression than "time inversion".)' (Wigner 1959, p. 325.)

[23] Wigner (1959, Ch. 20) gives a comprehensive treatment of the properties of antiunitary transformations, to which the reader is referred. See also Bohm 1951, pp. 372-5. Here we simply note that antiunitary transformations and operators may be understood in the context of unitary transformations and operators. A unitary operator U is defined as an operator which satisfies the equation $U^*U = 1$, which is equivalent to any of the following: $U = U^{*^{-1}}$, $U^* = U^{-1}$, $U^{-1}U^{*^{-1}} = 1$, where U^* is the complex conjugate of U. (Dirac 1935, pp. 110-11.)

A transformation $\Omega' = U\Omega U^{-1}$, where Ω is an arbitrary linear operator and U is a unitary operator, is called a *unitary* transformation (p. 111). This equation may be regarded as expressing a transformation from any linear operator Ω to a corresponding linear operator Ω', each Ω' having the same eigenvalues as the corresponding Ω (Dirac 1935, pp. 109-10).

A unitary transformation is one that transforms hermitian operators into hermitian operators, and linear operators satisfying the expansion theorem into linear operators satisfying the expansion theorem. (For details, see Dirac 1935, pp. 109-11ff.) Thus a 'unitary transformation is one that transforms observables into observables. It leaves invariant any algebraic equation between the observables and also, as may easily be verified, any functional relation based on the general definition of a function...' (p. 111). For instance, the Schrödinger evolution (or time displacement by t) of the wave function $\varphi_0 = \sum_n a_n \psi_n \rightarrow \varphi_t = \sum_n a_n e^{-iE_n t/\hbar} \psi_n$, where ψ_n are the stationary states and E_n the corresponding energy values, is a unitary transformation. (After Wigner 1959, pp. 325-6.) That is why the Schrödinger evolution is also called 'unitary evolution', or simply '$\boldsymbol{U}$'. A unitary transformation corresponds to a generalization of a rotation in three-dimensional space, which also leaves all vectors unaltered in length; moreover, it causes wave functions that were originally orthogonal to be transformed into wave functions that remain orthogonal, in that respect also resembling a three-dimensional rotation, which transforms any two mutually perpendicular vectors into a new set of mutually perpendicular vectors. (Bohm 1951, p. 373.)

We now move on to antiunitary operators. Every antiunitary operator, in particular $\hat{T}$, can also be written as the product of a unitary operator and the operator K of complex conjugation

$$\hat{T}K = \boldsymbol{U}; \quad \hat{T} = \boldsymbol{U}K,$$

where $\boldsymbol{U}$ is a linear unitary transformation. The effect of K is to replace the expression following it by its complex conjugate, such that $K\varphi = \varphi^*$. (Wigner 1959, p. 328.)

The reason that time reversal symmetry in quantum mechanics is special is just be-the consequence that Schrödinger's equation/ $\hat{H}$ is invariant under time reversal, or rather, under 'Wigner' time reversal.

Take Schrödinger's equation :

$$i\hbar\frac{\partial\Psi}{\partial t}=\hat{H}\Psi(t,\mathbf{r}).$$

Replace t by $-t$:

$$i\hbar\frac{\partial\Psi}{\partial -t}=\hat{H}\Psi(-t,\mathbf{r}),$$

i.e.

$$-i\hbar\frac{\partial\Psi}{\partial t}=\hat{H}\Psi(-t,\mathbf{r})$$

Operate on the state with the anti-linear operator K, which takes any complex number into its complex conjugate. We get

$$i\hbar\frac{\partial\Psi^*}{\partial t}=\hat{H}\Psi^*(-t,\mathbf{r}),$$

which is of the same form as the original Schrödinger equation above.[24]

This is usually taken to mean that the *complex conjugate* of the wave function is also the *time reverse* of the wave function.[25] The transformed wave function describes the original system running backward in time and is physically realiz-

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cause it does not lead to a unitary transformation $\hat{T}=\boldsymbol{U}$ but instead to the antiunitary transformation $\hat{T}=\boldsymbol{UK}$. In that regard, Robert Sachs (1987, p. 36) writes, 'In quantum mechanics we deal with unitary function spaces and, as a consequence, are accustomed to associating *unitary* transformations with physically meaningful operations such as symmetry operations.' But it turns out that time reversal symmetry in quantum mechanics is *anti*unitary because $\hat{T}$ must include the operator K, which takes any complex number z into its conjugate complex. It is because of the special properties of antiunitary transformations that 'the role played by time reversal in quantum mechanics is distinct from that of any of the other symmetry operations of physics'. Bohm (1951, p. 373) notes that if unitary transformations corresponds to a generalization of a rotation in three-dimensional space, then non-unitary transformations correspond to stretching and shearing.

24 See e.g. Callender 2000b, p. 262; Goswami 1997, pp. 529-32; Sachs 1987, pp. 33-6; Park 1974, pp. 151-3; Wigner 1959, pp. 325-48.

25 Cramer, for example, writes: 'Wigner's time-reversal operator is... just the operation of complex conjugation, and the complex conjugate of a retarded wave is an advanced wave.' (Cramer 1988, p. 229.) See also e.g. Davies 1974, pp. 156-7; Longini 1970, pp. 63-4. For an analysis of the *electromagnetic* wave equation and its four solutions in the context of the operation of time reversal, see Cramer 1980, 362-4.

able, since it satisfies Schrödinger's equation.[26] As far as Schrödinger's equation goes, then, the laws of quantum mechanics have no preference for either temporal direction, and quantum mechanics does not appear to provide support for arguments for a *lawlike* temporal asymmetry.

Although the great majority of investigators would agree with the above official line, there are exceptions. Craig Callender for one objects to the blurring of the distinction between *T* invariance and 'Wigner' invariance. The blurring is able to occur because of the usual claim that the physical content of quantum theory is exhausted by the probabilities. Davies, for example, notes that simply replacing t by $-t$ does not give an equation which would display time reversal symmetry. However, he writes that since a solution of the Schrödinger equation (the wave function) is not itself observable, time reversal symmetry can be restored by simply reversing the sign of the i [effecting a 180° phase shift – reversing the 'handedness'], while leaving the physical content unchanged.[27] As Callender puts it,

> The idea is simply that the observed configurations can only tell us about the absolute value of ψ, and not about ψ itself. That is, since the predictions are made using Born's rule, $\rho = |\psi|^2$, the probability that a state ψ will have a certain value equals the probability that a state ψ^* will have the same value.[28]

Callender's response to this claim is that 'although it is true that the observable content of the theory is given by Born's rule, unless we resort to operationalism or verificationalism, this is not relevant. Arguably, according to all the major interpretations of quantum mechanics, ψ is a genuine part of the ontology of the quantum world'.

Taking the second part of Callender's response first, it seems a little misleading. We have already seen (Chapter 1) that in Bohr's original Copenhagen interpretation, the *way* in which ψ is part of the 'ontology' of the quantum world is that it refers to a statistical ensemble of measurement results (of identically prepared systems), which is entirely consistent with an operationalist/verificationalist approach. Bohr was strongly against detailed ontology.[29] For Bohr, there is only the total unanalysable experimental behaviour, and no

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26 See Bjorken & Drell 1964, p. 72 for the equivalent statement in the context of Dirac's relativistic wave equation.

27 Davies 1974, p. 156.

28 Callender 2000b, p. 263.

29 Jaynes writes, 'To the best of our knowledge, the closest he [Bohr] ever came to making an ontological statement was uttered while perhaps thrown momentarily off guard under the influence of Schwinger's famous eight-hour lecture at the 1948 Pocono conference. As recorded in John Wheeler's notes on that meeting, Bohr says: "It was a mistake in the older days to be discontented with field and charge fluctuations. They are necessary for the physical interpretation."' (Jaynes 1990, p. 394.)

way to discuss in detail what this could mean ontologically.[30] Dirac, too, regarded ψ as a mathematical entity measuring probability. He emphasizes that 'science is concerned only with observable things',[31] and defines the quantum-mechanical conception of 'state' in terms of probabilities of observation.[32]

As for the first part of Callender's response, I'll say three things about it. The first is that the response amounts to asking what the justification is for calling a law *T* invariant if we find that even after we've changed the sign of the *t* in it, the law is still not *T* invariant – that we also need to put in by hand a reversal of handedness before we obtain a wave function which satisfies the Schrödinger equation. That's a good question. The two symmetries seem different because, as Callender says, 'a symmetry is defined by its operations on states. Because the two operations are different, the two symmetries are different'.[33]

A somewhat similar situation arose with the breakdown of the law of parity in 1956. Symmetry was quickly restored by carrying out both the parity operation *P* and the charge conjugation operation *C* on the system in which *P*-conservation fails (see §2.4 below). Even after the breakdown of *P*, however, physicists continued to say that nature shows no bias between right and left; 'nature's own' mirror is not the *P* mirror but a *CP* mirror. Symmetry was thereby restored.[34] Significantly, though, the restored symmetry was no longer *P* symmetry, but *PC* symmetry (or *T* symmetry). Taken individually, neither *C* nor *P* are true symmetry operations[35] ('true' in the sense, I think, of being universally applicable). An analogous situation, highlighted by Callender, seems to exist in the case of the Schrödinger equation. So then, rather than speaking of the *T* invariance of Schrödinger's equation, should we speak instead of its 'Wigner invariance'?

It seems clear that the only justification for calling Schrödinger equation *T* invariant is the assumption that the physical content of the theory *is* exhausted by the probabilities. Recall that in §2.2(3) we specified that the *T* invariance of theories is determined by the reversibility of the processes they describe, and that the test of reversibility is whether the theory provides identical algorithms for calculating future and past physical situations of the world from its present physical situation. In the case of intrinsically probabilistic theories, this requirement takes the form that the theory must provide identical algorithms for calculating *probabilities* toward both the future and the past (§2.2[6][ii]). Now, given our criterion of *T* invariance, and given also that the physical content of the

30 See e.g. Bohm & Hiley 1993, pp. 17, 137.
31 Dirac 1935, p. 3.
32 Dirac 1935, pp. 11-14.
33 Callender 2000b, p. 263.
34 Until the apparent breakdown of *CP* symmetry, which was in turn restored by *CPT* symmetry.
35 Wigner 1967, p. 59. See also Gardner 1982.

theory really is exhausted by the probabilities, it follows that Schrödinger's equation is indeed T invariant.

The second, related thing to be said about Callender's response (i.e. 'although it is true that the observable content of the theory is given by Born's rule, unless we resort to operationalism or verificationalism, this is not relevant...') is that this part of the response seems to commit him to the position that quantum mechanics is incomplete, and to a hidden variable theory of some kind. That's because the entire question of whether or not the wave function (say, of an electron) is a *complete* description of its state turns on the unknowability of the overall rather than relative phase of the wave function. According to the standard interpretation, questions about the overall as opposed to relative phase have no physical significance. That being so, Callender cannot here be arguing for the T non-invariance of the Schrödinger equation in the *standard interpretation,* according to which the physical content of the theory *is* exhausted by the probabilities, but in some other interpretation (some kind of Einsteinian realist interpretation) which holds that quantum mechanics is not complete.

In the above regard, the quantum wave function $\Psi(x,t) = re^{i\theta}$, representing a free particle such as an electron moving in the absence of an accelerating field of force, consists of two wave components at right-angles to each other in a complex plane. Rotating the wave about its axis is equivalent to a phase change (Appendix[k]). Moreover, for a normalized wave, the sum of the squares of their amplitudes is always 1, regardless of the overall phase (Appendix[h]). Thus, a measurement never gives information about the overall phase. This is a rotational symmetry property. Another way of describing this property is by saying that electron waves possess the property of *invariance under phase shift*.[36] In particular, the Schrödinger equation utilizes only phase or frequency *differences* in its time-dependent factor.[37] The quantum-mechanical 'pure ensemble' differs from a classical mixture of states just in that in the former the *phases* of the a_n in $P_n = |a_n|^2$ remain indeterminate, whereas this ignorance is not the case in the latter.[38]

The third thing I shall say about Callender's response is that Born's rule states that the probabilities are obtained by multiplying the wave function by its *complex conjugate*, the result always being equal to the absolute square of the

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36 After Watson 1990, p. 42.

37 The reference energy for the potential is arbitrary. (Longini 1970, p. 25.) Likewise, even with classical waves, the phase can be detected only as a difference between the phases of two waves. That's because any position on the phasor can be chosen as zero degrees. The invariance of physical measurement values under a phase shift corresponds to a conservation law, namely the conservation of electric charge. (Bernstein & Phillips 1981, p. 96.) The phase can shift and not affect the charge because of 'gauge symmetry' [phase symmetry] which compensates for such changes by creating virtual photons, whose electromagnetic field cancels out the effects of phase change. (Crease & Mann 1986.)

38 Goswami 1997, p. 522.

wave function. Callender ignores the first part of the prescription. It may be that we need to focus more directly on why the probabilities are obtained in such a peculiar way. In that connection we may also need to go to relativistic quantum mechanics to make more progress. We shall touch on these matters again in Chapter 5 and return to them more directly in Chapter 6.

2.4 Conservation laws & symmetries; the *CPT* theorem

We considered above the question of the *T* invariance of the Schrödinger equation. *T* invariance is an instance of a symmetry principle. We shall now take a brief look at the role of symmetry in the context of the laws of quantum mechanics.

In physics, the fundamental laws of nature are said to be *translation invariant*. By 'translation' is meant transference from A to B, where the transference could be in *space*, e.g. from 'here' to 'there', or in *time*, e.g. from 'now' to 'then'. Either way, *quod semper, quod ubique,* as used to be said of theology. Additionally, the laws of nature are also *rotation invariant*. Rotation invariance may be regarded as a special case of translation invariance.

A symmetry is an expression of *equivalence* between things. Each point on the circumference of a circle, for example, is related to the other points in the same way – and is thus equivalent to every other point. Move the circle to another location, or rotate the circle (or walk around it) and it remains unchanged. We say that the circle is both translation and rotation invariant. Both invariances are *symmetry principles*. An object is symmetrical if there is an operation we can perform on it such that when we have finished, it looks unchanged.

Symmetry principles imply *conservation laws*. Examples are the conservation of charge, conservation of energy, conservation of linear and angular momentum. The conservation of these quantities is associated with the fact that it doesn't matter where or when experiments involving the conservation of these quantities are carried out, or which way the experimental apparatus is oriented or rotated. They are always conserved.[39]

So attractive has the siren song of symmetry become that physicists nowadays identify laws with exact symmetries found in the world. Laws are *defined* in terms of those things we can do to the world that leave it essentially unchanged. Thus symmetry principles have been elevated to a higher station than the traditional differential equations that merely describe *how* things change. The physicist John Barrow writes, 'What could be simpler as a law of Nature than the statement that *nothing changes*?'[40]

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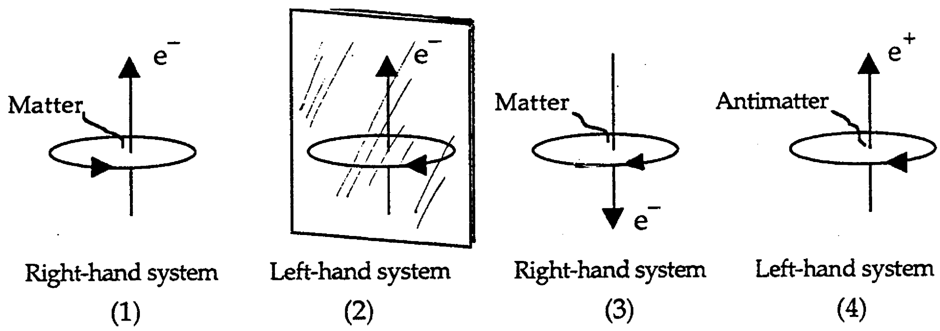
39 This paragraph and the two preceding it are loosely after Polkinghorne 1979, pp. 36-8.

40 Barrow 1988, p. 115.

Up to 1956, physicists believed that the laws of nature were invariant not only under translations and rotations but also under *reflections*. It seemed intuitively obvious that nature does not have a preference in a fundamental way for right-handedness over left, or vice versa. If we were to watch the reflection of a physical experiment in a mirror, the mirror image ought to show the same laws operating in both the actual experiment and its mirror reflection. Technically, this 'left-right' symmetry requirement is known as conservation of parity ('*P*').[41] But in 1956 it was shown that *P* is not conserved in weak interactions.

The cobalt-60 nucleus is β-decay unstable. It was found that there is a handedness in the direction in which radioactive Co^{60} emits electrons and antineutrinos when surrounded by a strong electric current. The Co^{60} nucleus emits electrons predominantly in a direction described by the right hand rule, parallel to the magnetic field lines generated by the current, and antineutrinos antiparallel to them. (The field enables the nucleus to 'know' which way it is oriented in relation to the current, and the nucleus tries to align its spin [which is an axial vector] antiparallel to the magnetic field [an axial vector]. The emitted electrons and antineutrinos, in turn, try to align their momenta [polar vectors] antiparallel and parallel, respectively, to the spin of the nucleus [axial vector].[42]) The consequent correlation of an axial vector with polar vectors is clearly not reflection invariant, and implies a breakdown of left-right symmetry – as is immediately evident if the process is observed in a mirror.



The mirror reverses the flow of current. But the decay products in the mirror image (2), continue to be emitted in the same directions as in the actual process (1). Now, if we actually carry out the experiment shown in the mirror image, i.e. reverse the direction of current flow, we never get (2). Instead, we always get (3). Reflection symmetry (*P*) fails. What is going on? Surely the laws of physics ought to be invariant under reflections.

41 Parity is a mathematical property of the quantum wave function (related to but not equivalent to mirror reflection invariance) and it has two values – even and odd. (If a wave function remains unchanged when the sign of one of the three spatial variables is reversed, it has 'even' parity, if not, it has odd parity; more specifically, eigenfunctions satisfying the relation $\Psi(-x, -y, -z) = \Psi(x, y, z)$ are said to be of even parity, while eigenfunctions satisfying $\Psi(-x, -y, -z) = -\Psi(x, y, z)$ are said to be of odd parity.)

42 Polkinghorne 1979, p. 41.

It is possible to restore symmetry by a *double* inversion – viewing the system in a mirror *and* reversing the signs of all charges in the mirror image (the latter known as 'C' or 'charge conjugation') – equivalent to replacing matter by antimatter. Imagine that the Co^{60} nucleus is replaced by an *anti*-Co^{60} nucleus – a nucleus of opposite charge – and also the emitted electron and antineutrino are replaced, respectively, by a positron and a neutrino. The mirror image then shows the process depicted in (4) above – a process that is as probable as the actual experiment (1). It is concluded that weak decays are asymmetric under *P* alone, but they are symmetric under combined charge and parity reversals (they are '*CP* invariant'). As it turns out, *CP* invariance implies *T* invariance (see below), so the weak interaction is also symmetric in time. It is sometimes said, apropos the charge reversal, that in 'nature's own mirror', reversal of handedness is always accompanied by charge reversal (whereas in an ordinary mirror, of course, only the handedness is reversed).

It has been speculated, on account of the exact cancelling out of any changes under *P* and *C*, that charge conjugation is nothing but reversal of handedness.[43] But there is an additional complication, suggesting that *T* must also be somehow involved.

In 1964 it was found that *CP* symmetry is violated in another weak process, neutral kaon (K^o) decay, the details of which need not be given here.[44] The important consequence for our purposes is that violation of *CP* symmetry entails a violation of *T* symmetry, owing to a theorem of relativistic quantum mechanics, known as *CPT*. The *CPT* theorem states that in circumstances that are so general that they include any realistic quantum field theory, the combined transformation of charge conjugation *C*, parity *P* and time reversal *T* has to be a symmetry of the theory.[45] That is, for any physical interaction between particles there exists another between their antiparticles with left and right interchanged and the sign of time of the interaction reversed. If *CPT* is a symmetry of the theory, it immediately follows that a violation of the symmetry of any two of the elements of *CPT* *ipso facto* violates the symmetry of the remaining element alone. Thus a violation of *CP* is a violation of *T*.[46]

Returning to *CP* symmetry, consider again the process depicted in (4) above. It shows the same process that is depicted in (1) – but *after* the application of

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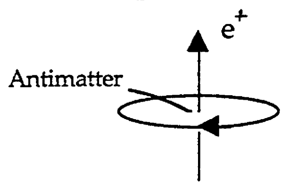
43 For an account of such speculations, see Gardner 1982, pp. 226-8.

44 For an account, see Davies 1995, pp. 208-13.

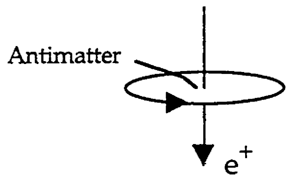
45 Eisberg & Resnick 1974, p. 702 put it as follows: '[F]or *any* system governed by *any* interaction that conforms to the relativistic requirement that cause must precede effect, the result of successively carrying out the charge conjugation operation, the parity operation, and the time-reversal operation is to leave the essential description of the behaviour of the system unchanged.'

46 It seems that on the quantum level, nature, in the shape of the neutral kaon particle, can distinguish in an objective way between the two directions of time.

CP: in the *CP* conjugated process there is a positron emitted upwards and a neutrino downwards, and reversal of the flow of current to that in (1). Now apply *T* to the *CP* conjugated process [i.e. to (4)]. The result, shown in (5), is a reversal of all velocities – there is a positron being emitted downwards and a neutrino emitted upwards, and a reversal of the flow of current to that in (4).[47]



Left-hand system
(4)



Right-hand system
(5)

The process depicted in (5) is just as probable as that in the actual experiment (1). The two are *CPT* conjugates of each other. Moreover, the electron and antineutrino of (1) may be thought of as a positron and a neutrino travelling backward in time, which is identical to (5), and vice versa. As Polkinghorne remarks (1979, p. 47), 'we cannot tell the difference between a matter system directly observed and a film of an antimatter system being run backwards and viewed in a mirror'.

Three noteworthy points emerge regarding time reversal in quantum mechanics.

(1) The first is that the asymmetries revealed by the breakdown of *P* and *PC* (i.e. *T*) are *factlike* asymmetries – everything to do with the initial conditions, and nothing to do with the laws *per se*. Given that *CPT* is true, it can be maintained that the *laws of nature* remain symmetric, despite the apparent breakdown of symmetry in our (human) laws of physics. This can be illustrated by reference to the failure of Newtonian mechanics. It was well-known before Einstein that Newtonian mechanics and Maxwellian electrodynamics were incompatible. The latter showed the irrelevance of absolute velocity in all electromagnetic experiments. One of the two had to go. It was Newtonian mechanics that went, replaced by relativistic mechanics. It turned out that the laws of Newtonian mechanics aren't fundamental laws at all, but 'laws' applicable only to a limited (human scale) range of phenomena, within which the fact that it gave wrong answers wasn't evident. The errors were too small to be noticed. Hence the incompatibility between the laws of mechanics and Maxwellian dynamics was only apparent – it was 'factlike' rather than

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47 Note that in each of charge conjugation, parity reversal, & time reversal, the sense of circulation of *charge* is reversed. These are improper transformations, meaning that there can be no smooth, continuous transition from positive charge to negative charge, or a left-handed system to a right-handed system, or from forward in time to backward in time.

lawlike, much in the nature of an *anthropocentrism*. There was no violation of any *law*.

Similarly, physicists were mistaken in believing that *P* and *CP* were true symmetries of the laws of nature. It is believed that only *CPT* is a true symmetry of any fundamental law. So the violation of *C* and *CP* is 'factlike' rather than 'lawlike' in the sense that no violation of *law* is involved. There is no violation of any genuine *non-human* symmetry. The violation is rather of our anthropocentric notions. Once we recognize the true underlying symmetry, all we need to do is to apply the appropriate boundary conditions to the relevant experiments showing the supposed failure of symmetry, and symmetry will be restored, i.e. symmetry will be seen to have existed all along. (In the case of the *P* violation, the appropriate boundary condition is to reverse the signs of charge etc. in the mirror image, as I've explained. Likewise, in the case of *CP* violation, the appropriate boundary condition is to apply the symmetry *T*.)

(2) The second point concerns the definition of an antiparticle (if we may extrapolate from what has been said about processes to *particles*). Strictly speaking, as Jeremy Bernstein observes, an antiparticle must be defined as the *CPT* conjugate of a particle and not simply the *C* conjugate.[48] Charge conjugation appears to involve not only reversing the sign of charge together with effecting a left-right reversal, but *also* running the system backward in time (to put it crudely).[49] We can perhaps understand this in an intuitive way as follows: There is a left-right reversal just because the particle is running backward in time. As for the charge conjugation, that's how we see a charged particle that has undergone a left-right reversal owing to it running backward in time. (Owing to our anthropocentric perspective as creatures *in* time, we don't perceive directly the backward-in-time nature and reversed handedness of the particle; instead we simply perceive a particle of opposite charge pos-

48 Bernstein 1989, p. 40.

49 'Running the system backward in time' is of course only convenient shorthand. More technically, one would say that the world line of the backward-in-time system lies along the same four-vector as that of the forward-in-time system, but with an opposite time direction. Moreover, as mentioned in the Introduction, the direction of time is a *global* property, like the direction of causation. Draw a line, or better, a system of one-way branching lines, on a blank sheet of paper. What is their direction? To answer this, reference will need to be made to something *outside* of the paper – to external considerations. It is like the order, or lack of order, of a deck of cards. No amount of scrutiny of the markings on the cards alone can tell us whether the deck is highly ordered or not. We need to make reference to information outside of the cards themselves, namely a coding schema. Likewise, if we draw a system of branching world lines on a space-time map, with the overwhelming majority of the branching in the same direction, we are unable to tell by looking at the world lines themselves whether their direction is forward or backward in time, without reference to something outside of the world lines themselves. (What that something *is*, is a matter of ongoing debate.)

sessing the usual time sense.)[50]

(3) The third point is a corollary of (2). Just as an antiparticle is, strictly speaking, the *CPT* conjugate of a particle and not simply its *C* conjugate, so also, by the same token, the *T* reverse of a particle is, strictly speaking, the *TPC* conjugate of a particle. That is, in 'nature's own *T* mirror', *T* reversal implies *TPC* reversal.[51] (By 'nature's own mirror', I mean the symmetry applying to fundamental laws of nature.) To 'time-reverse' a physical system, when the system is described in terms of *fundamental* law, involves applying not only *T*, but also *P* and *C*. No fundamental law contains *T* symmetry alone, without also containing *C* and *P* symmetry.

A fundamental law is a law containing no anthropocentrisms. Quantum theory is a candidate for such law, which is consistent with its counterintuitive, inhuman aspect. Quantum theory is indifferent as to whether an electron is described as an electron travelling forward in time or a positron of opposite spin travelling backward in time. Likewise, it says the same thing about Kant's right-hand glove floating along in space as about its mirror-reverse (i.e. a left-hand glove) made of antimatter floating along in space backward in time. It is indifferent as to which description is chosen. The individual elements *T*, *P* and *C* of *TPC* are anthropocentrisms, not existing independently of each other in a fundamental law.

So what is the upshot of the *CPT* theorem for our understanding of the laws of physics? Coveney & Highfield put it as follows: 'Loosely speaking, the *CPT* theorem contends that the laws of physics predict equal but opposite events in a kind of 'generalized mirror image' world.'[52]

This will be of relevance to the discussion in Chapters 5 and 6, where I argue that the universe is such a generalized 'mirror-image' world.

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50 Of course, this doesn't tell us what charge is.

51 The present section illustrates the history of physicists' search for symmetry. The most striking feature of that history is a move away from anthropocentrism. The search for symmetry is closely related to the search for fundamental laws of nature, and ultimately, a unified theory in which all the forces are described by a single law. The possibility of unification is taken as an article of faith. In much the same way, physicists treat the symmetry of the laws as if it were an *a priori* principle. Consequently, when a particular lawlike symmetry such as *CP* unexpectedly fails, everyone is shocked and puzzled – but faith in underlying symmetry remains undiminished. It is reasoned that the symmetry that failed wasn't a deep symmetry of laws at all, but more in the nature of an anthropocentrism, applicable to some of the 'laws' in their present, imperfect form but not to *all* of them. In contrast, *nature's own* symmetries must be applicable across the spectrum of laws. Indeed, it is thought that ultimately there is only *one* law, as the doctrine of unification teaches. Clearly , not *P* nor *C* nor *T* nor any combination of any two of these can be a symmetry of such unified law (or theory). All three symmetries must be involved.

52 Coveney & Highfield 1991, p. 139.

Moving on, we have seen that the quantum wave function can change over time in two very different ways. It can evolve *continuously* and deterministically as a solution to the time-dependent Schrödinger equation, or it can undergo a *discontinuous change* (collapse) as a result of a measurement. We've seen that the Schrödinger equation itself is T invariant, given our criterion of T invariance and that the physical content of the theory is exhausted by the probabilities. But so far we've said little about the collapse. Does the collapse of the wave function provide support for a lawlike temporal asymmetry? More specifically, does quantum theory provide identical algorithms (or instruction sets) for inferring *probabilities* forward and backward? We shall now take a look at this question with reference to specific arguments in the literature.

2.5 Does the collapse of the wave function render quantum mechanics time reversal non-invariant?

Most proponents of the standard interpretation seem to be agreed that quantum mechanics is a T invariant theory, despite indications from experiment (to do with K^0 decay) that not all elementary processes are reversible.[53]

It is also generally agreed that, even though the Schrödinger evolution of the wave function itself is T invariant, a consequence of wave function collapse upon measurement is that the processes described by quantum mechanics are irreversible. The irreversibility attendant upon measurement is usually reconciled with the presumed T invariance of the theory by noting that the irreversibility in question is all to do with the boundary conditions, and not the laws *per se*. The idea is that the irreversibility is 'factlike' (§2.2), not lawlike, arising from the use of different boundary conditions in the two temporal directions, not the laws themselves. Quite generally, if a theory is to be shown to be T non-invariant, it is necessary to show that boundary conditions have not entered into the argument for the alleged non-invariance in an essential way.

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[53] See e.g. Aharonov, Bergmann, & Lebowitz 1964; Longini 1970; Davies 1974 & 1977; Sachs 1987; Aharonov & Vaidman 1990; Leggett 1995; Savitt 1995; Stamp 1995; Unruh 1995.

The fact that quantum mechanics does not itself pick out a direction in time was recognized by Aharonov, Bergmann, & Lebowitz in 1964 . For discussion, see e.g. Unruh 1995, pp. 49-53, 61-5; Belinfante 1975, pp. 55-8ff., 68-88, 95-6.) Callender, however, is of the opinion that the conclusions of Aharonov, Bergmann, & Lebowitz in this regard (arrived at on the basis of their 'time symmetric formalism') are not particularly relevant, as they do not tell us anything about the status of the standard formalism as a fundamental law; the ABL formalism is just a reformulation of the mathematics of quantum mechanics. (Callender 2000b, p. 259.)

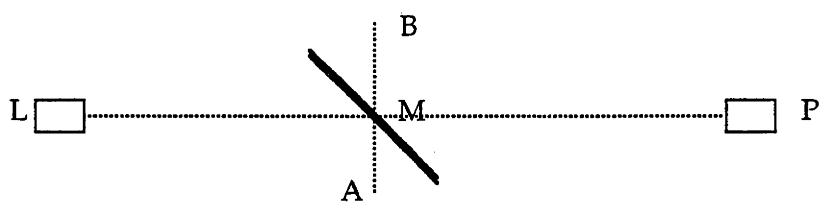
Aharonov & Vaidman (1990, p. 12) say that 'the difference between past and future is not an intrinsic property of quantum theory, but it is the feature of our approach to the arrow of time: at present we view the past as existing and future as nonexiting (yet)'.

Even though the *T* invariance of quantum mechanics seems to be generally accepted, opinion is by no means unanimous. Arguments surface from time to time purporting to show that the collapse of the wave function renders quantum mechanical systems irreversible in a *lawlike* way, with the consequence that quantum mechanics is a *T* non-invariant theory. Penrose makes such a claim in 1987, writing that 'state vector reduction – or the more complete process that underlies it – is... a time-*a*symmetrical process'.[54] He repeats this claim in his 1989 book.[55] Albert also argues in a recent book that quantum mechanics is not invariant under time reversal.[56] Callender, too, believes that the probabilistic algorithm used in quantum theory picks out a preferred direction for time in nearly all interpretations. (We have seen [§2.3] that he goes further and argues that even the Schrödinger evolution is *T* non-invariant.)

Huw Price is another notable exponent of the view that quantum mechanics in the standard interpretation is objectively asymmetric (though the reason for Price's claim differs from that of Penrose in an important way). Let us look at the claims of Penrose and Price more closely, and in the process, at Callender's criticism of both. We start off with that of Penrose.

2.5.1 Penrose's argument for the objective asymmetry of quantum mechanics

It seems clear, given our criterion of *T* invariance for probabilistic theories in §2.2, that the only way to successfully challenge the *T* invariance of quantum mechanics would be to show that the laws of the theory (and here I include not only the equations but also the theory's other postulates such as the collapse of the wave function), give different answers when applied in the forward and backward temporal directions – implying that quantum mechanics implicitly contains a time ordering. That is Penrose's strategy. He describes an idealized thought experiment consisting of a hot lamp L emitting photons, a photon-detector P a little distance away, a half-silvered mirror M at an angle of 45° in between them, and a pair of walls A and B on either side of L, M and P.



Every now and then L emits a single photon. Whenever it does, L registers a

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54 Penrose 1987, p. 37.

55 Penrose 1989, p. 355-9.

56 Albert 2000, pp. 14, 132-3. Albert goes further and argues (p. 14) that the same goes for *any* of the candidates for a fundamental theory that anybody has taken seriously since Newton.

photon event (say by an audible click and a little waving flag showing the clock time of the event). Likewise, the absorption of the photon is registered in an identical way. At the instant of absorption the wave function collapses. Consider one of the emitted photons, emitted at, say, $t = 0$. According to quantum mechanics, the photon's wave function travels to M, at which point (loosely speaking) it bifurcates so that there is equal probability of the photon going through the mirror to P, and of being reflected by the mirror to a spot on wall A. Suppose that P registers at $t = 1$, indicating that the photon has gone there. (It is virtually certain that the photon came from L and not from the laboratory wall at B, since L is hot and B cold, especially if neither A nor B registered a photon event in the interval $t = 0$ and $t = 1$.) So it is as good as certain that a photon has left L and arrived at P.

Now consider the emission of a photon with the same energy in the opposite direction. The wave function leaves from P (which registers at $t = 1$, indicating a photon event) and goes to M, where it bifurcates so that there is equal probability of the photon going through the mirror to L (where it would be absorbed, the wave function collapsing[57]), and of its being reflected by the mirror to a spot on the opposite wall B (absorption and concomitant collapse of the wave function). That is, the probability of the photon actually returning to L is one-half. Yet the probability ought to be one according to Penrose, rather than one-half, seeing that we already *know* that in the forward-time case, the photon left L at $t = 0$ and arrived at P at $t = 1$ – the time-reverse of which *must* be that the photon leaves P at $t = 1$ and returns to L at $t = 0$. So, says Penrose, the quantum-mechanical calculation has given 'completely the *wrong* answer' – 50% probability for each of the two possibilities, instead of the right answer which would be 100% for the one and 0% for the other. He continues, 'The standard rules of quantum mechanics were obtained by observing... the way in which the probabilities behave in the normal direction of time. These particular quantum-mechanical rules for calculating probabilities simply *do not work* when used in the reverse direction of time.'[58]

What are we to make of this? Well, Penrose seems muddled. His thought experiment does illustrate the fact that photon emissions/absorptions are not reversible in the strong sense described in §2.2(6)(i). But *no* intrinsically indeterministic processes can be reversible in that sense. So that is not contentious. Penrose's experiment is consistent with photon emissions/absorptions being reversible in the *weak* sense, though §2.2(6)(ii). Take a movie with sound of each photon emission event from L to P and A. Put to one side for the moment all

[57] Notice that the wave function *collapses* at the lamp L in this reverse-time picture, whereas in the normal picture it begins to *spread out* at L following the lamp's emission of a photon. And vice versa at the photon detector P.

[58] Penrose 1987, p. 41.

those clips in which the photon goes to A. All the remaining clips show a photon being emitted from L and going to P. (There is a registration at L *first*, and a registration at P at a *later* time.) Play each of these backwards. Each of the backward-played clips shows a *physically possible* sequence of events, namely the photon being emitted from P (P registers first) and going to L (L registers after P has registered). Now do the same for the clips in which the photon goes from L to A. Again, each of these clips, when played backward in time, will show a physically possible sequence of events (a photon being emitted from A and going to L). Consequently, the process is reversible in the weaker sense, consistent with quantum mechanics being a *T* invariant theory. Of course, it is not *probable* that all the photons would end up at L. But that's not required by our definition of *T* invariance.[59] Our definition merely requires that the theory provide identical algorithms for inferring *probabilities* toward both the future and the past.

So *does* quantum mechanics provide identical algorithms in the light of this gedankenexperiment? Consider another apparent difference in the probabilities predicted by the theory in the forward and backward time directions: In the forward time direction, the photon has an equal probability of going to P or A, with B being ruled out. In the backward time direction it has an equal probability of going to L or B, with A being ruled out. Does *that* difference in the probabilities show that quantum mechanics doesn't provide identical algorithms for inferring probabilities in the two temporal directions? I think not. Its explanation seems in principle no different from the explanation of what generally happens in the forward time direction alone when the boundary conditions are changed. Change the boundary conditions in significant ways and you change the predictions of the theory. The difference is nothing to do with time reversal *per se*.

What are these boundary conditions? There is the entire experimental configuration, in particular the mirror M's orientation relative to L and P. And there is the thermodynamic asymmetry in the experimental conditions arising from the fact that L is hot and P, A, B are cold, evidenced by the fact that over many runs of the experiment we always hear L click *before* P (or A) clicks, and we never hear B click. (As regards this thermodynamic asymmetry in the experimental set-up, Penrose admits he has not been concerned to see how far one can go with actually time reversing the *entire* experiment, including the registering of all the measurements involved, or how much physical sense there would be to doing so.[60] Consequently he still retains the above, normal sense of 'before' and 'after'.)

Consider again the absorption and the concomitant registration of a photon by P in time reverse, as Penrose does in his thought experiment. We're here talk-

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59 Such a possibility is also mentioned by Savitt 1995, p. 17.

60 Penrose 1989, p. 359. We shall go on to look at the question of how far one can go with actually time reversing an entire system in Chapters 4 & 5, in the context of Price's advanced action proposal.

ing about a new, and different experiment, with relevantly different boundary conditions, owing to which the laws of quantum mechanics entail different probabilities. In this new experiment, quantum mechanics predicts that the emitted photon has an equal probability of going to L or B. The irreversibility and the concomitant temporal asymmetry revealed by Penrose's gedankenexperiment is essentially factlike, not lawlike, crucially relying on an asymmetry in the experimental configuration in the two temporal directions. The probabilities differ simply because the experiments are not identically prepared in the two temporal directions.

Callender says as much when he refers to a simple experimental set-up described by Savitt 1995,[61] which shows that it is easy to build a classical version of Penrose's experiment with exactly the same frequencies as Penrose's one, even though classical mechanics is taken as the paradigm of *T* invariance. If Penrose's experiment demonstrates the *T* non-invariance of the laws of quantum mechanics, then by the same token it also demonstrates the *T* non-invariance of the laws of classical mechanics, which is nonsense. The lesson, according to Callender, is that the observed frequencies by themselves tell us nothing about whether the theory is *T* invariant or whether time is 'handed'. One can find all sorts of asymmetric phenomena in quantum mechanics, but then, so can one in classical mechanics. 'To see if quantum mechanics is interestingly different than classical mechanics in this regard, we therefore need to look at the laws and ontology of the theory.'[62] That seems right. As Savitt put it, the answer will depend on broad theoretical considerations, rather than simply counting the results of one run. Among the theoretical considerations, one might add, is the question of whether the wave function is to be interpreted as referring to an *ensemble* or to an individual system (and if the latter, whether it may nonetheless be correctly interpreted as referring to an *ensemble of measurements* of identically prepared systems).

This takes us to Price's gedankenexperiment. Price's experiment specifically concerns itself with the ontology. We now turn to a detailed consideration of Price's claim.

2.5.2 Price's argument for the objective asymmetry of quantum mechanics

Price's argument in support of his claim has many of the elements of Penrose's argument, with one crucial difference, which I shall identify when we come to it.

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61 Savitt 1995, p. 18. Replace the lamp by a mechanism that fires ball bearings, and replace the half-silvered mirror by an aperture that is open for exactly half the duration of any experimental run. No connection or correlation is to exist between the device controlling the aperture and that firing the ball bearings. Take a movie of the process and play it backwards. Compare the frequencies in both directions.

62 Callender 2000b, p. 257.

It is that difference which makes the argument of particular interest.

The claim may be summarized in brief as follows. According to the standard interpretation of quantum mechanics, the state of polarization (or spin) of a photon that has passed through a polarizer is wholly determined by the orientation of the past polarizer through which it passed. It does not, and cannot, depend on the orientation of any future polarizer which the photon is yet to encounter if the collapse of the wave function is to be real, and not an artefact of the theory.[63] This introduces an objective asymmetry into the standard interpretation and counts against it, as symmetry is always to be preferred to asymmetry in the absence of compelling reasons to the contrary. Here are the details.

Consider the standard interpretation's account of a previously unmeasured photon approaching a vertically angled polarizing filter (polarizer A). There is an equal chance of the photon getting through or being absorbed/reflected. If it gets through, the photon is then said to be *vertically polarized*. In the standard interpretation the photon's state of polarization after it has passed through a polarizer is always said to reflect the orientation of the *past* polarizer, never that of any future polarizer it will encounter. Presumably that is because repeating the experiment with a polarizer at the same angle as the previous one always gives the same result. This is taken to mean that the wave function describing the vertically polarized state of the photon is a *collapsed* wave function, its vertically polarized state being an *eigenfunction* of some earlier superposed state of polarization. The collapse has destroyed all traces of the previous initial conditions.

Now consider this vertically polarized photon approaching a second polarizing filter (polarizer B) placed in its path, but angled diagonally (at 45°) to the vertical (measuring clockwise from the vertical). At this angle there is an equal chance of the photon getting through or being absorbed/reflected. In other words, the outcome is random. If it gets through, it is then said to be *diagonally polarized*. Its polarization appears to have been *rotated* by 45° from the vertical to the diagonal by its interaction with the second polarizer. Placing additional diagonally angled polarizers in the photon's path do not block it. This is taken to mean that *after* the photon has passed through the diagonal polarizer, its state is one of diagonal polarization, exactly correlated with the *past* polarizer A. The wave function describing the now diagonally polarized state of the photon is (again) said to be a *collapsed* function (some eigenfunction) of its previous state. Now, we saw that its previous state was one of definite vertical polarization. So it may be wondered how its present state can be an eigenfunction of some *superposed* previous state. The answer is that pursuant to Fourier analysis, that previous *vertically* polarized state of the photon can also be correctly described as a state of linear superposition of slant (–45°) and diagonal (+45°) polarizations, i.e.

[63] See Price 1996, pp. 183, 187-8.

$$\Psi = a_1 \psi_{\text{slant}} + a_2 \psi_{\text{diagonal}},$$

where a_1 and a_2 are the amplitudes or weighting factors, the values of each being $\sqrt{0.5}$ in the present case.[64] This teaches that we don't need to take the expression 'collapse of the wave function' too literally even in the standard interpretation, at least in the present case. The passage of a photon through a rotated polarizer generally results in one linear superposition of eigenfunctions being *replaced* by another. When we speak of 'collapse' in what follows, it is to be understood in this way.

Now let us consider the above process in *time reverse*, still following the standard interpretation. (Recall that time reversal in the standard interpretation is essentially velocity reversal, together with taking the complex conjugate of the wave function. To time-reverse a process, we take the final boundary conditions of the original process, reverse all velocities and other changes, and make the result the initial boundary conditions.) We want to know if the collapse at polarizers A and B introduces a lawlike asymmetry between the forward-in-time and backward-in-time descriptions of the same process. Is the time-reversed description that of a physically *possible* process in quantum mechanics, having regard to the collapse? If not, then a collapse in one direction precludes a collapse in the other. If the collapse is to be real and not a mere artefact of the interpretation, it would follow that quantum mechanics is objectively asymmetric, i.e. it is *T* non-invariant. (The present discussion will be of especial relevance when we come to Ch. 4, and Price's claim that the standard interpretation of quantum mechanics embodies an objective asymmetry on the microlevel for the reason that in it the wave function is collapsed *after* but not *before* a measurement interaction. According to Price, in the standard interpretation, the state of the photon in the interval between the polarizers does not depend on the orientation of the future polarizer, which the photon has not yet reached, but is wholly determined by the orientation of the past polarizer through which it passed [§4.7.2].[65])

Above, we looked at the photon passing through both polarizers in the usual time direction, first through A and then through B. A collapse occurred at both A and B. We saw that *after* the photon had passed through each polarizer, it had a polarization exactly correlated with the polarizer it had just passed through. In the time reverse of the above process, all its elements occur in reverse order. A photon travelling backward in time passes through polarizer B first and then travels toward polarizer A, through which it also passes. Now, on its way to

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64 The photon's vertically polarized state can be described as a state of linear superposition in infinitely many ways, e.g. a superposition of –50° and +40° polarization, or 0° and 90° polarization, etc. (At different angles the weighting factors would have different values. For example, at 0° the value would be 1, and at 90° the value would be 0.)

65 See Price 1996, pp. 183, 187-8.

polarizer B (from distant infinity or from wherever), the backward-in-time photon is *diagonally* polarized, since that was how the forward-in-time photon exited that polarizer (B) on its way to distant infinity or wherever in the forward-time picture (owing to that polarizer being diagonally angled). What is the state of the backward photon *in between* the polarizers, as it travels toward polarizer A after passing through polarizer B? In particular, is the photon's polarization correlated with the polarizer it has just passed through, as in the forward-time picture, or is it correlated with the polarizer it is *yet to encounter*?

Since the photon has passed through the diagonal polarizer B to get to the region between the polarizers, one might think that it must continue to be *diagonally* polarized, at least if the laws and postulates of quantum mechanics are to be applied in the same way in the two temporal directions. But in the original *forward* temporal direction (the time reverse of which we are presently looking at), the photon was objectively *vertically* polarized between the polarizers. In the time reverse of that original picture, then, when the photon passes through the diagonal polarizer B, its state must *jump* from being diagonally polarized to being vertically polarized – else presumably it wouldn't be the time reverse of that picture. (That's because we are assuming all the time that the collapse is real, and hence that in the forward picture the photon *really is* in a vertically polarized state on its way from polarizer A to polarizer B.) Consequently, if the photon does jump, the backward photon's polarization does not match that of the polarizer it has just passed through, matching instead that of the polarizer A which it is about to meet. Hence quantum mechanics embodies an objective asymmetry. On the other hand, if the (backward) photon does *not* jump, remaining correlated with the polarizer it has just passed through – as symmetry and the reality of wave function collapse *also* seem to require – the photon remains *diagonally* polarized on its way from polarizer B to polarizer A. But that is a *different* state from its forward state. But if the collapse is real, it must be the same state.

Thus, the two requirements: (a) that the collapse be real and (b) that quantum mechanics be time reversal invariant, appear mutually exclusive, entailing as they do two incompatible objective states of the photon in between the polarizers. This is the difference between Price's claim and that of Penrose: the backward process described by Penrose was at least *possible*, even if highly improbable owing to the applicable boundary conditions. (Note that although Price's explicit conclusion is that given a real collapse, the behaviour of photons and polarizers reveals an objective asymmetry in the standard interpretation, what he really takes this to show is that the notion of a real collapse is an artefact of the interpretation, and must be given up. He does not advocate for one moment giving up symmetry.)

Is that right? There are various possibilities. The following do not exhaust the possibilities, but they are the only ones I shall consider here. They are not all mutually exclusive.

(1) The first is that Price is obviously right, and little else needs be said.

(2) The second is that, yes indeed, quantum mechanics is asymmetric. However, contrary to Price, the asymmetry can be traced to boundary conditions. Measurement entails loss of knowledge of the system's previous state, which is why the system is irreversible. But that loss of knowledge is no different in principle to the loss of knowledge of exact motions in thermodynamics (such loss of knowledge in the latter case permitting a factlike asymmetry in the two temporal directions in the case of non-equilibrium systems), and so the asymmetry is factlike, not lawlike.

(3) The third is that the charge of objective asymmetry is true, but only against the von Neumann/Dirac version of the standard interpretation, not against Bohr's original Copenhagen version (to be discussed more fully in Chapter 3), which remains objectively time-symmetric since it doesn't speak of the unobserved state of the photon in the unobservable region between the polarizers.

(4) The fourth is that, in any case, in both the von Neumann/Dirac and the original Bohr versions of the standard interpretation, the wave function is to be interpreted as referring to an ensemble of identically prepared systems (§1.3.2) rather than an individual system. Price either does not contemplate or erroneously dismisses this possibility.[66]

(5) The fifth is that the claim of objective asymmetry rests on a misunderstanding of the standard interpretation's conception of 'state'. By 'standard interpretation' here and in what follows I mean the Dirac/von Neumann interpretation, as contrasted with the original Bohr interpretation. It is that interpretation which seems most vulnerable to Price's charge (talking as it does of a system's *state* even when the system is unobserved). But correctly interpreted, even that interpretation is objectively symmetric.

There is also the view that the asymmetry is not problematic, a view advocated by Callender and probably held by the majority of physicists. One can simply take the asymmetry of the projection postulate as a law of nature. Why think that new theories or laws are more likely to be true if T invariant rather than T non-invariant? Callender admits that he himself prefers symmetry and T invariant laws. However, he notes that is an aesthetic rationale rather than an epistemic one, and difficult to justify save on aesthetic grounds. I shall not enter into

66 Price 1996, p. 208.

this debate here, merely referring the reader to Price's response.[67] (However, if one were asked to justify in a phrase the aesthetic preference for time-symmetric theories to time-asymmetric ones, it would be hard to go past Bell's 'discretion and good taste, born of experience'.[68])

To sharpen up the discussion, let us for the moment ignore all the possibilities except the last one. Possibility (5) is the one we shall focus on here. We shall return to (2) – (4) once we have finished with (5). Our question is, does the wave function collapse in the standard interpretation entail an objective asymmetry?

2.5.2.1 Does the collapse entail an objective asymmetry? Part I

Before we can answer this question, it is helpful to ask another question. Just what is it in the standard interpretation that enables the vertically polarized forward photon to pass through the diagonal polarizer, i.e. to jump from one eigenstate to another? We ask this because the photon is supposed to be definitely in a state of *vertical* polarization. But a diagonal polarizer allows only *diagonally* polarized photons to pass through. So at first sight it seems impossible for the photon to be passed by it. Is it that the photon somehow coexists in both states? Yes. When the photon is in a state of vertical polarization, that state may also be correctly described as a linear superposition of states of diagonal and slant polarization, with some *amplitude* of each. The diagonally polarized component (eigenfunction) of the superposed wave function passes through the polarizer, whereas the slant component is blocked. This is analogous to what happens classically. If the photon were replaced by a classical electromagnetic wave, only the diagonal component, $\varepsilon_o \cos \theta$, of the electric field would get through. The energy of the wave would be attenuated accordingly.[69]

In the classical theory, then, only some fraction of the energy that is sent comes through the polarizer. But in quantum mechanics, there is no such thing as a fraction of a photon. Instead, quantum theory says that *all* the energy comes through *some fraction of the time*. The *relation* of the two theories seems clear (as Feynman remarks), even if we don't really understand the quantum mechanics.[70] Let us look more closely at the quantum mechanics, following Feynman. With what probability will the second polarizer let the vertically polarized photon through?

> The answer is the following. After it gets through the first polaroid, it is definitely in the [vertical] state $|x'\rangle$. The second polaroid will let the photon through if it is definitely in the [diagonal] state $|x\rangle$ (but absorbs it if it is in

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67 Price 1997, pp. 78-85.
68 Bell 1987, p. 174.
69 Feynman, Leighton & Sands 1965, III-11, p. 10.
70 Feynman, Leighton & Sands 1965, III-11, p. 11.

> the [slant] state $|y\rangle$). So we are asking with what probability does the photon appear to be in the [diagonal] state $|x\rangle$? We get that probability from the absolute square of amplitude $\langle x|x'\rangle$ that a photon in the [vertical] state $|x'\rangle$ is also in the [diagonal] state $|x\rangle$.[71]

Let me emphasize this. A photon definitely in the *vertical* state $|x'\rangle$ *is* also definitely in the *diagonal* state $|x\rangle$ (with a certain probability). Feynman goes on to obtain the probability rule $P = \cos^2 \theta$.[72]

Dirac, too, writes that 'whenever the system is definitely in one state we can consider it as being partly in each of two or more other states. The original state must be regarded as the result of a kind of *superposition* of the two or more new states, in a way that cannot be conceived on classical ideas'.[73]

Now look at the backward case, bearing in mind what has been said about the forward case.

Our problem was: given a real collapse, are the quantum mechanical descriptions of the photon's forward and backward states mutually incompatible? In the light of the foregoing, we reply: no. That is because the backward photon's state of diagonal polarization is correctly described as a linear superposition of states of vertical and horizontal polarization, with a certain amplitude of each. The standard interpretation takes this to mean that the backward photon is (a)

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71 Feynman, Leighton & Sands 1965, III-11, p. 10. Following Dirac, the quantum wave function is often denoted by the symbol $|\ \rangle$, called a 'ket'. The other half of the symbol is $\langle\ |$, called a 'bra'; hence the symbol $\langle\ |\ \rangle$ ('bra-ket'), representing a scalar product. A ket is the initial state and bra the final state of a system. The scalar product is the probability amplitude that we start with the state represented by the ket and end up with the state represented by the bra.

(In the case described by Feynman, the experimental arrangement differs trivially from ours, in that the first polaroid is diagonal and the second vertical; accordingly $|x'\rangle$ designates a diagonal state, and $|x\rangle$ a vertical state. But everything Feynman says applies *mutatis mutandis* to our case.)

72 'What is $\langle x|x'\rangle$? Just multiply $(|x'\rangle = \cos\theta|x\rangle + \sin\theta|y\rangle)$ [i.e. $|x'\rangle$ expressed as a linear superposition] by $\langle x|$ to get

$$\langle x|x'\rangle = \cos\theta\langle x|x\rangle + \sin\theta\langle x|y\rangle.$$

Now $\langle x|y\rangle = 0$, from the physics – as they *must* be if $|x\rangle$ and $|y\rangle$ are base states – and $\langle x|x\rangle = 1$. So we get

$$\langle x|x'\rangle = \cos\theta,$$

and the probability is $\cos^2 \theta$.'

73 Dirac 1935, p. 12. Or take Jaynes, who writes, '...we cannot merely say that the atom is "in" state u_1 or "in" state u_2 as if they were mutually exclusive possibilities and it is only we who are ignorant of which is the true one; in some sense, it must be in both simultaneously or, as Pauli would say, the atom itself does not know what energy state it is in.' (Jaynes 1990, p. 391.)

definitely in the diagonally polarized state, and (b) definitely in the vertically polarized state (*inter alia*) with some amplitude. It doesn't need to 'jump' to get into that state. It is already *in* it (with some amplitude). If its state were to be *measured* by placing an additional vertical polarizer in between the two original ones, it would be *found* to be in that state with a probability given by the absolute square of the amplitude.[74] Richard Healey writes in his review article,

> In this view [referring to the standard interpretation], a system has exactly those properties which would be revealed on measurement. These properties are assigned probability one by its wave-function; hence I shall call this interpretative principle the *wave-to-property link* [though 'eigenvalue-eigenstate link' is technically preferable]. When the wave-function of a system equals the sum of some set of wave-functions, the system may be thought to exist in each of the states represented by each of these wave-functions...[75]

Of course, the *amplitudes* differ in the two directions. But *that* difference does not concern us. The amplitudes are all to do with probabilities – how *likely* is a particular measurement outcome – whereas our question concerned the *possibility* of a process. The process presently in question is the possibility, given an objective collapse, of the diagonally polarized backward photon also being in a vertically polarized state in between the polarizers, so that it can be passed not only by the diagonal polarizer B but also by the vertical polarizer A. We have seen that such a state is indeed possible in the standard interpretation. Not only is such a state *possible*, but its reality is a *central tenet* of that interpretation.

The problem we started out with was this. If quantum mechanics is *T* invariant and if the collapse is real, quantum mechanics predicts that the photon must be in two different states simultaneously when in between the polarizers. This was taken as obviously self-contradictory – an inconsistency in the standard interpretation. But it turns out that that's just what the standard interpretation explicitly maintains. Moreover, it is also the answer to Price's charge that in the standard interpretation, the polarization is correlated only with the *past* polarizer, never with the polarizer the photon is yet to encounter. Since the photon is both definitely diagonally and definitely vertically polarized, with some amplitude, when travelling between the two polarizers (in either direction), the photon's polarization is correlated with *both* polarizers, contrary to Price's

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74 Of course, it is also in the horizontally polarized state with the same amplitude, and indeed in *all* possible states with some amplitude.

75 Healey 1998, p. 82. In this connection, recall also that multiplying (or dividing) the wave function by some number yields the *same* wave function. Hence, if we wanted to, we could even multiply both the vertical and horizontal components of the backward wave by 2 to make the amplitude of the vertical component of the backward wave identical to the amplitude of the forward vertical wave (although that would be unnecessary).

charge.[76] Consequently, the charge of objective asymmetry fails.

The supposed objective asymmetry arises from having a too classical understanding of the standard interpretation's conception of 'state'. As Wheeler put it,

> What we do not know and ordinarily cannot know is 'the value' of a dynamical variable until (1) We or, better, our apparatus *decides* which of one or another complementary variables to measure and (2) 'Nature', in the shape of an 'irreversible act of amplification', gives us the answer. Only then do we know the position of the electron or through which slit the photon came, etc. [or the spin or polarization]. But the use of the [term] 'state' to describe *that* information, while understandable among friends who make allowances for slurring of terminology, is truly dangerous in the larger world, where people have such a tendency, an understandable tendency, to *misunderstand*.[77]

Possible objections

It may be objected that the second process is not the time reverse of the first because in that process the photon is (partially) in a state of horizontal polarization, whereas in the first process it is not (even partially) in a state of horizontal polarization. The backward state is relevantly *different* from the forward one.

It is true that the backward process is not the time reverse of the forward process in the *strong* sense of reversibility because the forward and backward states differ. (Recall the two senses of reversibility, the strong and the weak, detailed in §2.2[6].) But the two states do not need to be identical for T invariance. In fact, for T invariance to obtain when two or more incompatible measurements have been made, the forward and backward states in the present example *must* differ. That is for two reasons. (1) It is only the presence of both vertical and horizontal components in the backward wave function that enables the backward photon's polarization to be correlated with *both* polarizers, as we have seen. If the polarization weren't correlated with both, quantum mechanics would be objectively asymmetric. (2) The difference is just what enables the system to be reversible in the *weak* sense of reversibility, consistent with quantum mechanics being an intrinsically probabilistic system. We have seen that no probabilistic system is reversible in the strong sense. The difference in the forward and backward states enables the backward process to be the time reverse of the forward process in the *weak* sense of reversibility. Let us look at this point in a little more detail.

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76 It does not help to say that the photon's polarization is more strongly correlated with the past polarizer than the future one. The standard interpretation does not deny the asymmetry, only that it is lawlike. The 'strength' of correlation is to do with the probability amplitudes, and is irrelevant in the present regard, save in the limit.

77 J.A. Wheeler, quoted in Bernstein 1991, pp. 95-6.

The difference between the forward and backward wave functions arises because measurement in quantum mechanics wipes out the system's memory of its previous initial conditions. In the forward picture, there is a discontinuous change in the wave function at polarizer B from vertically polarized to diagonally polarized owing to the incompatible measurement there.[78] This is often described as a *disturbance* of the system. But it is more correctly described as a *selection* by the measurement of an eigenfunction out of a linear superposition or 'menu' of eigenfunctions. In the present case the selection is of a diagonal eigenfunction. The selected eigenfunction (or state) becomes the new initial boundary condition of the system. The change of state is naturally enough reflected in the backward wave function, which starts off as a diagonal eigenfunction, with 'no memory' of its previous initial conditions, as it is often put. Crucially, however, the diagonal eigenfunction is also a linear superposition of vertical and horizontal eigenfunctions susceptible to such selection (just as the forward wave function was also a linear superposition of diagonal and slant eigenfunctions).

It is the presence of both these vertical and horizontal components to the backward wave function that ensures that the backward photon is passed by polarizer A (returns to its earlier state) only *some* of the time (with 50% probability in this case), as is to be expected of a probabilistic system, reversible only in the weak sense. To leave out the horizontal component would mean (quite apart from the problem of the jump) that the photon would be passed by polarizer A every time. But then the system would be reversible in the strong sense, like a deterministic system. But no intrinsically probabilistic system can be reversible in that sense. If T is applied to such a system, the system need not and generally does not return to its earlier state owing to the probabilistic nature of the relevant laws. It could do so only if incompatible measurements (e.g. of vertical and horizontal spin components) entailed no loss of knowledge of the system's previous state. And if that were so, it would follow that quantum mechanics is not intrinsically probabilistic after all, which is contrary to the standard interpretation. It is of course equally impossible to leave out the vertical component, since without it the system could not be passed by polarizer A at all. Perfectly analogous considerations apply in the case of the forward wave function. There is symmetry in the standard interpretation's account of the behaviour of quantum-mechanical systems in the two temporal directions.

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78 Call the vertical axis the z axis and the horizontal axis the y axis. The photon propagates along the x axis. Measurement of the photon's z axis spin at P_A gives complete knowledge of its z axis spin. By the same token, the later measurement at P_B gives complete knowledge of the photon's diagonal (call it the z' axis) spin. However, the two measurements are incompatible (simultaneous knowledge of both spins would violate the Heisenberg indeterminacy principle). Consequently, the later measurement destroys the former knowledge, as is evidenced by what happens when the motion is reversed.

I shall now mention one more possible objection to the above account. It is the claim that it is no good giving an account of the underlying state of the system in terms of possible measurement outcomes, i.e. utilizing the eigenvalue-eigenfunction link, as the standard interpretation does. If we want to establish the *T* invariance of quantum mechanics in the standard interpretation, we cannot avoid the central issue: what is *real* about the wave function? *That* is what ultimately matters, not possible measurement outcomes.

Such an objection would be misguided, given the standard interpretation. I shall now explain why reversibility (and so *T* invariance) is ultimately determined by possible measurement outcomes in the standard interpretation, and why measurement outcomes are all-important.[79] Consider the standard interpretation's claim that the backward photon (or the forward photon for that matter) is in a linear superposition of all possible states when between the two polarizers, even when the states are mutually incompatible. But it may be asked: How can we possibly know this, seeing that the wave function is unobservable even in principle? Is there perhaps some procedure that is part of the standard interpretation for determining what these possible states are? The standard interpretation says there is such a procedure. The possible states are determined by an ensemble of measurements made on identically prepared systems. If a photon is *sometimes* passed by a polarizer set at some particular angle θ, then the photon is said to be (with some appropriate amplitude) in the eigenfunction correlated with that angle. The set of eigenvalues of an observable are just the possible results of measurements of that observable. In this way, the ensemble of measurements is used to give all the different superposed states in which the photon coexists when in a 'pure state'. This is just the linear superposition of eigenfunctions. (See §1.3.2 for the meaning of 'pure' and 'mixed' states.) The quantum-mechanical operator formalism is a postulated formalism, built around the results of an ensemble of measurements.[80] As Wigner put it,

> Since, according to quantum mechanics, all information is obtained in the form of the results of measurement, the standard way to obtain the state vector is also by carrying out measurements on a system.[81] *...the laws of*

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[79] I think this is uncontroversial. When we characterize a QM system as being in some *state*, what we mean is ultimately defined in terms of results of measurements. E.g. we say that a photon is in sharp state of vertical polarization when it is passed by a vertical polarizer every time. But there is a continuum of (relative) polarizer angles giving different probabilities. So the standard interpretation says that the photon is also in these other states with the appropriate amplitude. States are differentiated by the results of measurement. So also when we *compare* states, e.g. to see if one state is the time reverse of another.

[80] As Feynman noted, the operators simply 'give the average quantities. They do not describe in detail what goes on inside an atom'. (Feynman, Leighton & Sands 1965, III-20, p. 17.)

[81] Wigner 1967, p. 164.

> *quantum mechanics only furnish probability connections between results of subsequent observations carried out on a system.*[82]

Given the standard interpretation, all that is needed for the reversibility in principle of our system (i.e. in the weak sense), and so for the T invariance of quantum mechanics is that (a) identical algorithms exist for inferring probabilities in both temporal directions, and (b) that just *one* of the above linear superposition of eigenfunctions or possible states, is correlated with polarizer A. We've seen in §2.3 that the theory does provide such identical algorithms, so there's no question about the former requirement. Therefore we concentrate on the latter. The relevant question regarding the latter is: given the backward photon's initial state (diagonal), and given its loss of knowledge of its original forward state (vertical) owing to the measurement at polarizer B, and given the standard interpretation's conception of 'state', *can* the backward photon be passed by the vertical polarizer A? Does the theory furnish a 'mechanism' whereby the photon/wave function is able to be passed by polarizer A? In other words, *given* the theory, is it *physically possible* for the system to return to its original vertical state, with everything, including the amplitude, ending up the same? If the answer is yes, the system is reversible in principle in the weak sense.

Since the wave function itself is in principle unobservable, our test is necessarily built around (i) the results of measurement, and (ii) the standard interpretation's conception of 'state'.[83]

In the above connection, it is important to bear in mind that Price's intention is to show that the standard interpretation entails, *when taken on its own terms*, a lawlike asymmetry. The standard interpretation's conception of 'state' is of course not a classical one – as evidenced by the fact that the photon is said to exist in a superposition of states even when these are incompatible. Price isn't objecting to the incompatibility of the superposed states (at least not here), or its

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82 Wigner 1967, p. 166.

83 In the Penrose case, too, our criterion of reversibility was simply (a) whether the theory provides identical algorithms for inferring probabilities in both temporal directions, and (b) whether the reverse process was physically possible. These questions were answered on the basis of observables (clicks and flags). We didn't dwell on the fact that the backward wave function differed from the forward wave function at places in the region between the photon detector and the lamp.

In Penrose's example, the forward state of the photon between the lamp L and the mirror M is a linear superposition of transmitted LP (lamp to photon detector) and reflected LA (lamp to A wall) waves. The forward state between the mirror M and photon detector P is a transmitted LP wave.

In the backward direction, in the region P to M, the general state predicted by QM is a linear superposition of transmitted PL (photon detector to lamp) and reflected PB (photon detector to B wall) waves. However, in his example, Penrose has in effect thrown away all the PB instances by selecting an LP photon. So the actual state in the region P to M is simply a PL wave. And in the region M to L the state is a transmitted PL wave. The backward state differs from the forward one at least in the region M to L.

metaphysical inadequacy. His claim is simply that the standard interpretation, taken on its own terms, seems to have no means of enabling a photon whose state is described by a *collapsed* wave function (where the collapse is not an artefact of the theory) to be correlated with *both* polarizers, as would be necessary to avoid a lawlike asymmetry. But, as we've seen, the claim fails. If so, Price's attempt to find a symmetry argument in favour of advanced action has failed.[84]

We now briefly look at possibilities (2), (3) and (4).

2.5.2.2 Does the collapse entail an objective asymmetry? Part II

(2) The claim is that although it is true that there is an asymmetry in the forward and backward cases (the process in either direction being irreversible), the asymmetry is simply due to *loss of information*, just as (a) in thermodynamics (Maxwell's demon), or (b) in computing – and therefore factlike, not lawlike. That is, the loss of knowledge in quantum mechanics arising from measurement is no different in kind to the loss of knowledge of exact motions in thermodynamics (such loss of knowledge in the latter case permitting a factlike asymmetry in the two temporal directions in the case of non-equilibrium systems).

Take (a), the analogy with thermodynamics. There seem to be at least two important differences in the two cases. The first is that thermodynamics is a *statistical* theory. It cannot be applied to individual particles, whereas, even though quantum mechanics is also a statistical theory, the above interpretation *is* supposed to be applicable to individual particles. The second is that in thermodynamics, knowledge of the underlying motions (the necessary boundary condition for reversibility) at any one instant is supposed to be possible, even if only in principle (Laplace's demon). Not so in the standard interpretation of quantum mechanics. For that reason, the latter irreversibility seems more fundamental – less to do with an *unavailability* of information than the *non-existence* of information. So this particular defence of the symmetry of quantum mechanics does not seem to work very well.[85]

Now consider (b), the computing analogy. R. Landauer and C. Bennett have shown that the irreversibility inherent in computing is due to discarding information.[86] Likewise, obtaining new knowledge in quantum mechanics seems

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84 See Price 1996, pp. 181-2. It would appear that in the standard interpretation we make asymmetric judgments about the photon's state for much the same kind of reason as we do in the case of a pair of colliding frictionless Newtonian billiard balls – namely the conventional asymmetry in the way we assess counterfactuals in general. However, that does not reflect any intrinsic asymmetry in the underlying theoretical picture.

85 Nick Herbert (1985, p. 191) seems to make much the same point.

86 See Bennett 1987, p. 96. See also Bennett & Landauer 1985, pp. 38-46. Bennett and Landauer also showed why Maxwell's demon could not violate the second law. It wasn't because finding out the locations of the molecules of gas was thermodynamically

equivalent to discarding old information. The argument, then, is that quantum mechanics is irreversible in the same way that computing is. Now, it is true that the new state selected by a measurement (say a diagonal eigenfunction as in our above example) becomes the new initial boundary condition of the system. The change of state is naturally enough reflected in the backward wave function, which starts off as a diagonal eigenfunction. It is supposed to have no memory of its previous initial state (vertical eigenfunction), which is presumably equivalent to the information loss in computing. However, we have seen that the diagonal eigenfunction is also a linear superposition of vertical and horizontal eigenfunctions, the effect of which is to give the backward wave function a 'memory' (in the statistical sense) of its previous state just to an extent determined by its amplitude for that state. Unless there is a corresponding feature to the information loss in computing, the analogy seems to fail.

(3) I have argued that quantum mechanics is objectively symmetric in the standard interpretation. But let us suppose for the sake of argument that Price is right after all in his claim that quantum mechanics *is* objectively asymmetric in the standard interpretation. It can still be argued that the asymmetry is peculiar only to the standard interpretation, not arising in Bohr's original Copenhagen interpretation.

We have seen that in the standard interpretation, the concept of quantum *state* plays a key role, the wave function yielding a description of the objective properties of an individual system even when they are not observed, albeit in an incompletely defined form. Such an approach differs sharply from that of Bohr, who maintained that the system exists together with the observing apparatus as a single indivisible system not susceptible of further analysis. In particular, we may not speak of the state of the system independently of the observation to be made on it. Thus we may not say that the state of the forward photon in between the polarizers, when it is unobserved, is definitely vertically polarized, and that of the backward photon diagonally polarized. The 'state' is a *relational* property between the photon and the measuring apparatus, and depends on the actual measurement being performed – thus referring to the entire experimental situation. It follows that in between measurements, the photon has *no* intrinsic state. Consequently, the claimed objective asymmetry does not arise. (Heisenberg, too, doubted the reality of the past unobserved history of a particle, even though, as he noted in 1930, we are able to calculate backwards to its momentum and position in the past [indeterminacy relations notwithstanding] with arbitrary degree of accuracy. 'It is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or

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costly, as had been thought. First, in 1961 Landauer showed that it was getting *rid* of this information that was costly – but necessary; then in 1982 Bennett showed that the cost of so doing saved the second law.

not.'[87])

(4) Let us suppose once more for the sake of argument that Price is right in his claim that quantum mechanics is objectively asymmetric in the standard interpretation, at least on the basis of what has been said so far. We ask: is it asymmetric because, as Price claims, the interpretation mistakenly assumes that there is an objective collapse of the wave function? Or is it asymmetric because we have mistakenly read too much into it, and the asymmetry is an *artefact of our interpretation of the standard interpretation*? Maybe quantum mechanics looks asymmetric in the standard interpretation only because we have erroneously taken the interpretation to entail that it is actually possible to *locate* the collapse (locate the von Neumann cut). For example, we have assumed for simplicity throughout the preceding discussion that the collapse *is sharply located,* occurring at polarizers A and B. But perhaps that is an erroneous idea, responsible for the apparently lawlike irreversibility – which may not be lawlike at all. Given that it is always possible to push back the collapse as far as one likes – to the observer's consciousness and beyond, as we've seen in the case of Schrödinger's cat and Wigner's friend – and having regard to the fact that the further back it is pushed, the greater the accuracy and completeness obtained,[88] it seems unavoidable that, strictly speaking, only the *entire system* (of photon, apparatus and observer) has a pure wave function.[89] The individual elements will not be represented by a pure wave function even after measurement. The entire system is assumed to have some pure wave function when the experiment starts, and after the interaction (the measurement) is over it will go into some other pure wave function.

Once again, in both the von Neumann/Dirac and the original Bohr versions of the standard interpretation, the state described by the pure wave function is defined by an ensemble of measurements made on identically prepared systems. This time though, because our wave function is not just that of the photon alone, but of the larger system, we must take into account not only those outcomes in which the forward photon is passed by the second polarizer, but also the outcomes in which it is *blocked* by it, i.e. the half of measurement outcomes we have hitherto 'thrown away' by confining our treatment to the cases in which the photon was passed by the second polarizer. To see why, compare the present case to that of Schrödinger's cat, in which the individual system of live cat or dead cat is always more correctly described as being in a superposed state (a pure state) in some larger system even if it is collapsed in the frame of some observer. The principle is the same in the present case too.

Consider once more the wave function of the vertically polarized photon as

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87 Heisenberg 1930, p. 20.

88 And also the uselessness, for most purposes, of the result. (See Bell 1987, p. 188.)

89 See Sharp 1961. For discussion, see Putnam 1961, and Hooker 1971.

it travels (forward in time) from polarizer A to the diagonally rotated polarizer B. This vertically polarized state is also correctly described as a linear superposition of slant and diagonal eigenfunctions. The diagonal eigenfunction passes through the polarizer while the slant polarized eigenfunction is blocked. Born's statistical interpretation of the wave function then tells us that there is an equal probability of the photon being passed and of being blocked by the polarizer. If the photon is passed, its (collapsed) state is then a diagonally polarized eigenfunction of its previous state. If it is blocked, there is no photon beyond the second polarizer.

Now consider the above account in time reverse. This time, in our time-reversed picture we must include both the passed and blocked waves. We start off with a diagonally polarized wave approaching polarizer B from distant infinity. It passes through B and then continues on its way toward polarizer A – still diagonally polarized. At the instant this wave passes through polarizer B, polarizer B emits a *second* wave, that wave being the one absorbed by it in the forward-time direction. This wave is polarized perpendicularly to the diagonally polarized one (it is slant polarized). This wave also travels toward polarizer A. The two waves together constitute a wave that represents a linearly superposed state of diagonal and slant polarization, which, as we have seen, can also be correctly described as a vertically polarized state.[90] But that was just how we described the state of the photon in between the polarizers in the *forward*-time picture.

Once again, the point of the above account is that it (a) eliminates the claimed non-correlation of the backward-in-time wave with the polarizer it has passed through, while still remaining faithful to the standard interpretation, and (b) shows that the backward process is certainly a possible one according to standard quantum mechanics. The behaviour of the backward wave functions is a solution of the Schrödinger equation, and Born's statistical interpretation of the waves in question gives the correct probabilities. It can readily be applied to any relative orientation of the two polarizers, and it can be done starting from either end. It shows, in other words, that the argument for a *lawlike* or objective asymmetry in the standard picture fails.

Such an ensemble interpretation of the wave function does not preclude an objective collapse (though it does nothing to remove the attendant interpretative problems of the collapse). Even though von Neumann proved that one could locate the collapse anywhere between the source and the observer without changing the results of an experiment,[91] he did not deny the objectivity of the collapse. And the collapse is certainly taken as real in both Bohr's interpretation

90 Taken individually, each one of these two waves can be further analysed into a superposition of two waves – in each case of vertical and horizontal polarization.

91 Herbert 1985, p. 152; Stapp 1993, p. 139.

and in the standard interpretation – even though strictly speaking only the entire system has a wave function. The reality of the collapse is unaffected by the fact that it can't be uniquely located. It is the *reality* of the collapse that is Price's main target, with the supposed time-asymmetry of quantum mechanics being one of the problematic consequences of it. Price denies a collapse *tout court*, save in the knowledge sense. He doesn't dwell on the fact that it cannot be sharply located. He takes it as given, for the purposes of his present criticism, that the collapse *is* sharply located in the standard interpretation. Nor does he distinguish between the Dirac/von Neumann and Bohr versions. For these reasons, Price's criticism[92] of Aharonov, Bergmann & Lebowitz's 1964 assertion that the time asymmetry of the measurement process is not problematic owing to the fact that the wave function describes an ensemble (and is related to the manner in which statistical ensembles are constructed) seems to miss the point.

Summary

In this chapter we have examined the role of time in quantum mechanics. We have compared how time and time reversal enter into classical and quantum physics. Important issues arose with a bearing on our later critical assessment of Price's proposal. The issues concern the question of whether the laws of quantum mechanics are time reversal invariant. The laws in question are Schrödinger's equation and the reduction/projection postulate (collapse of the wave function). One of Price's main arguments for the advanced action interpretation of quantum mechanics is that the standard interpretation unnecessarily introduces an objective asymmetry into quantum mechanics by way of the wave function collapse. (Symmetric theories are to be preferred to asymmetric ones, other things being equal.) In the advanced action interpretation, on the other hand, there is no objective collapse as the wave function is not objectively real. It has the same ontological status as the classical probability function. An objective collapse is an artefact of the standard interpretation. Clearly, if the collapse is not objective, the asymmetry generated by it can hardly be objective either. Thus, going to an atemporal picture results in the restoration of symmetry (subject to the Schrödinger evolution itself being symmetric).

I investigate the details of the collapse in the standard interpretation and find that, contrary to Price (and Penrose too), it does not render quantum mechanics objectively asymmetric in that interpretation. This is the second of the four main claims argued by this thesis. I also briefly consider an argument for the non-time reversal invariance of the Schrödinger equation itself, advanced by Callender, and spell out its ontological implications.

We now turn to the Einstein-Bohr debate on the nature of quantum reality.

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92 Price 1996, p. 208.

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The Einstein-Bohr Debate on the Nature of Quantum Reality; Umpire Bell Steps In

This theory reminds me a little of the system of delusions of an exceedingly intelligent paranoic, concocted of incoherent elements of thoughts.

(Letter from Einstein to D. Lipkin, July 5, 1952)

Einstein's biographer, Abraham Pais relates that during one of the lunchtime walks that Einstein and he used to take in around 1950, Einstein suddenly stopped, turned and asked him if he really thought that the moon existed only when he looked at it.[1]

Einstein's remark needs to be understood in the context of the debate between him and Niels Bohr regarding the nature of physical reality, and in particular, the nature of an unmeasured quantum object. More technically, the issue between Einstein and Bohr comes down to an interpretation of the quantum-mechanical conception of 'state' (of a system), represented by Ψ.

In Chapter 1 we saw that the issue between the Einsteinian realists and the proponents of Bohr's Copenhagen interpretation is whether the quantum-mechanical description of physical reality is *complete* – and in particular, whether the quantum wave function (or state vector) Ψ contains a complete physical description of the state of the system in question. We briefly looked at an argument against the Copenhagen view put forward by Einstein (§1.3.2). Einstein's position amounted, in effect, to the postulation of some kind of unspecified local 'hidden variables' or 'inner properties' of quantum objects that would restore completeness and objectivity to quantum theory.

Historically, there have been three main motivations for constructing hidden variable theories.[2] One is a reluctance to accept the radical conceptual innova-

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1 Pais 1994, p. 36.

2 Isham 1989, p. 382.

tions of quantum theory. Another is the desire to avoid the measurement problem of the standard interpretation (see §1.3.1). A third, and the one of most interest to us in the present chapter, is a very famous thought-experiment put forward in 1935 by Einstein and two collaborators, known as the EPR argument, mentioned in §1.3.3. This chapter is structured around that argument, and its subsequent experimental realization. It is the perfect platform for displaying, in the proper historical context, the workings and subtleties of the main elements of the two broad contending positions described in §1.1. The EPR argument also constitutes the natural point of departure for all non-standard interpretations, some of which we look at in §3.5. There is simply no interpretation that can afford to just ignore it.

By 1935 Einstein thought he had a definitive answer to the question of whether the quantum-mechanical description of physical reality is complete. His argument was contained in a paper known as the EPR paper, after its joint authors, Einstein, Podolsky and Rosen. It was entitled 'Can quantum-mechanical description of physical reality be considered complete?'[3] The aim of the paper was to show that the standard/Copenhagen interpretation of quantum mechanics was faced with a dilemma: it had either to (a) violate the principle of local causality (separability and the absence of faster-than-light signalling [§3.1]) or (b) be incomplete. Since hardly any physicist seriously believed in faster-than-light signalling, the EPR conclusion was that quantum mechanics was incomplete. There were conceptual and technical difficulties in the original EPR thought experiment which won't concern us here,[4] and in 1951 David Bohm came up with a spin version of the experiment that is simpler both conceptually and technically, on which more later.[5] Stripped to the essentials, though, the original ERP argument may be stated as follows.

3.1 The EPR argument

We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete.

(Einstein, Podolsky, Rosen, 1935)

The argument starts from the twin Copenhagen claims that (a) until a measurement is performed, a particle does not possess a definite momentum and posi-

[3] Einstein, Podolsky & Rosen 1935.

[4] For example, the original argument was in terms of momentum and position. But eigenfunctions of momentum extend over all space, and for that reason it is difficult to maintain locality for two such functions. Furthermore, there exist several later versions of the EPR argument, written by Einstein himself. For a comprehensive analysis of the EPR paper and reactions to it, see Jammer 1974, pp. 166-247. See also Jammer 1985, pp. 129-49; Rosen 1985, pp. 17-33; Fine 1986, pp. 26-39, 59-63; Bub 1997, pp. 40-5. For a relativistic treatment of the argument, see Smith & Weingard 1986.

[5] See Bohm 1951, pp. 614-22; Bohm & Aharonov, 1957.

tion, an appropriate act of measurement being required to bring such properties into existence, and (b) it is impossible even in principle to measure both the momentum and position of a particle at the same time.[6]

The purpose of the argument is to force the Copenhagen theorist into making the additional claim (regarded as obviously false by EPR) that the properties of a particle in space-time region **B** are determined by an act of measurement of the properties of a particle in space-time region **A**, even when the two regions are so far apart that there is no possibility of any interaction between the regions by any known dynamical mechanism. The argument depends on certain assumptions which EPR made, some explicit and some implicit, which will be analysed following the description of the EPR argument itself, given below.

EPR considered a pair of particles (particle 1 and particle 2) in a state with definite total momentum $p_x = p_{1x} + p_{2x}$ and definite relative distance $x = x_1 - x_2$, both of which are known. (Note that even though Heisenberg's indeterminacy principle prevents us from measuring both the momentum and position of either particle simultaneously, there is nothing to prevent us from measuring the *sum* of their momenta ($p_x = p_{1x} + p_{2x}$), and the *distance* between them ($x = x_1 - x_2$) without any indeterminacy, since these quantities commute.[7]) Suppose for instance that a single particle at rest decays into a pair of particles 1 and 2, possessing equal momenta. Because of conservation of momentum, the two particles fly away from each other in opposite directions at high speed. Conservation of momentum implies classically that at any moment their momenta, velocities, and therefore their positions are related. If we measure the momentum of one particle, we immediately know the momentum of the other. Likewise, if we measure the distance one particle has travelled, we immediately know the distance the other has travelled. Suppose now that the momentum p_{1x} of particle 1 is measured some appreciable time after the interaction. The momentum p_{2x} of particle 2 can then be calculated without measuring it from the known total momentum of the two particles. The second particle is too distant from the first particle to be affected in any way by the measurement made on the latter, given the absence of faster than light influences. (The total momentum of the system is conserved since no torques have acted on the particles.)

It is assumed, owing to the distance between the particles, that they are no longer interacting. In particular, particle 2 is not disturbed or affected in any way by our measurement of particle 1 (or of course by our calculation of particle 2's momentum). This is EPR's famous *locality* assumption. EPR concluded that particle 2 must have possessed the property of a definite momentum all along, even before the momentum measurement made on particle 1.

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6 Of two physical quantities represented by noncommuting operators, the precise knowledge of one of them precludes such knowledge of the other.

7 See Bohr 1949, p. 233; also e.g. Pais 1991, p. 429.

Furthermore, the decision could have been taken to measure position instead of momentum. In that case, the single measurement of the position x_1 of one particle would immediately yield the position x_2 of the second particle without a second measurement being necessary. Hence, by the same argument as above, the second particle must have possessed the property of definite (albeit changing) position all along even before the measurement. (This conclusion, which seems self-evident, is supported by EPR's criterion of physical reality – the core assumption of their paper.)

But since it is completely arbitrary which of the two properties of the first particle (momentum or position) we decide to measure (about which decision the second particle can know nothing), the only conclusion that can be drawn is that the second particle must have possessed *both definite momentum and position* before any measurement was made. And finally, since it is arbitrary which of the two particles is selected to be the measured particle 1, *both* particles must have possessed both definite momentum and position before measurement. Since this is impossible according to quantum mechanics, EPR concluded that the wave function of quantum mechanics does not provide a complete description of the physical reality. (EPR left open the question of whether or not such a description exists, saying, however, that they believed that such a theory was possible.)

It is evident that Einstein's point of departure in EPR is 'realistic' rather than 'deterministic'.[8] EPR did not assume determinism, but *inferred* it, taking not determinism but the principle of *local causality* as the overriding physical principle, as e.g. Bell points out.[9] Einstein himself described the main point of EPR to Max Born as follows:

> That which really exists in **B** should... not depend on what kind of measurement is carried out in part of space **A**; it should also be independent of whether or not any measurement at all is carried out in space **A**. If one adheres to this program, one can hardly consider the quantum-theoretical description as a complete representation of the physically real. If one tries to do so in spite of this, one has to assume that the physically real in **B** suffers a sudden change as a result of a measurement in **A**. My instinct in physics bristles at this.[10]

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8 This was pointed out by Wolfgang Pauli in a letter to Born from Princeton in 1954. Cited in Mermin 1985. For discussion, see Fine 1986, pp. 101ff.

9 Bell 1987, p. 143.

10 Cited in Mermin 1985, p. 38. Notice that Einstein's requirement of 'local causality', in the form expressed here, rules out not only faster-than-light signalling but also backward causation. More particularly, it presupposes that the causes of an event cannot lie in both its past and future light cones – a possibility that allows seemingly faster-than-light causal connections to be propagated within light cones. We shall investigate such a possibility as an explanation of the EPR/Bell correlation in the following chapters. Einstein's 'local causality' may be succinctly stated as the principle that all the causes of an event lie in its past light cone.

It is important to realize that EPR did not find any *error* in the predictions of quantum mechanics. Rather, they used one of the predictions of quantum mechanics itself (entanglement of wave functions) to try to show that as a theory of reality it was incomplete.

It is also important to appreciate that for the purposes of EPR's original paper there is no need to perform a second measurement (of the non-commuting property) following our initial measurement. It might erroneously be thought, once we have measured the momentum of particle 1, and calculated the momentum of particle 2, that it is necessary to go and *actually measure* the position of particle 2, thereby obtaining in a more direct way than above definite magnitudes for both the momentum and position of particle 2. (Alternatively, following our measurement of the momentum of particle 1 and calculation of the momentum of particle 2, we could measure the position of particle 1, and calculate the position of particle 2 from that measurement.) However, given both locality and EPR's criterion of physical reality (see below), the latter accepted by both Einstein and Bohr, such measurement would be redundant. *Given* EPR's assumptions, the above thought experiment already conclusively demonstrates the existence of simultaneous definite momentum and position before any measurement is made. Only if we *deny* any of EPR's assumptions will there be need of a second measurement.

I mentioned above EPR's *locality* and *physical reality* assumptions. Regarding the former, it is necessary to isolate the elements of the assumption since Bohr's objection to the EPR argument crucially depends on denying one of those elements. As for the latter, since the argument between EPR and the Copenhagen theorists concerns the nature of physical reality, some mutually agreeable criterion of physical reality also needs to exist – else the argument won't even get off the ground. Likewise, since the point of the EPR paper is to try to show that quantum mechanics is incomplete, a criterion of *completeness* of physical theory also needs to be agreed upon. And finally, the assumption that the statistical predictions of quantum mechanics are *correct* might as well be made explicit, seeing that such correctness is assumed by both sides. Here, then, are the main premises of the EPR argument:[11]

EPR's assumptions:

The physical reality criterion

'If, without in any way disturbing a system, we can predict with certainty

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11 Passages within quotation marks in the indented paragraphs are from the EPR paper. The commentary within square brackets is provided by me. See also Bohm's discussion of the EPR criteria, in Bohm 1951, pp. 611-12; see also Jammer 1974, p. 185.

(i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity... Regarded not as a necessary, but merely as a sufficient, condition of reality, this criterion is in agreement with classical as well as quantum-mechanical ideas of reality.'[12]

[This criterion constitutes the *philosophical core* of Einstein's picture of physical reality, and is the basis of the EPR paper – identified as such by Bohr in his reply to it.[13] At the end of their paper, EPR contrasted this criterion with a more restrictive one, in which 'two or more physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted*'. On this latter point of view, according to EPR, one would not arrive at their conclusion. That's because only one or the other of the two quantities – momentum or position – can be simultaneously predicted,[14] and so the two could not be simultaneously real. That would make their reality 'depend on the process of measurement carried out in the first system, which does not disturb the second system in any way'. In the opinion of EPR, '[n]o reasonable definition of reality could be expected to permit this.' It is ironic that Bohr's reply to EPR permitted just such 'unreasonableness' (as we shall see), even though Bohr accepted EPR's own less restrictive criterion of physical reality.]

The locality assumption

If 'at the time of measurement... two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system. This is, of course, merely a statement of what is meant by the absence of an interaction between the two systems.'

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12 EPR's condition of reality is a *sufficient* but not a necessary one because there may be other elements of reality which one cannot predict with certainty, but which exist nevertheless, e.g. a definite position of a particle even when its momentum has been measured or predicted. To argue this is the whole point of the EPR paper.

13 Bohr 1935a and 1935b.

14 What EPR had in mind here is the fact that measurements of the momentum and position of the first system can be carried out only one at a time. If we first measure its momentum, for example, then we must use a different experimental set-up to measure its position. It's impossible to measure both properties simultaneously if measurements are to be confined to the first system. EPR's hypothetical more restrictive criterion of physical reality is based on this fact. It seems evident that at the time EPR did not contemplate the possibility of simultaneous measurements on both systems, e.g. measuring the momentum of the first particle and the position of the second particle (or vice versa), although Einstein later did so. *Given* EPR's assumptions, such measurements (which could easily be carried out simultaneously) would enable the prediction of the simultaneous momentum and position of the second particle (and conversely, of the first particle). According to quantum theory such prediction is impossible even though the two measurements can be carried out simultaneously.

[The assumption of interest is that the two systems no longer interact. This is the crucial assumption that enables EPR to infer that particle 2 must have possessed both definite momentum and position even before measurement. (Even though EPR's criterion of physical reality, their other principal assumption, constitutes the basis of the EPR paper, it [the criterion of reality] is uncontentious, in the sense that Bohr himself subscribed to it). The locality assumption may be broken down into three related assumptions.[15]: (a) *separability*: the world can be correctly analysed in terms of distinct and separately existing elements of reality[16], i.e. the complete physical state of the world is specified once one has specified the intrinsic state of each small spatio-temporal region of the world.[17] It was this separability assumption which formed the basis of Bohr's attack on EPR, rather than their reality criterion; (b) *no action-at-a-distance* between spatially separated systems: nothing that happens to a system can instantaneously affect another system (the speed of light is the upper limit for the transmission of influences between systems); and (c) *counterfactual definiteness*: hypothetical measurements would have led to definite outcomes. In EPR locality is 'derived' in terms of hypothetical results: *If* a certain measurement, say q, had been performed (which could have been performed but wasn't), then there is a certain value of q that *would have been obtained*. Not just any value would do. There is a *particular* value that would have come up.[18] Unless counterfactual definiteness of *some* kind is presupposed, the notion of locality cannot even be formulated. For example, it would make no sense to talk of the speed of light as the upper limit for the transmission of influences between separated systems, because the notion of a *disturbance* of the existing system by an incoming influence would make no sense, both 'system' and 'disturbance' being ill-defined.[19] Yet counter-factual definiteness is by its very na-

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[15] Different interpretations of quantum mechanics violate different ones of these conditions. See Healey 1998, pp. 87-103.

[16] As e.g. Bohm points out (1951, p. 612).

[17] This of course presupposes that such spatio-temporal regions *have* intrinsic physical states, as e.g. Maudlin points out (1994, p. 97).

[18] Statements of the above form are known as contrary-to-fact subjunctive conditionals (counterfactuals). There are also subjunctive conditionals which are not counterfactuals, e.g. 'If it were to be the case that A, then it would be the case that B.' (More on counterfactual analysis of the EPR thought-experiment later in this chapter.)

[19] In this connection, some remarks of Bell and Bohm are relevant. Bell (1987, p. ix) writes, 'If local causality in some theory is to be examined, then one must decide which of the many mathematical entities that appear are supposed to be real, and really here rather than there.' As for Bohm (1951, pp. 167-8), he writes that 'the classical concepts of continuity, causality and the analysis of the world into distinct parts are all necessary for each other's consistency; foregoing any one of them leads to the necessity for giving up all'. It is also 'easily seen' (he writes) 'that the concept of precisely defined causal laws has meaning only in a context in which the world can be analysed into

ture untestable in any direct way, since each event happens only once. The choice to look at one aspect of physical reality necessarily entails not looking at some other aspect of it.[20] That's not to say that it cannot be tested indirectly, though – witness Bell's theorem, and scientific theories generally.[21] Different responses to EPR in fact involve adopting different versions of counterfactual definiteness, as we shall see.[22] For completeness a fourth assumption ought to be mentioned, related to the preceding three, namely the *absence of backward causation*.]

The completeness condition

A physical theory is not complete unless it meets the following requirement: 'every element of the physical reality must have a counterpart in the physical theory.'

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distinct elements moving continuously. For without such elements there will be no precisely definable variables to which the causal laws can be applied.'

20 As Herbert 1985, pp. 188, 236 points out.

21 Bell writes, 'In this matter of causality, it is a great inconvenience that the real world is given to us once only. We cannot know what would have happened if something had been different. We cannot repeat an experiment changing just one variable; the hands of the clock will have moved, and the moons of Jupiter. Physical theories are more amenable in this respect. We can *calculate* the consequences of changing free elements in a theory, be they only initial conditions, and so can explore the causal structure of the theory.' (Bell 1987, p. 101.)

22 John Cramer (1986, p. 648n) points out that there have been several attempts in the literature to answer the question of what the minimum assumption is about the physical world that one must relinquish in order to retain the locality assumption in the face of the Bell inequality experimental result. Bernard d'Espagnat has suggested (1979, p. 128) that the minimum such assumption is the existence of an objective external reality that is independent of the knowledge of observers. He believes that even in a world where Einstein separability is violated, it may be that the 'concept of an independently existing reality can retain some meaning, but it will be an altered meaning and one remote from everyday experience' (p. 140). Clauser & Shimony (1978, p. 1881) have suggested the weaker assumption of 'realism', i.e. that an external reality exists and has definite objective properties whether we measure them or not. Stapp has put forward *contrafactual definiteness* (CFD) as a minimum assumption. It means that for the various alternative possible measurements (perhaps of noncommuting variables) which might have been performed on a quantum mechanical system, each would have produced a definite (but unknown and possibly random) observational result and further, that this set of results is an appropriate matter for discussion. According to Cramer himself (for whom CFD is the minimum assumption of choice), CFD is a rather weak assumption and is often used by practising physicists in investigating and discussing quantum mechanical systems. It is completely compatible with the mathematics of quantum mechanics but is in some conflict with the positivistic element of the Copenhagen interpretation and with certain other interpretations. Cramer himself advocates retaining CFD while abandoning locality. His own 'transactional interpretation' of quantum mechanics (§4.7.1) doesn't require any revision of the mathematical formalism of quantum mechanics, but only a revision of the interpretation of the formalism. It is 'explicitly nonlocal but is also relativistically invariant and fully causal' (Cramer 1986, pp. 648-9).

[If, for example, a particle possesses both definite momentum and position as simultaneous elements of physical reality, as EPR thought they had shown, then quantum mechanics clearly fails to satisfy this criterion, since the wave function of the entangled system can specify, at most, only one of these components with complete precision.]

The validity assumption

The statistical predictions of quantum mechanics – at least to the extent they are relevant to the argument itself – are confirmed by experience.

[Such correctness is tacitly presupposed in the EPR argument, and of course taken for granted by the Copenhagen theorists. This is just where the whole interest of the debate comes from: *Given* the correctness of the results, what are the moves open to the disputants? Quantum theory may be a correct theory of microscopic *phenomena*, but is it a complete theory of microscopic *reality*?]

EPR concluded that 'the quantum-mechanical description of physical reality given by wave functions is not complete'.

3.2 Bohr's reply to EPR

...we now see that the ... criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression 'without in any way disturbing a system'.

(Niels Bohr 1935)

Bohr managed to answer the EPR onslaught in a way that convinced most physicists, though his answer never convinced EPR. His reply centred on the holistic nature of quantum mechanics. The important thing in quantum mechanics, according to Bohr, is *the entire experimental situation*. It is a mistake to think that the two-particle system envisaged by EPR exists in its own right with clearly defined, intrinsic classical properties such as momentum and position, independent of the particular experimental situation. In particular, the system cannot be divided up into distinct parts that could be 'disturbed' by the initial measurement. Instead, it exists together with the observing apparatus as a *single indivisible system not susceptible of further analysis*.[23] Quantum mechanics only predicts experimental results made within a definite experimental context. Such context includes the nature of the measuring instruments. To perform the two different kinds of measurements involved in the thought experiment (measurement of the first particle's momentum or of its position), we would need to use different instruments in the laboratory. That being the case, the experimental conditions

23 '... will defy any closer analysis'; see Bohr 1935b, p. 701.

would be different in the two cases, and therefore the *total system* would be different (i.e. we would have a *different* 'single indivisible system not susceptible of further analysis') – which is why different 'results' are obtained regarding the state of the second particle when the two different kinds of measurements are actually made on the first particle.

Since there is never any *disturbance* of the original system, there is never any question of the existence of faster-than-light influences or action-at-a-distance. For similar reasons, it would be wrong to say that a measurement of (say) the first particle's momentum at location **A** *confers* reality on the momentum of the second particle at location **B**, nor that a subsequent measurement of the first particle's position at location **A** confers reality on the position of the second particle at location **B** – while at the same time somehow *removing* the reality of its already conferred momentum. A particle's properties are simply *relations* between the particle and the measuring apparatus, and are not intrinsic to either.[24] That is why the second particle has *neither* momentum nor position to be conferred (or unconferred). That is also why one can seemingly 'influence' the attributes by using a different experimental set-up. For Bohr, there is simply no meaning to such talk outside the unanalysable entire experimental situation in which the particle is actually observed.

Heisenberg later suggested an intermediate view, in which a particle has only a *potential* to show these complementary properties when placed in an appropriate experimental situation.[25] But according to Bohm & Hiley, Bohr specifically rejected even this suggestion, evidently because he felt that it gave too much independent reality to whatever is supposed to be represented by the wave function.[26] Leon Rosenfeld, though, a close collaborator of Bohr and the major apologist of the Copenhagen interpretation after Bohr's death, said that Bohr was never acquainted with Heisenberg's idea of 'potentiality'.[27]

Subtle stuff. It seems that Einstein had great difficulty in reaching a sharp understanding of what Bohr had in mind. J.S. Bell, too, wrote that he had little

24 For discussion, see e.g. Herbert 1985, pp. 160-2.

25 It would seem that this conception was first proposed by Bohm. See Bohm 1951, pp. 132-3, 138-9, 620, *passim*. See also Bohm & Hiley 1993, p. 18n.

26 Bohm & Hiley 1993, pp. 18-19.

27 Rosenfeld said, 'When you use such a vague word as *potentiality*, you can give it whatever meaning you like. The wave function or the state vector, whatever you call it, may be said to contain an infinity of potential answers to the question. Once you have made a measurement, let us say of position, then you get the wave function which is localized, which is a wave packet containing many values of the momentum, if you analyse it. Here one can use the word *potentiality*. Bohr was never acquainted with this idea of Heisenberg, but I can guess the way he would have taken it. He would have said: "Well, that's a word, 'potentiality'! If it is useful, all right, let us use it." But I, personally, don't see this particular use.' (Rosenfeld 1979, p. 24.)

understanding of Bohr's position.[28]

Let us take a closer look at it. At first sight, Bohr's acceptance of the fact that the measurement of the first particle's momentum would also (indirectly) yield the second particle's momentum seems to commit him, as was the intention of EPR, to accepting that the second particle is in some sense instantaneously 'disturbed' or 'influenced' if a subsequent measurement is made of the first particle's position (an incompatible property). As Stapp, for example, points out, if a second measurement – of an incompatible property – is performed on particle 1 at **A**, then the situation in the second region **B** becomes, according to the quantum-mechanical formalism, identical to what would obtain at **B** if the corresponding measurement of an incompatible property had been carried out on particle 2 there (and vice versa).[29] If particle 2 actually possessed the real property inferable from the first measurement, and subsequently went on to possess, not that property but a *different* real property inferable from the second measurement, and if neither real property was present before the measurements, then Bohr 'cannot evade the conclusion that the far-away system is *disturbed* by the action of the first device'. If so, the quantum-theoretical account of the nature of microphysical reality does not satisfy EPR's locality assumption.[30]

Bohr would demur. His reply to EPR fastened onto what Bohr himself described as an *essential ambiguity in the EPR criterion of physical reality*. ('If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.') The ambiguity concerned the meaning of the expression, 'without in any way disturbing the system'.[31] According to Bohr, there is never any question of dividing up the system into distinct 'parts' that can be disturbed. Hence it makes no sense for EPR to talk of *disturbing* (or not disturbing) the second particle by our measurement of the first particle, and for that reason to talk of instantaneous nonlocal influences would be inappropriate. If we *were* to speak of a 'disturbance', then we would need to say that it is the *entire*

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28 See Bell 1987, pp. 155-6, 189-90.

29 Stapp 1991, p. 8.

30 This objection is especially pertinent against today's standard interpretation of quantum mechanics (the modern version of the Copenhagen interpretation – as contrasted with Bohr's original formulation). According to today's standard interpretation, the wave function gives the real properties of an individual system even when unobserved. That is, the classical properties of particles *actually exist* even when unmeasured, albeit in an incompletely defined form. For example, 'when the wave function of a system equals the sum of some set of wave functions, the system may be thought to exist in each of the states represented by each of these wave functions, participating in some sense in all their properties, even when these are incompatible'. See Healey 1998, p. 82.

31 See Bohr 1949, pp. 233-4.

system that is disturbed.[32] But in that case it is more appropriate simply to talk of performing a *different experiment* – a new experimental situation then exists and for that reason the question of the existence of special relativity-violating nonlocal influences again does not arise. Thus Bohr is able to both retain special relativity and accept EPR's criterion of physical reality, and yet maintain at the same time that quantum mechanics is complete. In Bohr's view, although it's true that an element of physical reality exists whenever one can be predicted with certainty, it turns out that it can be predicted under fewer circumstances than is assumed by EPR and classical physics. The quantum-mechanical formalism codifies the circumstance in which such prediction is possible.

The nub of Bohr's argument is contained in the following passage in his paper:[33]

> From our point of view we now see that the wording of the above-mentioned criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression 'without in any way disturbing the system'. Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence of the very conditions [i.e. experimental conditions] which define the possible types of predictions regarding the future behaviour of the system.* [Bohr's italics.] Since these conditions constitute an inherent element of the description of any phenomenon to which the term 'physical reality' can be properly attached, we see that the argumentation of the mentioned authors does not justify their conclusion that quantum-mechanical description is essentially incomplete.[34]

He continues rather more obscurely,

> On the contrary, this description... may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory.[35]

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[32] In view of this, rather than talking of an 'ambiguity' in the EPR criterion of physical reality, as Bohr does, it may be better simply to talk of a different stance as to what counts as a system.

[33] It is advisable to read the preceding several paragraphs of the paper to place the passage in context, bearing in mind that in those paragraphs Bohr is talking of the two-slit experiment – it being understood that the EPR experiment is no different in principle from the two-slit experiment, and to explicate one is to explicate the other. (I shall have more to say about this in Chapter 5.)

[34] Bohr 1935b, p. 699. (In Wheeler & Zurek 1983, p. 148.)

[35] Bohr 1935b, p. 699. The reference to 'finite and uncontrollable interaction between the objects and the measuring instruments' appears to refer to the transfer of uncontrollable quanta from the observing apparatus to the system under observation. Such transfer is equivalent to uncontrollable changes in the wave function of the quantum system owing to the appearance of random phase factors in the wave function – the destruction

Notice that Bohr doesn't speak of the conditions having to do with *producing* the state of the physical system under consideration (if he did, the EPR case would be unanswerable because that would entail both separability and faster-than-light signalling)[36], but of the conditions having to do with *defining the possible types of predictions* regarding the future state of the system.

> ... It is just this new situation as regards the description of physical phenomena that the notion of *complementarity* aims at characterizing.[37]

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of definite phase relations between the various parts of the wave function by a measurement resulting in the destruction of the interference terms in the QM formalism. The effect of which is to leave the system in a more or less classical state as regards the measured variable, while rendering its conjugate almost totally indeterminate. For details regarding the transfer of such uncontrollable quanta, see Bohm 1951, pp. 121-31, 600-4.

More generally as regards Bohr's position, I've mentioned that even the great John Bell said that he had little understanding of it. Here is what Bell had to say on the above passage: 'I do not understand in what sense the word "mechanical" is used, in characterizing the disturbances which Bohr does not contemplate, as distinct from those which he does. I do not understand what the italicised passage means – "an influence on the very conditions..." Could it mean just that different experiments on the first system give different kinds of information about the second? But this was just one of the main points of EPR, who observed that one could learn *either* the position *or* the momentum of the second system. And then I do not understand the final reference to "uncontrollable interactions between measuring instruments and objects", it seems just to ignore the essential point of EPR that in the absence of action at a distance, only the first system could be supposed to be disturbed by the first measurement and yet definite predictions become possible for the second system. Is Bohr just rejecting the premise – "no action at a distance" – rather than refuting the argument?' (Bell 1987, pp. 155-6.) Bell was of course fully conversant with the formalism of quantum mechanics and how the destruction of interference in the quantum mechanical formalism comes about upon measurement. His point regarding Bohr's 'uncontrollable interactions' seems to be that in so far as the wave function of the combined system of the two particles and the measurement device is taken to extend between the separated particles, and the second particle is affected by a measurement made on the first, then there is action at a distance, no matter by what means it arises in the formalism of quantum mechanics. There must be action at a distance just to the extent that the wave function is affected *throughout all the space between the particles* – even if the wave function is defined in non-relativistic quantum mechanics over the configuration space of the system. However, if this is what Bell had in mind, Bohr would no doubt reply that Bell is making the mistake of assuming 'separability' – that the world can be analysed in terms of distinct and separately existing elements of reality – one of the elements of the EPR assumption of locality. And to assume that the wave function is an object that is somehow *located* in the space connecting the two particles would be to make essentially the same mistake. It would be more accurate to say that what is classically regarded as the pair of particles and observing apparatus, are *constituted* by the wave function.

In a later paper Bell remarked, for good measure, 'Einstein himself had great difficulty in reaching a sharp understanding of Bohr's meaning. What hope is there for the rest of us?' (Bell 1987, p. 189.)

[36] As d'Espagnat (1989b, p. 159) points out.

[37] Bohr 1935b, p. 699. (In Wheeler & Zurek 1983, p. 148.)

In later lectures and writings, Bohr specifically warned against the use of phrases such as 'disturbing of phenomena by observation', or 'creating physical attributes to atomic objects by measurement' (for example in the context of discussing the apparent paradoxes of quantum theory), because they are apt to cause confusion.

> As a more appropriate way of expression I advocated the application of the word *phenomenon* exclusively to refer to observations obtained under specified circumstance, including an account of the whole experimental arrangement.[38]

It is for such reasons that Bohr wrote that

> in quantum mechanics, we are not dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with a recognition that such an analysis is *in principle* excluded.[39]

In brief, then, Bohr's claim is that we have reached both *epistemological* and *physical* explanatory bedrock with the quantum-mechanical account of the entangled wave functions of the two particles constituting a single wave function, and the related notion of complementarity. Bohr and the Copenhagen theorists arrived at this position largely because it seemed to them that there was no further room to manoeuvre. The predictions of quantum mechanics were unambiguous and, in any case, the great mathematician von Neumann had proven in 1932 (so it was thought) the impossibility of the existence of 'hidden variables' that might restore some kind of objective reality in Einstein's sense to quantum objects.[40] The situation became even more uncomfortable for the Einsteinian realist after Bell's 'impossibility proof' in 1964.

It is important to realize that Bohr's reply to EPR was not a knockdown argument, in the sense of showing inconsistency or experimental error. It was rather an argument to show that the quantum theorist could give a *consistent interpretation* of the quantum-theoretical predictions of the results of the EPR type experiment while retaining special relativity. It's not that Bohr claimed that EPR had made any *mistake* in their paper, just as, for their part, EPR never claimed that the predictions of quantum mechanics were wrong. The EPR apparatus would do

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38 Bohr 1949, pp. 237-8.

39 Bohr 1949, p. 235.

40 In his 1932 book, *Mathematische Grundlagen der Quantenmechanik* (*Mathematical Foundations of Quantum Mechanics*), von Neumann was supposed to have proved that 'no conceivable distribution of motion of "hidden" parameters in the observed system could lead to precisely the same results as those of Schrödinger's equation, plus the probability interpretation of the wave function'. (Bohm 1957, p. 86.) But the proof, though mathematically valid, rested upon an arbitrary and physically unreasonable assumption. For discussion, see Bell 1983a (in Wheeler & Zurek 1983, pp. 398-9), and Bohm 1957, pp. 95-6; see also Bell 1987, p. 4.

just what EPR said it would. It could be used first to measure, indirectly, the exact momentum of the distant particle. It could then be modified to measure, indirectly, the exact position of the distant particle. But these are two distinct experiments according to Bohr, the second involving a modification of the measuring apparatus and thus of the 'very conditions which define the possible types of predictions regarding the future behaviour of the system'. If one uses the EPR set-up to measure the momentum, then the position is thereby rendered completely uncertain, and vice versa. We can choose to investigate either one of these complementary properties, but that will preclude simultaneous investigation of the other. The experimental arrangements suitable for determining a particle's momentum-energy, and for locating a particle in space and time, are mutually exclusive. In modifying the experimental apparatus, we are, in the words of Bohr,

> not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrificing other such elements, but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of a space location, or for the legitimate application of the conservation theorem of momentum... Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way.[41]

In the context of hidden-variables theories, this is known as contextuality.[42] In 1949 Bohr added:

> ... it obviously can make no difference as regards observable effects obtainable by a definite experimental arrangement, whether our plans of constructing or handling the instruments [i.e. the selection of the entire experimental arrangement] are fixed beforehand or whether we prefer to postpone the completion of our planning until a later moment when the particle is already on its way from one instrument to another.[43]

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41 Bohr 1935b, p. 700. (In Wheeler & Zurek 1983, p. 149.)

42 D'Espagnat 1995, pp. 225. Contextuality is to do with context-dependency. Take a *contextual* hidden-variable theory of some kind. It is one in which the truth value of each of the quantum-mechanically recognized eventualities is not determined by the complete state φ, but the outcome of each experimental test of an eventuality is determined by φ together with some features of the experimental arrangement. In contrast, a *non-contextual* hidden variable theory is simply one in which the complete state assigns a definite truth value to each of the quantum-mechanically recognized eventualities, i.e. to each one that corresponds to a projection operator. (After Isham 1989, p. 382.)

43 Bohr 1949, p. 230.

In view of Wheeler's 1977 'delayed choice' version of the experiment (§1.2[e]), this was a very pregnant remark indeed. See §§3.5 & 5.1.2. for some reflections on it. D'Espagnat, too marvels at Bohr's physical intuition in anticipating 'what nowadays we call contextuality'.[44]

Most philosophical critics of Bohr's position have missed the implications of Bohr's above remark, if, indeed, they've noticed it at all. Take Tim Maudlin and the original EPR argument (in the Bohm version). He claims that careful consideration shows Bohr's remarks to be 'completely inappropriate to the EPR experiment'.[45] He first notes that the original quantum state assigned to the pair of photons ascribes no definite polarization to either photon. But once the left photon passes through a polarizer (set to say 0°),

> that state is changed to one in which the right-hand photon *has* a definite polarization and will definitely pass [through a polarizer on the right set to 0°]. So the question is not what role *experimental arrangements* on the left have in defining quantities on the right but rather how one is to understand the *change in state* on the right which follows the *outcome* on the left. If no physical change has occurred, then the photon on the right which has a definite, well-defined polarization after the left-hand measurement, must also have had one before. So the original quantum state was incomplete.[46]

Unsurprisingly, Maudlin also claims that 'this dependency constitutes a causal connection'.[47] Moreover, the connection shows that 'the underlying true dynamics must be deterministic in order to account for the perfect correlations'.[48]

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44 D'Espagnat 1995, p. 225. He writes (1995, p. 290) that 'it seems natural to expect that in a deterministic hidden-variables theory, measurement of an observable would yield the same value independently of the "context", that is: quite independently of the other measurements that may be made simultaneously with this one.' This is not generally true for quantum mechanics. Consequently, quantum mechanics is contextualistic. The 1967 Kochen & Specker theorem, for one, proves its contextuality (1995, pp. 225, 291). Quite generally, according to d'Espagnat, contextuality in quantum mechanics appears as a generalization of Bell's theorem and nonlocality (1995, pp. 290-1). No doubt that is so. However, it is worth explicitly stating that 'contextuality' also appears from Bohr's doctrine of complementarity (which of course preceded Bell's theorem), and the doctrine of complementarity in turn arose from the principle of quantization. D'Espagnat also notes (1995, pp. 291-2) that the notion of contextuality has been generalized to non-deterministic (stochastic) hidden-variables theories, and that it is possible to prove that 'there exist quantum systems for which some experimentally verifiable quantum predictions cannot be reproduced by any [stochastic, non-contextual hidden-variables] theory'.

45 Maudlin 1994, p. 143.

46 Maudlin 1994, pp. 143-4.

47 Maudlin 1994, p. 139. Maudlin sees no inconsistency between superluminal causal connections (involving superluminal transmission of information) and relativity. Indeed, as we saw in a note in §1.4, he argues, using physically unrealistic models, that relativity *per se* doesn't rule out even superluminal *signalling*.

48 Maudlin 1994, p. 144. Maudlin seems to be pretty much on his own here. Compare his position for example with that of van Fraassen who writes (1991, p. 94) '... there are

However, Maudlin begs the question, as his argument presupposes the very thing that is at issue and which Bohr emphatically denied, namely that a quantum system may be divided into distinct parts that can be 'affected' or 'changed' by a measurement. In particular, Bohr would deny that even *after* a sharp measurement – let alone before – a photon would have a well-defined state of polarization independently of the total experimental situation, as his remark anticipating Wheeler's 'delayed-choice' experiment clearly shows. (See also §5.1.2 for discussion.) Oblivious to this crucial point, Maudlin sanguinely concludes, 'In sum, EPR had a perfectly cogent argument against the completeness of quantum mechanics if one grants that distant measurements cannot affect the physical state of a local system.'[49] But that is just the kind of language that Bohr warned against, insisting instead that the photon and measuring apparatus exist together as a *single system* that will 'defy any closer analysis'.[50] The key to Bohr's response to EPR is his doctrine of complementarity, which Bohr used to respond to just the kind of picture put forward by Maudlin.[51] Simply reiterating the EPR argument and ignoring a crucial element of Bohr's reply to EPR does not amount to Maudlin having proven his case.

So far, we've concentrated on the original EPR experiment, in which measurements were made only on the *first* member of the particle pair – either a momentum measurement or a position measurement. Both the corresponding properties of the second particle were then *inferred*, on the basis of the fact that the choice of actual measurement was arbitrary – the measurement of the non-commuting property that wasn't made *could* equally have been made in place of the one that actually *was* made. But for the purposes of the rest of this chapter it may be instructive to look at a variant of the original EPR experiment. What if an *actual* measurement of an incompatible property were made on the *second* particle – a possibility not explicitly contemplated by EPR in their paper, but contemplated by other authors subsequently? For example, suppose that we measure the first particle's momentum, and the second particle's position. Our momentum measurement would give not only the momentum of the first particle but, indirectly, also that of the second particle, without disturbing the latter in the slightest. Armed with complete knowledge of the second particle's momentum, we can

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possible phenomena for which it is logically *impossible* to have a causal model', namely phenomena which would violate Bell's Inequalities (see §3.3). Moreover, such phenomena seem to exist, for 'without any reliance on theory, we can show that the experimentally established EPR/Bell 'frequency counts display an unacceptably – indeed, incredibly – bad fit to the probabilities of any causal model whatsoever' (1991, p. 95). And on p. 84 he writes that in the quantum-mechanical world, causality is lost even in Reichenbach's minimal sense – which is that causality exists not for every event but for every correlation. For a discussion, see van Fraassen 1991, pp. 81-4.

49 Maudlin 1994, p. 144.

50 Bohr 1935b, p. 701.

51 For a fine analysis of Bohr's response to EPR in terms of complementarity, see Bub 2000, pp. 196-204.

then go on to measure the second particle's exact position – which can be done simultaneously with the first measurement if desired, and to an arbitrary degree of precision. Although our measurement will disturb the second particle, such disturbance is relevant only in respect of any *subsequent* measurements, at least, if EPR are to be believed. But that's not a worry for us, since in the present experiment we're concerned only with what we can know *now*. And our experiment appears to give us complete knowledge *now* of the second particle's simultaneous momentum and position, contrary to Bohr – unless, of course, as EPR would be quick to add, Bohr is prepared to give up special relativity. Does this variant of the original EPR bring anything essentially new into the debate? How good is Bohr's reply to the original EPR in also answering this hypothetical objection?

Bohr would reply that the second measurement would add nothing of substance to the original EPR experiment, and therefore nothing would need to be added to his reply. That's evident from his response to the original EPR paper and his subsequent writings. But to get a better handle on Bohr's response to the above variant of EPR, and to EPR generally, it may be helpful to look at that response in terms of *counterfactual definiteness*. The central move in Bohr's response to EPR was to deny separability,[52] one of the three elements of the locality assumption, as we've seen. Denying separability while also denying faster than light influences amounts to a denial of counterfactual definiteness in the usual sense of that term (though not a denial of counterfactual definiteness *tout court*).

It seems to me that Bohr's response to EPR is equivalent to a proposal for modifying the usual scope of 'counterfactual definiteness', when that term is applied to physical reality on the microlevel.[53] An examination of the above variant of EPR enables us to see, I think, how Bohr's position entails such modification. The modification amounts to *partially retaining* and *partially rejecting* the counterfactual definiteness of physical reality in EPR's sense, that sense corresponding to the usual one. It is evident that Bohr at least partially retains the EPR understanding of counterfactual definiteness (of physical reality), because he would accept the truth of the following statement: *If* the second measurement had been a measurement of the corresponding property as the first measurement – i.e. momentum, which in fact it wasn't – *then* the second measurement would

52 That is, to deny that the world can be correctly analysed in terms of distinct and separately existing elements of reality.

53 We've seen that a counterfactual is a contrary-to-fact subjunctive conditional, in the present examples referring to some past event (s) ['if it had been the case that A, then it would have been that B'], though the reference to past events is not a necessary property of counterfactuals. We've also seen that there are subjunctive conditionals which are not counterfactuals, e.g. 'If it were to be the case that A, then it would be the case that B' (e.g. if explorers were to discover a several-thousand mile wide opening at the North (and/or South) Pole leading to the centre of a hollow Earth, then the 1818 theory of U.S. Infantry Captain John Cleves would be confirmed.

indeed have yielded the value of that property inferable from the first measurement. Moreover, repeat measurements would have continued to yield an inferable value, though one would need to calculate and take into account the change in momentum arising from the Compton effect of the measurements, and correct for it.[54] The piece of reasoning conveyed by the preceding statement completely accords with our usual understanding of counterfactual reasoning: that hypothetical actions – actions not in fact performed but which could have been performed – would have led to certain *particular* outcomes (not just *any* outcomes), and sometimes those outcomes are predictable. It is also consistent with EPR's criterion of physical reality, which Bohr accepted. Our natural bent is to go on to say that such a state of affairs is explicable only if the second particle *actually possessed* the real property of a determinate momentum all along. This is just what EPR did. They inferred the existence of a reality that is essentially independent of ourselves, regulated by physical law (or symmetry principles, or whatever).

Bohr, on his part, would make a much more limited inference, namely that the second particle would indeed have possessed the property of a determinate momentum at the instant when the second (momentum) measurement was made (owing to the preceding measurement of the first particle's momentum), *had such a measurement been made*. But since the actual second measurement (in the case we are now considering) *wasn't* in fact a measurement of the corresponding quantity (momentum), but of the *complementary* quantity (position), he would say that we may not, as it turns out, infer that the second particle possessed the property of a determinate momentum at the instant of the second measurement.[55] That would be false. The most we may legitimately say (that is permitted by quantum theory) is that the second particle possessed the property of a determinate *position* at the instant of the second measurement (the value of that position being given by the second measurement). If, however, the second (position) measurement of the second particle isn't made, all we can say, rather trivially, is that it *would* possess a determinate position if such measurement were made – but in the absence of such measurement it does not possess any such property. Thus, Bohr partially rejects the counterfactual definiteness of physical reality in EPR's

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[54] The fact that repeat measurements disturb the momentum doesn't seem problematic here. Bohr's views about a like disturbance in the context of Heisenberg's gamma-ray microscope thought-experiment are well known. Rosenfeld writes, 'Bohr admitted that it was true that the electron gets new momentum from the Compton effect, but he said we can calculate the change of momentum, and therefore correct for it. So it is *not* something which we cannot know or observe.' (Rosenfeld 1979, p. 23.) See also Bohm 1951, p. 592 regarding such correction. Moreover, it is possible in principle to design a measurement apparatus with which any given variable can be measured such that the *measured* variable suffers no change. See Bohm 1951, pp. 591-2.

[55] This is of course because, when its exact position is measured, its momentum is completely indeterminate according to quantum theory.

sense.

Bohr's strategy, then, is to restrict the scope of counterfactual definiteness (CFD) when applied to physical reality on the level of the microworld, by leaving out everything that is in principle untestable, but retaining everything that isn't. That way, he still subscribes to CFD, but it is a CFD of restricted scope – the point being that CFD of the restricted scope described above is consistent with both quantum theory and relativity whereas CFD of unrestricted scope is not. Bohr would not want to deny CFD outright. That would not be a cost-effective thing to do, since CFD is essential for *any* theoretical understanding of the world.[56] So Bohr retains CFD – but for him CFD has a restricted scope on the

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56 Our theoretical picture of the world relies crucially on counterfactual conditionals. Implicit in any scientific theory or model is the understanding that if the past had been different, the future would have been different, too – which only makes sense if CFD is assumed. Maudlin (1994, p. 131) makes the same point.

Not everyone would agree. D'Espagnat (Schommers 1989, pp. 130-1) claims that there are some types of probabilistic laws in which, at least on a first analysis, *no* hidden reference to counterfactuals is to be found. In particular, he has in mind the kind of view – such as Bohr's – in which it is asserted that any analysis of a quantum phenomenon must take into account the whole experimental set-up. No counterfactuals are involved, according to d'Espagnat, if such an assertion is taken literally, because the assertion implies that 'we can meaningfully speak of the probability of obtaining the result *a* upon measuring observable *A* only if an instrument fit for measuring *A* is actually in place, or if, at least, we have decided once and for all that such an instrument is to be used. We may then imagine that somehow the measurement is actually repeated and... we may identify , in the usual way, the probability with a frequency limit.' [On pp. 92-3 d'Espagnat defines probabilities 'not in a formal, but in an operational way, that is essentially as (limits of) frequencies of occurrence over statistical ensembles whose number of elements is very large'.] D'Espagnat writes: 'No counterfactuals are involved [in Bohr's view] since all the elementary measurements composing the set [of all repeat measurements] are assumed to take place either in the actual world or – what amounts to the same – in possible worlds that are identical to the actual one, in particular as regards the presence of the instruments.' (D'Espagnat goes on to say that, as a rule, the physical probability laws are not of this kind, not in classical physics nor even in the usual formulation of quantum mechanics. In both of these, 'when we say that the probability of such and such a value of an observable *A* is equal to some number *p*, what we mean is that *if* an appropriate instrument were placed at an appropriate location and if the experiment were repeated a large number of times, the value in question, let us call it *a*, would be obtained in a fraction of the cases (approximately) equal to *p*. And we consider such a statement to be meaningful even in the cases in which no instrument is actually set that way and even in the cases in which another instrument, fit for measuring an observable which is incompatible with *A*, is in place instead. As already noted, when the probability laws are understood in this manner it should be clear that – just as the deterministic laws – they implicitly involve counterfactual implications.' [pp. 130-1])

I agree with d'Espagnat that in the usual formulation of quantum mechanics the physical probability laws implicitly involve counterfactuals. But it is not clear to me that in Bohr's interpretation of the probability laws there is no hidden reference to counterfactuals. See the main text above. In any case, Bohr insisted that measuring instruments are to be described classically. So to that extent at least, Bohr seems to accept CFD. As I see it, Bohr merely reduces the scope of CFD; he does no give up CFD altogether.

microlevel.

In brief, then, Bohr's hypothetical reply, couched in terms of counterfactual analysis, to our variant of EPR would be as follows. Our variant crucially depends on accepting the same scope for the CFD of the microworld as EPR's original argument did.[57] Since the original argument itself crucially turns on accepting or rejecting that scope, our variant adds nothing essentially new to the argument. (And since the unrestricted scope of CFD is an unwarranted assumption when applied to the microworld, he need not add anything to his original reply either.)

Leaving aside for now both counterfactual definiteness and the 'Vienna Circle' flavour of the Copenhagen reply, and staying with the bare physics (in so far as that is possible), the realist may object that to invoke an 'explanatory bedrock' is little more than another way of stating the original problem, and it is a matter of opinion whether we've reached bedrock. Price, for example, writes, 'The state function tells us *that* possible results of measurements are correlated, but not *why* this should be so. It gives us a *description* of the correlation, but not an *explanation*'.[58] After all, if there is nothing 'out there' in reality except the quantum fog, 'why is it that what materializes at one side always stands in the same relation to what materializes at the other? Why is there such a constraint on what would appear to be distinct indeterministic processes?[59] There seems to be a puzzle here which could only be resolved by adding something to the formalism of quantum mechanics – in other words, by conceding that quantum mechanics does not provide a complete description'.[60] In Price's view, while it is always possible that at a given stage in physics we may strike bedrock, it is a *prima facie disadvantage* of a theory if it requires us to suppose that we've actually done so – and it is foolish to paint oneself into a corner in that way. I agree. Price's way of looking at the matter is especially appealing of course if one thinks that one has found a way of making progress (such as utilizing advanced action in Price's case).

The upshot was that, as late as 1948 Einstein was able to write, speaking of local causality, '... I still cannot find a fact anywhere which would make it ap-

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57 We noted earlier that in EPR, locality is expressed in terms of hypothetical results: *If* a certain measurement had been performed – but which wasn't – *then* a certain result would have been obtained. Unless counterfactual definiteness of *some* kind is presupposed, the notion of locality cannot even be formulated (for example, it would make no sense to talk of the speed of light as the upper limit for the transmission of influences between systems, because the notion of a *disturbance* of the existing system by an incoming influence would make no sense).

58 Price 1996, p. 204.

59 Mermin (1985, p. 47) also asks this question.

60 Price 1996, pp. 203-4.

pear likely that that requirement will have to be abandoned'.[61]

3.3 Bell's impossibility proof: introduction

The reasonable thing just doesn't work...

(J.S. Bell to J. Bernstein, in Bernstein 1991)

For nearly thirty years physicists and philosophers debated the conclusions of the EPR article. Was quantum mechanics incomplete? If it was, and given that its predictions were correct, there had to exist a 'substratum' of physical reality on the microscopic level (so-called 'hidden variables' or 'inner properties' of quantum objects), lurking behind the formalism, accounting for the missing 'causes' of quantum mechanical phenomena – just as in statistical mechanics for example the atoms act as 'hidden variables' whose causal behaviour accounts for the laws of thermodynamics. Most physicists believed there weren't any hidden variables in quantum mechanics – that quantum mechanics couldn't be interpreted as some kind of 'statistical mechanics' in the classical sense; it was complete in itself and *intrinsically* probabilistic. What about the EPR thought experiment, then? Most physicists would have said that its conclusions were probably wrong for the reason Bohr had identified. In any case, it was difficult to test, since it was only a thought experiment. What was needed was some *fact* that would decide between EPR and Bohr. Then in 1964 John Bell came up with a 'fact'. He proposed a real experiment,[62] using a generalization of David Bohm's 1951 reformulation of the EPR experiment. In Bohm's reformulation, the momentum and position functions are replaced by spin functions.[63] To understand Bell, we first need to

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61 Cited in Bell 1987, p. 150. Einstein (1949b, p. 674) also refers to the lack of any such fact.

62 Strictly speaking, Bell's experiment, too, was a thought experiment, in the form of a mathematical proof. But its advantage over the EPR thought experiment was that (as Bell said) it 'requires little imagination to envisage the measurements involved actually being made'.

63 Spin is the intrinsic angular momentum of an elementary particle or group of particles. The fundamental unit of spin angular momentum is $\frac{1}{2}\hbar$. The spin can be either $+\frac{1}{2}\hbar$ or $-\frac{1}{2}\hbar$, the difference between the two being $\hbar$. Think of the wave function (or state vector) of the quantum particle such as an electron as rotating in an abstract space. (A classical analogue would be a circularly polarized electromagnetic wave.)

Although there are similarities between quantum spin and classical angular momentum, there are also fundamental differences. For example, classically, a projection of the spin vector through the poles of a spinning object into the x, y and z axes is the component of the spin along those axes. Adding the three components together using vector arithmetic gives the total spin. In the case of quantum spin, however, experimental knowledge of any one of the three spin components precludes in principle any possibility of knowledge of the other two components (even though the total spin is known). The entire spin component is always along the axis along which the spin measurement is made. This non-classical property of quantum systems is known as *space quantization*.

take a quick look at Bohm's reformulation.[64] That will be followed by a more detailed description of Bell's experiment.

Bohm asks us to consider a system composed of two spin-half particles in a singlet state. A singlet state is one in which the spin angular momenta of the particles add up to zero.[65] (See Appendix[d].)

The spins of such a particle-pair are described by a single wave function that is a superposition– a linear combination[66] – of the basic wave functions of the two particles, each possessing two eigenstates of zero total angular momentum. We may write it as follows:

$$|\psi_0\rangle = \frac{1}{\sqrt{2}}(\psi_1 - \psi_2).$$

Writing out ψ_1 and ψ_2 in full, we have:

$$|\psi_0\rangle = \frac{1}{\sqrt{2}}\left(|\alpha\uparrow\rangle|\beta\downarrow\rangle - |\alpha\downarrow\rangle|\beta\uparrow\rangle\right),$$

where the '–' sign refers to an *alternative*: the two wave functions ψ_1 and ψ_2 are mutually exclusive, in the sense that only one of the two conjunctions will be found upon measurement.

The two particles then move apart in opposite directions, as in the EPR case. Although the particles are emitted from the singlet state with a random spin each, the two random spins are nonetheless perfectly correlated. Now, if the spin were a *classical* angular momentum variable, when the particles moved apart, each particle would continue to have every component of its spin angular momentum equal and opposite to that of the other. These correlations in the particles' angular momenta would be maintained by the deterministic equations of motion of each spin-angular-momentum vector separately, as the particles move apart, bringing about conservation of the separate spin-angular-momentum vectors. A measurement of the spin angular momentum of either one of the particles would immediately yield that of the other, indirectly without need of measurement, since the two are equal and opposites.

But the intention of EPR is to examine how this experiment is to be described in quantum theory, and to *attack the theory using its own predictions*. In

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64 See Bohm 1951, pp. 614-22 for the reformulation, including the mathematical details which are omitted here. The generalization of Bohm's reformulation which Bell used is that of Bohm & Aharonov 1957. We shall follow Bohm 1951 here.

65 An example would be an unstable positronium 'atom', constituted of an electron and positron orbiting each other. In the present case, one member of the particle pair in the singlet state has spin $+\frac{1}{2}\hbar$. and the other equal and opposite spin $-\frac{1}{2}\hbar$.

66 Recall that a linear equation is one in which the sum of two of its solutions is also a solution of the equation.

quantum theory, the experimenter is free to measure either the x, y or z component of the spin of particle 1, but not more than *one* of these components, in any one experiment. The choice to measure one spin component necessarily entails not measuring the others. It turns out that whichever component is measured, the results are perfectly correlated, so that if the same component of the spin of particle 2 is measured, it will always turn out to have the opposite value. It just cannot happen that the two will have the same value. Thus the measurement of any component of the spin of particle 1 provides, as in the classical case, an indirect measurement of the spin of particle 2, without in any way disturbing that particle – since, by hypothesis, they no longer interact. If we accept the criterion of physical reality provided by EPR (see above), it is clear that after we have measured e.g. $spin_Z$ of particle 1, then $spin_Z$ for particle 2 must be regarded as an element of reality, existing separately in particle 2, independently of the rest of the system. Since there is no interaction between the particles, this element of reality must have existed even before the measurement of the first particle's spin. Moreover, in each case, the experimenter is free to reorient the measuring apparatus in an arbitrary direction *at the last moment while the particles are already in flight* (to safeguard against possibility of signalling between the particles using some hypothetical mechanism permitted by special relativity – 'permitted' in the sense of not violating special relativity's prohibition of faster-than-light signalling) and thereby get a definite (but unpredictable) value of the spin component in any direction that he chooses. Since this can be achieved without in any way disturbing particle 2, and given the EPR criterion of physical reality, we are forced to conclude that precisely defined elements of reality must exist in the second particle corresponding to the simultaneous definition of all three components of its spin, even in the absence of measurement – each having the value *it would have if it were the one chosen to be measured*. Recall that the alternative according to EPR is that the experimenter's selection of a spin axis actually *generates* that spin component in the incoming particle – which is contrary to the physical reality criterion, and totally unacceptable to EPR. If the spin is generated by a random mechanism, it is then the machine's selection of the spin axis that generates the spin – equally unacceptable. This is the first EPR conclusion.

Since the wave function can specify, at most, only one of these components with complete precision, it then follows that the wave function does not provide a complete description of all the elements of reality existing in the second particle. This is the second EPR conclusion. Bohm remarks:

> If this conclusion were valid, then we should have to look for a new theory in terms of which a more nearly complete description was possible.[67]

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67 Bohm 1951, p. 615.

It was for the investigation of this conclusion that Bell proposed his experiment, which was set out in the form of an impossibility proof (the irreproducibility of all quantum-mechanical predictions by any local hidden variable theory). There were at least three possibilities.

(1) The original EPR picture. Precisely defined elements of reality must exist in both particles corresponding to the simultaneous definition of all three components of their spin, even in the absence of measurement – though the class of 'precisely defined elements of reality' may need to be extended by including in that class elements that are unobservable. Such extension may be necessitated by the statistical predictions of quantum mechanics regarding the spin correlation of particle pairs in space-time regions **A** and **B** when the spins of both members of the pair are measured and the axes for the measurement of their spins are at *intermediate* angles – between zero and 180°. The situation is then no longer as clear-cut as in the original EPR experiment, in which the axes agree and the spins are always perfectly correlated. In any case, in the original EPR picture the two particles are separate entities each of which always possesses a definite spin component along every axis. There is no need for faster-than-light influences. This is a *local* realist theory with Einstein separability.

(2) The alternative picture as conceived by EPR; namely a counterintuitive 'Jumping Jack' kind of reality, entailing violation of special relativity. A particle always possesses a definite spin component along some axis. The axis along which the particle possesses such spin component is changed by the next act of measurement whenever a different axis for the measurement is selected. Any such change is transmitted instantaneously to the particle's twin, no matter how far away, violating the speed of light as the limiting velocity of transmission of causal influences. The spin of the twin instantly changes upon receipt of the influence to conform to the new state of affairs so as to preserve the spin correlation between the two at all times. Thus, the reality of the physical properties of one member of the twins depends upon the process of measurement carried out on the other member. In particular, the state arrived at depends on the *kind* of measurement one chooses to take, and not only on the numerical result the measurement yields. (E.g. x-axis spin *or* y-axis spin; momentum *or* position...) Local causality fails in a most mysterious way. Recall that '[N]o reasonable definition of reality could be expected to permit this', had been the verdict of EPR. It should be noted that even in this picture, quantum mechanics doesn't permit actual superluminal *communication* for the reason identified in §1.3.3. Hence, despite the explicit violation of special relativity, we may characterize quantum me-

chanics in this picture as only *weakly nonlocal*.[68]

(3) The holistic alternative as conceived by Bohr (now known as the Copenhagen interpretation)[69]. No precisely defined elements of reality, observable or unobservable, exist in either particle in the absence of measurement, and for that reason there is no need for faster-than-light influences. The wave function defines a statistical ensemble to which the microsystem belongs.[70] This picture denies Einsteinian separability, with the consequence that quantum mechanics doesn't explicitly violate special relativity. If there *were* any superluminal 'influences', they would always be within a single extended system. However, even this would be to misrepresent the picture, at least as conceived by Bohr, as it denies *tout court* the existence of any 'influence', let alone superluminal communication. For that reason, quantum mechanics in this interpretation may be once again characterized as only *weakly nonlocal* (albeit even more weakly than in [2] above). In this case, the nonlocality simply amounts to a denial of Einstein separability.

For Einstein, of course, both of the above alternatives (2) and (3) to the EPR picture appeared 'entirely unacceptable'.[71] For the rest of his life, he continued to search for a deeper-lying theoretical framework which would permit the description of phenomena independently of the specifics of the experimental conditions of observation. That's what he meant by 'objective reality'.[72]

Bell had already written a paper (in the northern summer of 1964) on the problem of hidden variables in quantum mechanics.[73] He next immersed himself in the EPR-Bohr argument, and in September 1964 he proved what is now known

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68 This is in line with our usage in §1.3.3.

69 Here I've been speaking of *the* Copenhagen interpretation, as if there were only one such. But as we've seen in Chapter 1, the 'Copenhagen' interpretation exists in several versions. One is that of Bohr, which is commonly understood to maintain that unmeasured properties of a quantum object do not exist. A variant is the view set out in Bohm (1951, p. 620). Bohm writes, '... in general, all three spin components exist simultaneously in roughly defined forms, and any one component has the potentiality for becoming better defined at the expense of the others if the associated system interacts with a suitable measuring apparatus'. We saw in Chapter 1 that according to what is now taken as the standard interpretation, the wave function gives a description of the real properties of an individual system even when they are not observed. When the wave function of a system is made up of superposed wave functions, the system (e.g. Schrödinger's cat) may be thought to exist in *each* of the states represented by each of the superposed wave functions, and to participate in all their properties – even when the properties are incompatible. This is, I think, essentially Bohm's view expressed in the above-quoted passage.

70 Zimmerman 1966, p. 484. See Chapter 1 for details regarding ensembles, and the question of whether Ψ represents an ensemble or an individual system.

71 Einstein 1949a, p. 85.

72 Pais 1982, p. 455.

73 Bell 1983a.

as *Bell's theorem*.[74] He realized that there was something which both Einstein and Bohr had overlooked.[75] *Local realist theories entailed correlations between distant singlet particles that were different from those predicted by quantum theory.* Any conceivable local realist theory had a built-in upper limit to the correlations it could predict (or explain). Tighter correlations than that limit were a logical impossibility. There was no such limit on quantum theory, which predicted that the world is more tightly correlated than any local realist theory can explain, even in principle. This difference in the experimental predictions is known technically as 'violation of Bell's inequality'.

In particular, an actual measurement in the laboratory of the spins of large numbers of real particles in the x, y, and z directions *could distinguish in principle between possibilities (1) and (2) above*, given the assumptions of EPR – even when possibility (1) was strengthened by the assumption of hidden variables. If the EPR alternative (1) came up in the experiment, EPR were vindicated. If the EPR alternative (2) came up, ruling out (1), then EPR were wrong, and the 'Jumping Jack' kind of reality described above, with its violation of special relativity, was the case. But *only* if we accepted (contrary to Bohr) the implicit EPR assumption that the world can be correctly analysed in terms of distinct and separately existing 'elements of reality'. If we rejected this assumption, then only possibility (3), Bohr's interpretation, would be left out of the possibilities listed above.[76]

Thus, the way was open for an *experimental resolution* of the debate between Einstein and Bohr!

Bell's assumptions:

Bell accepted EPR's conclusions as premises of his argument. These were (when extrapolated to the spin-case):

The existence of precisely defined elements of spin even in the absence of observation

> Precisely defined elements of reality (the EPR assumption) must exist in particle 2 corresponding to the simultaneous definition of all three components of its spin, even in the absence of measurement – each having the value

74 Bell 1983b.

75 And everyone else, too, it would seem, except Tsung-Dao Lee. Jammer writes that Lee had anticipated Bell's essential idea by about four years. Lee was struck by certain correlations between simultaneously created neutral K-mesons. He realized that the situation was intimately related to the EPR one, and soon convinced himself that neither classical ensembles nor systems with hidden variables could ever reproduce such correlations. For more details, see Jammer 1974, p. 308.

76 The listed possibilities do not exhaust the possibilities, of course. The selection simply reflects the historical position at the time of Bell's work. We shall go on in §3.5 to look at other possibilities, including some involving modification of the quantum mechanical formalism.

it would have if it were the one chosen to be measured. And since it is arbitrary which of the pair of particles is measured first, the same conclusion must apply, *mutatis mutandis*, to particle 1. Thus, *both* particles must have possessed precisely defined elements of reality corresponding to the simultaneous definition of all three components of their spins even before measurement.

Quantum mechanics is incomplete

The quantum-mechanical description of physical reality is *incomplete*. (Since the wave function can specify, at most, only one of the three spin-components of each member of the particle pair with complete precision, it follows that the wave function does not provide a complete description of all the elements of reality.)

Bell also made two explicit assumptions:

Locality

He assumed locality – just as EPR had done. Here's how he put it: 'The vital assumption... is that the result B for particle 2 does not depend on the setting $\bar{a}$ of the magnet for particle 1, nor A on $\bar{b}$.'[77] In other words, the properties of interacting physical systems (such as the setting of the detector and the spin of the incoming particle in the EPR experiment) are *independent* before they interact.[78] There are several elements to this independence assumption. The main ones are:

(i) the EPR assumption that the world can be correctly analysed in terms of distinct and separately existing elements of reality;

(ii) that there is no action-at-a-distance, the speed of light being the limiting velocity for the transmission of influences between separated systems;

(Elements (i) and (ii) above are known together as Bell's doctrine of *local causality*: the doctrine that all the causes of an event must lie in its past light cone.)

(iii) an implicit assumption, also implicit in EPR but not identified in our

[77] Bell cited Einstein: 'But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former' (Einstein 1949a, p. 85).
[78] Though not necessarily *afterwards*, since interaction can obviously give rise to correlations.

> preceding analysis of it, that quantum-mechanical systems are not correlated with the settings of measurement devices in relevant ways *prior* to their interaction. This is another way of saying that there is no cosmic conspiracy or 'superdeterminism' at work, such that both the experimenter's choice of measurement setting and the particle's spin are already predetermined in the *past* in such a way that the two will be correlated no matter which setting the experimenter selects. (The assumption of superdeterminism would have the consequence, as Bell later pointed out, that the experimenter's subjective feeling of free will is an illusion.[79])

Bell's assumption of locality together with his denial of superdeterminism is known as his *independence assumption*.[80] (We shall investigate that assumption and the possibility of relaxing it in the next chapter.)

The explicit possibility of 'hidden variables'

> He explicitly allowed for the possibility of a deterministic 'hidden variable' theory, by extending the range of the class of 'precisely defined elements of reality' by including in that class elements that were *unobservable*. These were some kind of 'inner properties' of quantum systems, acting as the missing 'causes' of quantum mechanical phenomena.[81] Bell wrote: 'In a complete physical theory of the type envisaged by Einstein, the hidden variables would have dynamical significance and laws of motion; our λ [the missing element necessary for a complete specification of the state of a system] can then be thought of as initial values of these variables at some suitable instant.'[82] In contrast, EPR were noncommittal about the precise nature of the physical reality, save for its macroscopically localizable character.

Here are the details of Bell's proposed experiment/impossibility proof. (For simplicity we shall use David Mermin's easy-to-follow version of EPR and Bell's

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79 Bell 1987, pp. 100, 154.

80 Bell did not make this assumption explicit in his paper. It was something that he identified later (1987), in the context of considering local causality, and possible ways of evading the consequences of his own theorem.

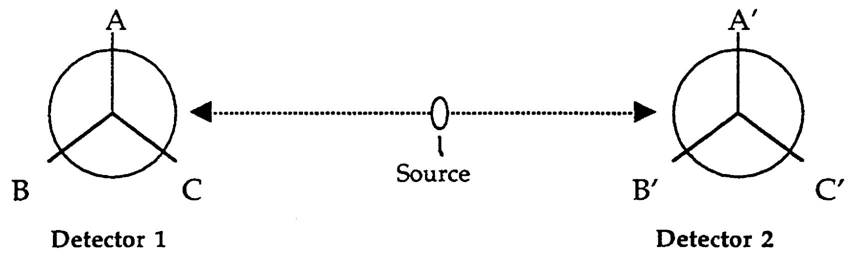
81 Home (1997, p. 195) emphasizes that Bell's notion of hidden variables is extremely general. Hidden variables can be regarded as simply *parameters* determining the outcomes of measurements on an event-by-event basis. 'In particular', he writes, 'it is not necessary to require noncontextuality in the sense of assuming that the premeasurement value of an observable is the same as the value obtained by a measurement.'

82 Bell 1983b, p. 404. Bohm writes: 'The idea that a particle has simultaneously well-defined values of position and momentum, which are uncertain to us, is equivalent to the assumption of hidden variables... that actually determine what these quantities are at all times, but in a way that, in practice, we cannot predict or control with complete precision.' (Bohm, 1951, p. 101.)

theorem,[83] which retains all the essential features.)[84]

3.4 Bell's proof

Consider once more a system composed of a pair of spin-half particles in a singlet state at the origin 0. The two particles then move apart in opposite directions, as in David Bohm's version of EPR, toward a pair of detectors. The detectors are for measuring the particles' spins.



The detectors each consist of a Stern-Gerlach spin analyser – basically a magnet – which can be rotated through 360° in a plane perpendicular to the flight of the incoming particle, making possible a selection of the particle's spin component to be measured (whether its spin is up or down along a selected spatial axis).[85] Each detector in this particular experiment is limited to *three* measurement settings (for the selection of three possible measurement axes) at 120° to one another: say A, B, C at detector 1 and A′, B′, C′ at detector 2. The axes A, B, C are all in the one plane (perpendicular to the flight of the incoming particle as already mentioned), and parallel to the axes A′, B′, C′, respectively, which are also in one plane. Each detector (magnet) can be rotated at the flick of a switch to measure the spins of incoming particles along any one of the three axes.

The idea is to measure the spins of both particles along *arbitrarily selected axes*, and then compare results. Both detectors have automatic switching mechanisms whereby they randomly select (independently of each other) one of their three measurement settings for each new measurement after the completion of the preceding measurement, ensuring that arbitrary axes-selections are made for each measurement. The selections are always made while the incoming particles are already in flight toward their respective detectors, and at the last instant. This latter is done to strengthen locality. The idea is to ensure that nothing that is done to either particle can possibly affect the other. For example, how detector 1

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83 Mermin 1985. Mermin's version concerns photon polarization, whereas our version concerns fermion spin. Nothing of consequence hangs on the difference.

84 For discussion of Bell's proof and Bell's assumptions, see e.g. Mermin 1985, pp. 38-47; Earman 1986, pp. 213-19, 226-31; Fine 1986, pp. 59-63; Redhead 1987, Ch. 4; Hughes 1989, pp. 238-48; Penrose 1989, pp. 282-5; van Fraassen 1991, pp. 85-105, *passim;* Maudlin 1994, pp. 18-20, 87-98; d'Espagnat 1995, pp. 141-56; Home 1997, pp. 194-234.

85 Or a polarization analyser if we were doing a photon polarization correlation experiment. However, the present example is concerned only with fermion spin.

is set when the first particle traverses that detector should not be able to affect the second particle's 'decision' to be spin up or down along the selected axis at detector 2. If the detector settings are selected only at the last instant, any information by some hypothetical (presently unknown) mechanism concerning the settings of the detectors would have to travel faster than light between the particles to be able to make a difference to the spins of the incoming particles.[86]

The mechanism at detector 1 thus measures either the A, B or C component of the spin of the incoming particle but never more than one of these components. Similarly for detector 2. (It is worth repeating here that in quantum mechanics, in contradistinction to classical mechanics, the *entire* spin component is always along the measured axis. This property is known as space quantization.) Since the selection of axes is random at both detectors, the selections made at the two detectors may agree or disagree for any pair of particles, e.g. they may be, say, BB′, in which case they agree (the magnets are parallel), or BC′, in which case they disagree (the magnets are at 120°/240°). There are nine possibilities in all: AA′, AB′, AC′, BA′, BB′, BC′, CA′, CB′, CC′, three in which the selections agree and six in which they disagree.

Now repeat the experiment very many times using a fresh pair of particles each time. Statistically, in one third of all measurement runs, the selection of axes agrees, and in two thirds of the runs they disagree. The choice of axes is the first variable of the experiment.

The second variable of the experiment is whether the spins of each pair of particles are correlated or non-correlated when measured along arbitrarily selected axes. We're interested in the *ratio* of correlation to non-correlation when the experiment is repeated over and over.

The important characteristic of the singlet state for the above purposes is that the spins of the particle-pair emitted from that state are *jointly correlated, though individually random*. The latter feature means that the absolute direction of the spins varies in a random way for each fresh pair of particles. In other words, the measured spin of each member of a freshly emitted particle pair is as likely to be **up** as **down** along any selected measurement axis for that particle (parallel or antiparallel to the detector's magnetic field along that axis), and it

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86 Bell writes, 'Conceivably [the quantum-mechanical predictions] might apply only to experiments in which the settings of the instruments are made sufficiently in advance to allow them to reach some mutual rapport by exchange of signals with velocity less than or equal to that of light. In that connection, experiments of the type proposed by Bohm and Aharonov... in which the settings are changed during the flight of the particles are crucial.' (Bell 1983b, p. 407). Perhaps it ought to be added that in those variants of Bell's experiment (e.g. Aspect 1976), which take photon polarization, rather than spin, as the property to be measured, it is sufficient to wait until the photons are already in flight before selecting the axes, without additionally having to wait until the last instant to do so. That is because photons always travel at the speed of light.

must be one or the other. To take an example, the selected axis at the first detector may be A, in which case that detector will measure the incoming particle's A-axis spin as either **up** or **down** along that axis, with a 50% probability of either in the absence of a previous measurement (i.e. the spin is *random*). Say the result is **up**. If the second detector happens to select the corresponding axis A′, then there is 100% probability that it will measure its incoming particle's A′-axis spin as **down** (the two results are *correlated,* in the sense that the spins are the *opposites* of each other, as we'd expect having regard to the conservation of angular momentum). It turns out that, as in the EPR case, whenever the *same* components of the spins of the two particles are measured (when the two detectors are parallel), the measurement results are perfectly correlated, with the spins always having opposite values. Thus the measurement of any component of the spin of an incoming particle at detector 1 provides, as in the classical case, an indirect measurement of the corresponding component of the spin of an incoming particle at detector 2, without in any way disturbing that particle (since, by hypothesis, they no longer interact).

What happens when the selected axes at the two detectors do not agree, e.g. if the selected axes were, say, A and B′? Well, first consider what would be the case if A and B′ were at *right angles*. For detectors at right angles, quantum mechanics predicts that there would be *no* correlations – a spin down result at one detector is just as likely to be associated with a spin down as a spin up result at the other detector. What happens when the angle is neither a right angle nor zero? For any angle in this range, quantum mechanics predicts that there ought to be *some* correlation in the spins, greater than zero and less than 100%, the amount of correlation being a function of the angle. If A is **up**, say, then quantum mechanics predicts that there is a 75% probability that B′ will also be up; and a 25% probability that it will be **down**. If A is **down**, then there is a 75% probability that B′ will also be **down**, and a 25% probability that it will be **up**. (We shall have a closer look at the quantum-mechanical probability rule at work here presently.)

The quantum-mechanical probabilities have the following general properties:

(1) If the axis-selections of the pair of detectors are the same, i.e. AA′, BB′, CC′, the spins of the particle pair recorded at the detectors must always disagree: spin **up** at one detector entails spin **down** at the other, and vice versa. (This is an uncontentious property, predicted by both classical mechanics and quantum mechanics.)

(2) Whatever the axis-selections of the pair of detectors (i.e. whichever of the nine possibilities of the first variable is selected), statistically or in the long run the spin-measurements at the two detectors must agree as often as they

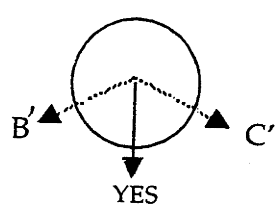
disagree. Quantum mechanics predicts that the probability of either AGREE or DISAGREE in any one case and in the total of all cases is 1/2. In other words, if we examine all measurement runs, with each of the two detectors set to select one of the three possible axes completely at random (yielding each time one of the nine possible configurations of the first variable), then we must find that the pattern of AGREE/DISAGREE for the spins is random.

Consideration of the latter property is the new element Bell added to the EPR analysis. He realized that property (2) is inconsistent with any local realistic model (at least if we ignore models utilizing advanced action, on which more later). However, it is predicted by quantum mechanics. For that reason a real experiment could distinguish in principle between the positions of Einstein and Bohr.

Here is how quantum mechanics predicts property (2):

Suppose that detector 1 acts first and finds the spin of particle 1 along one of the three randomly selected axes, say A. Suppose that the spin is **up** (i.e. parallel to the detector's magnetic field at along that axis). We might symbolize 'parallel to the field', i.e. **up** along that axis, by YES, and antiparallel to the field (or **down** along that axis) by NO. The result YES at detector 1 along A implies the result NO along A′: the spins DISAGREE at A and A′:

As for the other two axes at the second detector, namely B′ and C′, note that for YES along A, the axes B′ and C′ are each at 60° to the spin state that would be found to exist at the second detector, namely YES, if the spin were to be measured there along an axis antiparallel to A′. In other words, the axis to be measured at the second detector, either B′ or C′, is angled (rotated) by 60° to the hypothetical axis position at that same detector at which it is *certain* that a spin measurement would give a YES answer if such measurement were to be made.



At 60° to such axis, quantum mechanics predicts there is a probability of 75%, or

3/4 [given by 1/2(1 + cos 60°) = 3/4], that the spins measured along B′ and C′ would each be found to give YES, and thus AGREE with the YES result obtained at A, and a probability of 25% that each disagrees with that result (gives NO) [given by 1/2(1 + cos 120°) = 1/4].

(The general rule for the probability of the second spin measurement *agreeing* with the first spin measurement is given by the formula

$$P = \cos^2(\theta/2),$$

where θ is the angle of the second measurement *relative to the known spin direction of the particle whose spin is to be measured in the second measurement,* i.e. relative to the direction along which a spin measurement would be certain to give YES, if made on that particle. This rule applies equally to the cases of a *single* particle on which successive spin measurements are to be made, and a singlet-*pair* of particles, as in our examples above, where the members of the particle-pair are necessarily at different space-time locations.[87] On the other hand, the general rule for the probability of the second spin measurement *disagreeing* with the first spin measurement is given by the formula

$$P = \cos^2([180° - \theta]/2),$$

where θ is again the angle of the second measurement relative to the *known* spin direction at that space-time location. These formulae are equivalent, by simple trigonometry, to $P = 1/2(1 \pm \cos\theta)$, the formula we've used above.[88] [We use the (+) form when, whatever the result of the first measurement (i.e. YES/NO), we want to know the probability of obtaining an AGREE result in the second measurement, and the (–) form when, when (whatever the result of the first measurement) we want the probability of obtaining a DISAGREE result in the second measurement.])

Returning to our examples, if the first detector gives NO along A, then the axes B′ and C′ are at 120° to the spin state that would be found to exist at the second detector if the spin were to be measured there along A′. Consequently, it is more likely than not that the spins at B′ and C′ would each give NO, and thus again AGREE with the NO result obtained at A. In fact there is a probability of 25%, or 1/4 [given by 1/2(1 + cos 120°) = 1/4], that the spins measured along B′ and C′ would each be found to give YES, and thus DISAGREE with the NO result obtained at A, and a probability of 75% that each AGREES with that result (gives NO) [given by 1/2(1 + cos 60°) = 1/4]. However, to compensate, the probability of YES at A′ is in this case 1 or 100%.

87 The equivalent polarization correlation rule (when the particles are photons) is $P = \cos^2\theta$.

88 For photons, $P = 1/2(1 \pm \cos 2\theta)$.

Thus the *averaged probability* for the three second-detector settings to give YES if the first detector gives YES (i.e. to AGREE) is 1/2:

$$1/3(0 + 3/4 + 3/4) = 1/2.$$

Likewise of course (given the above result), the *averaged probability* for the three second-detector settings to give NO if the first detector gives YES (i.e. to DISAGREE) is:

$$1/3(1 + 1/4 + 1/4) = 1/2.$$

Similarly if the first detector gives NO. In that case the *averaged probability* for the second-detector settings to give YES (i.e. to DISAGREE) is 1/3(1 + 1/4 + 1/4) = 1/2. The *averaged probability* for the second-detector settings to give NO (i.e. to AGREE) is 1/3(0 + 3/4 + 3/4) = 1/2.

Here are the above results in tabulated form:

SPIN CORRELATION PREDICTED BY QUANTUM MECHANICS

AA′	DISAGREE:	1	AGREE:	0
AB′	DISAGREE:	1/4	AGREE:	3/4
AC′	DISAGREE:	1/4	AGREE:	3/4
BA′	DISAGREE:	1/4	AGREE:	3/4
BB′	DISAGREE:	1	AGREE:	0
BC′	DISAGREE:	1/4	AGREE:	3/4
CA′	DISAGREE:	1/4	AGREE:	3/4
CB′	DISAGREE:	1/4	AGREE:	3/4
CC′	DISAGREE:	1	AGREE:	0
	(3 × 1) + (6 × 1/4)		(3 × 0) + (6 × 3/4)	
	= 3 + 1.5		0 + 4.5	
	= 4.5		4.5	

TOTAL: 9, COVERING ALL POSSIBILITIES

Therefore the probability of AGREE or DISAGREE in all runs is 1/2, or 50%.

Inconsistency of the above QM predictions with local realistic models:

We earlier noted that one of Bell's assumptions for the purposes of his proof was the EPR assumption that the two particles are separate entities each of which always possesses a definite spin component along every axis. That assumption is tantamount to requiring that each particle somehow carries in its own body an 'instruction set' to its future detector telling that detector what measurement it is to give upon encountering the particle. That is so because there is no communication between the source and the detector other than the particle itself. As Price puts it, '... a single hidden state should be required to reproduce the quantum predictions for any possible next measurement'.[89]

Such an instruction set would have to be of a peculiar kind, taking into account (in the sense of partly depending on) the measurements obtained at the other detector for the other member of the particle-pair. Because a spin observation along one axis randomises the spin along an incompatible axis, and because of quantum-mechanical entanglement of the singlet-pair (described in Chapter 1), a detector at one location must always be prepared to measure the spin of a particle along any selected axis as being either (a) determinate (in the sense of already known) along that axis, or (b) indeterminate (unknown or unknowable) according to the above general quantum-mechanical rule, along that same axis, the particular kind of results obtained, either (a) or (b), being a *function of the measurement results obtained at the other location by the other detector* (nonlocality). Furthermore, we may select either particle as the recipient of the *first* measurement – the results obtained are independent of which particle is measured first.

The problem is, how do the particles manage to have their spins correlated in accordance with the quantum rule? How does each particle 'know' what the spin of the other is going to be at the other's detector (so as to be able to arrange its own spin accordingly)?

That is possible on the EPR picture of microscopic reality only if both particles have their instructions, YES or NO, for their respective detectors *prepared in advance for every possible combination of axis-selection at both detectors*. (We've seen that in the present experiment there are nine such combinations.)

Suppose for example that the answers carried by the first particle to its detector (detector 1) are YES, YES, YES, respectively, for the A, B, and C axes. In that case the answers carried by the second particle to its detector (detector 2) must be NO, NO, NO, respectively, for the A′, B′, and C′ axes, so as to be consistent with property (1). Likewise, if the answers carried by the first particle are, say, NO, NO, YES, the second particle's answers must be YES, YES, NO. The two parties' answers for the identical axis alignment must always be the opposite of each other.

That takes care of property (1). But taking care of property (1) has the consequence that property (2) cannot be met. (This consequence is disastrous for EPR's picture as there is no disagreement between EPR and the Copenhagen theorists regarding the predictions of quantum mechanics as regards *empirical phenomena*; see 'the validity assumption' in 'EPR's assumptions', earlier in this chapter.) Property (2) is that the ratio of AGREE to DISAGREE of the answers obtained at the two detectors must be 50/50 for all the possible pairings of measurement angle, nine in all. Take the first example above, YES, YES, YES for particle 1, and NO, NO, NO for particle 2. In that case there are nine cases of

89 Price 1996, pp. 125-6.

DISAGREE and no cases of AGREE for all the nine possible pairings:

	PARTICLE 1	PARTICLE 2	AGREE	DISAGREE
AA′	YES	NO	–	×
AB′	YES	NO	–	×
AC′	YES	NO	–	×
BA′	YES	NO	–	×
BB′	YES	NO	–	×
BC′	YES	NO	–	×
CA′	YES	NO	–	×
CB′	YES	NO	–	×
CC′	YES	NO	–	×
			TOTAL = 0	TOTAL = 9

Likewise, if the instructions were NO, NO, NO for particle 1 and YES, YES, YES for particle 2, there would again be nine cases of DISAGREE and no cases of AGREE. We might call this result the 'first category of AGREE/DISAGREE'.

The only other possibility is that the instructions carried by each of the two particles are in a 2 to 1 or 1 to 2 ratio of YES and NO, e.g. YES, YES, NO for particle 1 and NO, NO, YES for particle 2, or YES, NO, NO for particle 1 and NO, YES, YES for particle 2.

In that case, there are always 5 cases of DISAGREE and 4 of AGREE, no matter which combination of YES and NO we have. This is the 'second category of AGREE/DISAGREE'. Here are the results in a tabular form of taking YES, YES, NO for the first particle, and NO, NO, YES for the second particle:

	PARTICLE 1	PARTICLE 2	AGREE	DISAGREE
AA′	YES	NO	–	×
AB′	YES	NO	–	×
AC′	YES	YES	×	–
BA′	YES	NO	–	×
BB′	YES	NO	–	×
BC′	YES	YES	×	–
CA′	NO	NO	×	–
CB′	NO	NO	×	–
CC′	NO	YES	–	×
			TOTAL = 4	TOTAL = 5

There are no other possible categories of combinations of AGREE:DISAGREE than the two given above, namely nine DISAGREE, nil AGREE, or five DISAGREE, four AGREE. No matter how these possibilities are distributed, they can only give either a 9:0 or 5:4 ratio of AGREE:DISAGREE (or vice versa). Clearly, neither is the 50/50 ratio required by property (2). That is to say, *no combination of classically carried instruction sets can give property (2), that the probability of AGREE/DISAGREE in all runs is 50/50.*

This is David Mermin's succinct summary of what Bell managed to do:

> Bell's analysis adds to the discussion those runs in which the switches have different settings, extracts the second feature of the data [property (2)] as a further elementary prediction of quantum mechanics, and demonstrates that any set of data exhibiting this feature is incompatible with the existence of the instruction sets apparently required by the first feature, quite independently of the formalism used to explain the data, and quite independently of any doctrines of quantum theology.[90]

Bell concluded his paper with the following words:

> In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant.

Since Bell's original paper, his proof of the irreproducibility of all quantum-mechanical predictions by any local hidden variable theory (advanced action aside) has been given in different versions by several authors. The proof given above loosely follows a proof Penrose gives – which appears to be based on an earlier one given by Eugene Wigner in 1970.[91]

For completeness, it ought to be mentioned that Bell's proof is not the only problem for realist 'hidden variable' theories. There is also the later GHZ argument which achieves Bell's conclusions by non-statistical means, and the class of 'no hidden variables' theorems, culminating in the works of Kochen & Specker in 1967.[92] We shall not look at these results, as (a) their general conclusions are the same as those of Bell's theorem, and (b) they rely on the same assumption as Bell's theorem, namely the absence of pre-interactive correlations, or the independence assumption.

Experimental confirmation of the predictions of quantum mechanics regarding the spins of singlet-state particles when the spins are along arbitrarily

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90 Mermin 1985, p. 45.

91 Penrose 1989 and Wigner 1970. According to Jammer (1974, p. 309), Wigner's proof is probably the simplest one given to date.

92 Kochen & Specker 1967. For an analysis, see e.g. Hughes 1989, pp. 164-70.

selected axes wasn't very long in coming.[93]

Bell remained puzzled to the end of his life. He told Jeremy Bernstein:

> The discomfort that I feel is associated with the fact that the observed perfect quantum correlations seem to demand something like the 'genetic' hypothesis [identical twins, carrying with them identical genes]. For me, it is so reasonable to assume that the photons in those experiments carry with them programs, which have been correlated in advance, telling them how to behave. This is so rational that I think that when Einstein saw that, and the others refused to see it, *he* was the rational man. The other people, although history has justified them, were burying their heads in the sand. I feel that Einstein's intellectual superiority over Bohr, in this instance, was enormous; a vast gulf between the man who saw clearly what was needed, and the obscurantist. So for me it is a pity that Einstein's idea doesn't work. The reasonable thing just doesn't work.[94]

Einstein, too, remained puzzled – and unconvinced – to the end of his life. In 1952, less than three years before his death he wrote to his friend Michele Besso:

> ... A real state is not describable in the present quantum theory, which furnishes only an incomplete knowledge of a real state. The orthodox quantum theoreticians, in general, don't admit the notion of a real state (based on positivistic considerations). One ends up, then, in a situation that strongly resembles that of the good bishop Berkeley.[95]

One wonders what Einstein would have said had he lived to see Bell's proof. One thing at least seems certain, as Bell remarked in one of his papers: 'Einstein could no longer write so easily, speaking of local causality, "... I still cannot find any fact anywhere which would make it appear likely that that requirement will have to be abandoned"'.[96]

3.5 'How can quantum mechanics be like that?' The shaky game

Every absurdity now has a champion.

(J.L. Borges, Chronicles of Bustos Domecq, 1976)

'The shaky game' in the title of this section refers to the endeavour of interpreting quantum mechanics, with particular reference to the standard school of interpretation, and is taken from Arthur Fine's book of the same title. In Chapter 1 of that book, Fine notes that the principles and ideas that mark the development of

93 Clauser, Horne, Shimony & Holt 1969; Freedman & Clauser 1972; Aspect 1976.

94 Bernstein 1991, p. 84.

95 Quoted from Bernstein 1991, p. 161.

96 Bell 1987, p. 150.

the quantum theory display a curious parallel with those to be found in one of Borges' tales satirizing modernism – represented by the Uninhabitables, an architectural movement centred around the schizoid idea of using all the basic elements of habitable dwellings – doors, windows, walls, etc. – while abandoning the usual and ordinary connections between them. In a similar way, according to Fine, 'Niels Bohr's principle of complementarity is supposed to involve 'a rational utilization of all possibilities of unambiguous'... 'use of the classical... concepts'... while also systematically abandoning the usual connections between them'.[97]

Even though Fine's intention is to contrast Bohr's position unfavourably with that of Einstein, I think that something very similar could be said about Einstein's position. Einstein wrote in 1949 that the most effective scientist

> must appear to the systematic epistemologist as a type of unscrupulous opportunist: he appears as *realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as the free inventions of the human spirit (not logically derivable from what is empirically given); as *positivist* insofar as he considers his concepts and theories justified *only* to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research.[98]

Does this not represent an abandonment of the usual connections between the customary (or 'classical') concepts of realism, idealism, positivism, Platonism?

This kind of 'schizoid' juxtaposition of ordinarily mutually incompatible views is evident also in Einstein's position on the interpretation of quantum mechanics, even going on the basis of Fine's account of that position. According to Fine, the 'round picture' of Einstein's realism [his motivational programme] goes something like this: 'In the center are the linked, primary requirements of observer-independence and causality. Important, but not indispensable, are the secondary requirements of a spatio-temporal representation, which include separation, and of monism. This whole circle of requirements, moreover, is not to be interpreted directly as a set of beliefs about nature, but rather it is to be entheorized; i.e., to be taken as a family of constraints on theories. Realism itself is to be understood as a program for constructing realist theories, so conceived.'[99] Fine emphasizes that Einstein's realist language must not be taken at face value.

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97 Fine 1986, p. 1.
98 Einstein, in Schilpp 1949, p. 684.
99 Fine 1986, pp. 104-5.

The realism is, rather, motivational.[100] Moreover, the question of what the truth of these realist tenets amounts to is not one that can be answered. Such a question is rather to be deflected by turning to enquiry about the instrumental success (or failure) of the programme of some particular realist theory construction utilizing these tenets.[101]

Is this not very much like a rational utilization of all possibilities of unambiguous use of certain classical concepts (such as realism, instrumentalism and different theories of truth), while also being a systematic abandonment of the usual and ordinary connections between them?[102]

That being said, I agree with Fine's assessment of the nature of Einstein's realism. Moreover, it is an entirely appropriate heuristic outlook for a scientist. It is of course also in accord with the fact, puzzling to some, that Einstein never spelled out his own conception of quantum reality, and why his best-remembered contribution remained a negative one. The simple answer is that Einstein's own realist theory construction of quantum mechanics motivated by these tenets wasn't going anywhere.

As for the relative contributions of the two men, Fine assesses Bohr's contribution harshly, accusing Bohr, not Einstein, of being bound by classical concepts, unable to escape their limitations. (He has in mind, I think, Bohr's insistence on the indispensable use of classical concepts in the description of all proper measurements.[103]) He is particularly dismissive of the 'notoriously obscure' (p. 20) doctrine of complementarity, as the supposed Borges parallel shows.

I disagree with this part of Fine's analysis on both counts. I think the doctrine of complementarity shows that Bohr cannot be accused of being bound by the classical concepts in Fine's pejorative sense. All it shows is that we are, all of us, largely 'bound', in a non-pejorative sense, by classical concepts in our perception of the world and our need to talk about the world, as Bohr well knew. And quantum mechanics imposes certain limitations on our ability to do so as freely as we are classically accustomed to doing. Bohr's doctrine of complementarity was a self-consistent way of fully acknowledging this limitation, including its experimental consequences. But the question of which of the two men was the more classical and hidebound in his outlook is not what I want to dwell on. It is rather the quality of Bohr's contribution represented by the Copenhagen interpretation.

It needs to be remembered that the Copenhagen interpretation, used in conjunction with the mathematical formalism of quantum mechanics, has provided a

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100 Fine 1986, p. 111.
101 Fine 1986, p. 105.
102 On Einstein's use of the concept of 'truth', see Fine 1986, pp. 90-91.
103 See e.g. Bohr 1949, p. 209.

powerful heuristic for the investigation of quantum phenomena – one that continues to be powerful today. (In this regard there is no need to distinguish between the original Copenhagen and the standard interpretation of today.) The Copenhagen interpretation has also passed every experimental test.[104]

I'll give just one example of how powerful this heuristic is. Recall that in 1949 already, as we saw in §3.2, Niels Bohr managed to anticipate the nature of the 1982 results of Wheeler's 'delayed-choice' type of experiments, simply on the basis of his Copenhagen interpretation – and in particular his doctrine of complementarity.[105] This is worth dwelling on for a moment. Bohr's prediction seems to me on a par with Dirac's prediction of antiparticles on the basis of his (Dirac's) heuristic (see §6.3; for some related remarks see also §6.10.1). Bohr's doctrine of complementarity has been much derided by philosophers of realist inclination as incomprehensible.[106] Yet Bohr, using that supposedly incomprehensible doctrine, managed to make this significant prediction, while his philosophical critics have predicted nothing. Nor are the predictive successes of the Copenhagen interpretation's doctrine of complementarity limited to 'delayed-choice'. They are quite general. Englert, Scully & Walther report in their *Scientific American* article 'The duality in matter and light', that while they have been able to devise experiments that get around the Heisenberg indeterminacy relations, they have not been able to evade Bohr's principle of complementarity. They conclude that complementarity is deeper than has been appreciated – it is both more general and more fundamental to quantum mechanics than the Heisenberg indeterminacy.[107]

Fine himself notes that quantum theory is most often seen as a coherent framework for the predicting of experimental consequences, the principal justification of which lies in the 'uncanny success' of that enterprise.[108] He also acknowledges that '[t]hose inspired by realist ambitions [himself included] have produced no predictively successful physics'.[109] It is important to realize that when Fine talks about the quantum theory, he means not only the mathematical formalism of quantum mechanics but also its interpretation, i.e. the *Copenhagen* or standard interpretation, with its doctrine of complementarity. Not only has

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104 Schulman 1997, p. 200.

105 Following up on certain remarks by Bohr (see §3.2 for these remarks), Wheeler in 1977 described seven different versions of a gedankenexperiment in which such retroaction would be expected to occur, their common feature being that each imposed a choice between complementary modes of observation. (Wheeler 1978, pp. 9-48.) Wheeler's gedankenexperiment (beam-splitter version) was successfully carried out five years later by groups working independently at the Universities of Maryland and Munich, as reported by Horgan 1992, p. 75.

106 See e.g. Maudlin 1994, pp. 141-4, and of course Fine himself (1986, p. 20).

107 Englert, Scully & Walther 1994, p. 59.

108 Fine 1986, p. 22.

109 Fine 1986, p. 125.

the Copenhagen interpretation passed every experimental test, but complementarity is often the express heuristic (or rule of thumb) when devising the test, as was the case for example with Englert *et al.* above, as the authors affirm.

Even so, I think that Fine is onto something with his Borges parallel. However, I think the parallel applies not so much to Bohr's complementarity, in view of its predictive successes and continuing heuristic potential, but rather to the constructions of his successors in the Copenhagen tradition, none of which makes presently testable predictions. The same goes for some of the constructions of Einstein's successors in the realist tradition. In this latter category I have in mind especially David Bohm's famous causal theory.

In my opinion, all the prominent current interpretations, whether in the spirit of Bohr or Einstein (GRW, decoherence, consistent histories, modal, many worlds, Bohmian causal) are fatally flawed. Not so much because they don't make presently testable predictions. That, after all, can conceivably change tomorrow. Instead, the reason is to do with the nature of the solution (or rather, *non*-solution) offered by each to the interpretative problem, having regard to the nature of the interpretative problem itself. Here are the brief details. In what follows, owing to space constraints I shall only sketch each interpretation, without any attempt at a self-contained exegesis, and without going into the details of the variants of the interpretation. I shall assume that the details are known to the reader, and mention only those aspects of each relevant to my criticism of it.

Ghirardi-Rimini-Weber (GRW) theory

Before launching into the details of the theory, I'd like to remind the reader of the nature of the interpretative problem of quantum mechanics (as I see it), which ought to be kept in mind also when we come to the other interpretations discussed below. I want to emphasize that the interpretative problem is *not* the measurement problem of quantum mechanics (described in §1.3.1). *That* is merely an internal problem of the standard interpretation. The general interpretative problem is deeper than that. It is ultimately to do with the *non-separability* or 'wholeness' exhibited by interacting quantum systems.

The problem arises in the standard interpretation of quantum mechanics owing to its doctrine of wave-particle duality and complementarity, motivated by quantization (§1.1). From these flow the remarkable and sometimes problematic features of the standard interpretation, such as the existence of 'grotesque' or superposed states of matter, the interference of possibilities, nonlocality as manifested for example in the mysterious EPR-type correlation of widely separated quantum systems, the measurement problem, the lack of the full complement of classical attributes possessed by a quantum entity, and the generally intrinsically probabilistic behaviour of quantum systems.

What is the GRW solution? It is to retain the orthodox interpretation of the quantum state and to modify the unitary Schrödinger dynamics to achieve the required state evolution for both measurement and non-measurement interactions,[110] i.e. to provide a solution to the measurement problem. In it, both the Schrödinger evolution and the collapse are treated in the same lawlike way. As Bell puts it, 'in the GRW theory everything, including "measurement", goes according to the mathematical equations of the theory. Those equations are not disregarded from time to time on the basis of supplementary, imprecise, verbal, descriptions'.[111]

Here are the brief details. Although the wave function is always governed by the Schrödinger equation, it is also subject to spontaneous localization processes, i.e. spontaneous non-linear 'collapse', whereby the spread of the wave function is reduced to a very small range, c. 10^{-7}m. About every 10^{8} years, at random but with fixed probability per unit time, the wave function of a single free particle is multiplied or 'hit' by a strongly peaked Gaussian function with a general spread of σ. The wave function of the particle instantly becomes very localized (peaked), ready once more to begin its outward spread. The probability that the peak of the Gaussian finds itself in one place or another is proportional to the squared modulus of the wave function at that location. This ties in the interpretation with the squared modulus rule of the standard interpretation. Clearly, there is only a very low probability that any one particle will have its wave function reduced by being hit by the peaked Gaussian in this way in any given period of say one second (about one chance in 10^{-15}), so we wouldn't expect to observe any such events. On the other hand, when the number of particles is large, as in the case of say a small mouse ($\sim 10^{25}$ particles), we would expect a hit on average every 10^{-10} seconds. Any such hit would reduce the wave functions of *all* the particles in the object, owing to the entanglement of the wave function of the particle that is hit with those of the other particles making up the object. Hence a macroscopic object such as Schrödinger's cat with an even larger number of particles could exist in a state of superposition of macroscopically different states such as dead and alive for only a tiny fraction of a second.[112]

The main merit of GRW is that it provides a uniform dynamical law for all interactions, one in which the notion of measurement doesn't appear.[113] There is nothing in the theory but the wave function.[114] Moreover, the 'mechanism' provided by the uniform law for the collapse is such that the time-rate of the localization process depends in a natural way on how big the composite system or

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110 Bub 1997, p. 118.

111 Bell 1987, p. 205.

112 In this paragraph, I've followed Whitaker 1996, pp. 305-8, and Penrose 1994, pp. 331-4.

113 As pointed out for example by Albert & Loewer (1990, p. 26).

114 Bell 1987, p. 204.

object is. Additionally, the mechanism creates a change in the wave function where change is needed, but avoids noticeable change nearly everywhere else.

The usually given general demerit of GRW is that it is hopelessly *ad hoc*. No explanation is given of the emergence of the peaked Gaussian. It corresponds to nothing known in nature but is postulated as a fundamental law of nature. Other demerits are that the localization of the wave function gives rise to an additional energy input into the system undergoing the process with the result that energy is not conserved,[115] and that the theory is formulated for non-relativistic quantum mechanics[116]. There are also strong specific criticisms of the theory in a certain respect (see e.g. Albert & Vaidman 1988, Albert & Loewer 1990, and Albert 1992). Without discussing these criticisms in detail, the worry is that GRW collapses almost never produce *definite* outcomes, in the sense of producing a collapse into an eigenstate of position. GRW cannot make a superposed state like that of Schrödinger's cat collapse into one of its component states of dead, alive, and make the other vanish entirely, but only *almost* entirely, i.e. for all practical purposes. But that's fudging. The state being *almost* defined is not the same as the state being defined. (It may be worth mentioning that Albert has since come round and no longer thinks the idea is bad.[117])

As for the charge of *ad hoc*ness, I agree. However, that isn't why I think the GRW approach ought to be rejected. It is rather that the approach hardly touches on the general problem of the standard interpretation from the Einsteinian realist point of view – namely its wave-particle duality and complementarity, arising from quantization. It is the wave aspect of matter that gives rise to the particular problems of the standard interpretation – such as the existence of superposed or 'grotesque' states of matter, the unexplained and essentially probabilistic collapse of the wave function, the mysterious entanglement or nonlocal connection of widely separated quantum systems, the lack of the full complement of classical attributes possessed by a quantum entity, the measurement problem, and the generally intrinsically probabilistic behaviour of quantum systems.

GRW simply presupposes the orthodox conception of state, and addresses itself to the measurement problem attendant on this conception. The interpretation begins by *accepting as givens* all or most of the drawbacks of the standard interpretation from an Einsteinian realist viewpoint – and therein lies the trouble. In GRW, matter is still essentially described as waves (moreover, waves that are now taken to be real). Admittedly, there is no longer any duality, but that's only because *everything* is a wave. As Bell puts it, '[t]here is nothing in this theory but

115 Home 1997, p. 104.

116 Bell, however, discerns in it 'a residue, or at least an analogue, of Lorentz invariance' (1987, pp. 206-7).

117 Albert 2000, p. 149, note 10.

the wavefunction'.[118] The principle of superposition with its interference of possibilities continues to apply to quantum systems, with the difference that now the superposition is one of real waves, and all systems (including macroscopic ones) are quantum systems. Both distant and nearby objects are still generally entangled and represented by a single wave function, owing to the superposition. Such a wave-state instantaneously 'collapses' to the more peaked function even over spacelike distances just as in the standard interpretation, with the difference that, since the waves are now real, serious difficulties also arise in making the theory relativistically covariant.[119] Moreover, the physical collapse of the real wave function is a superluminal process.[120] Such collapse is intrinsically random, since the *ad hoc* precipitating event itself is intrinsically random (it has been put in by hand).

It is true that GRW provides a mechanism for the collapse that requires neither measurement nor observers, and which treats both the (Schrödinger) evolution of the wave function and its collapse as being governed by the same dynamical law. But that is of more importance to the standard theorist than to the Einsteinian realist, since the measurement problem is internal to the standard interpretation (an artefact of the standard interpretation according to the Einsteinian realist). The Einsteinian realist is likely to be underwhelmed by the proposed GRW solution because she believes that the wave function itself is an artefact. It is as if Borges' modernist had noticed a certain lack of internal consistency in his schizoid architectural world picture, and formulated a theory that rendered the picture consistent, but which did nothing to make its architectural designs habitable.

Decoherence (the 'new orthodoxy')

'Decoherence' is another programme of interpretation of quantum mechanics that is squarely in the Copenhagen tradition. It is often associated with the name of Wojciech Zurek, and also that of Roland Omnès. Gell-Mann and Dieter Zeh are other notable proponents. The interpretation starts off from the orthodox conception of state, save that it accepts as a correct and complete representation of the final state of a system only the wave function description of the *combined* state of the system and measuring apparatus – a *pure* state. However, given the action of environmental decoherence, it is practically just about impossible to

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118 Bell 1987, p. 204.

119 It seems that the theory can be made relativistically covariant in a weakened form, called 'stochastic relativistic invariance', because only the *ensemble* of predicted results of experiments is frame-independent. Even this is obtained at the cost of replacing the original Gaussian localization function by some more local entity (a delta function), with the unfortunate consequence that there is infinite energy production by unit volume. (D'Espagnat 1995, pp. 294-5.)

120 As Maudlin reminds (1994, p. 160, n. 11).

distinguish such pure state from a statistical *mixture* of states. This is how the interpretation tries to solve the measurement problem attendant on the orthodox conception of state – to explain why we see what we see – why, for example, Schrödinger's cat is always observed to be either dead, or alive, but never in some grotesque state in which it is a superposition of both. The proposed solution turns on this indistinguishability. Here are brief details.

We saw in §1.2(d) that in the standard interpretation, an arbitrary wave function Ψ for a physical system can be expanded in terms of a complete set of linearly independent, orthonormal eigenfunctions ψ_n of the Schrödinger equation, such that $\Psi = a_1\psi_1 + a_2\psi_2 + \ldots a_n\psi_n + \ldots$, where both the coefficients a_n and the values of the functions ψ_n are generally complex numbers. That means that the general state of the system can be expressed as a coherent linear superposition of (complex-number weighed) macroscopically distinguishable alternative states. (Such a state is called 'coherent' because, if a system is in such a state to start with, Schrödinger evolution will maintain it in that state.) The characteristic quantum-mechanical interference between the component states follows.

We say that there is *decoherence* when the various contributions of these component states are effectively decoupled so that interference can no longer take place. One ends up with the system in a mixed state as opposed to a pure state.

We also saw in §1.3.1 that the measurement problem arises in the standard interpretation when it seeks to describe both the measured system and the measuring apparatus in quantum mechanical terms. The total system is a linear superposition of system + apparatus. Given the eigenvalue-eigenstate link (the assumption that an observable of the system has a value if and only if the state of the system is an eigenstate of the observable, or an eigenstate of the projection operator representing the property – see §§1.2[d],[e],[f], 1.3.1), no measurement ought to have determinate results. The decoherence programme of interpretation tries to solve the problem by bringing in the *environment* an additional factor.

The main idea is that the environment is continuously decoupling the various contributions of the component states of the linear superposition, by interacting with a macroscopic system or object in an uncontrollable way, randomising the phases of its constituent wave functions and so destroying the interference between them, making the system behave like a classical mixture, rather than a system in a pure state which it really is. (By 'environment' is meant not only the system's external environment but also its inner environment, such as the behaviour of its internal variables which don't appear in the quantum description.) In effect, the environment acts as a measuring device, constantly 'watching' the object. For this reason, macroscopic systems generally do not obey Schrödinger's equation, which applies only to closed systems. Additional mathematical terms need to be added to the equation, reflecting the coupling between the system and its environment. Schrödinger's equation does apply, however, to the *combined* state of system and environment.

Given these, and other assumptions, it can be shown that a macroscopic object such as Schrödinger's cat is unable to exist in a macroscopically distinguishable superposed state exhibiting interference effects for much more than 10^{-23} seconds, explaining why we don't see quantum effects on a macroscopic scale.[121] It would seem, at least at first glance, that the combined state of the (system + apparatus) may be described as a mixture, eliminating the measurement problem.[122]

The main claimed merit of decoherence, then, is that it eliminates the artificial division between the object and the observer, which was so troublesome in Bohr's interpretation. In decoherence the degree of quantum behaviour exhibited by an object depends in a natural way directly on its size.

However, the idea behind the interpretation has been severely criticized, e.g. by Bell, Bub, d'Espagnat, Healey, Herbert, Home, Leggett, Penrose.[123] Without going into specifics, the substance of the criticism is that the proposed solution depends essentially on fudging.

The trouble is that the combined state (of system + apparatus + environment) is *not* a mixture, but a superposition. Home writes, 'Despite the fact that decoherence effects are important in accounting for the usual absence of quantum interference effects in the macrodomain, they are irrelevant as far as the measurement paradox is concerned. This is essentially because the interpretative shift from the notion "a pure state of entangled system + apparatus behaves as *if* it were a mixed state" to "a pure state *is actually* a mixed state" entails a major logical nonsequitur.'[124] (Technically, once the systems have interacted, we are obliged to describe them not by individual density matrices but by a combined density matrix for the joint system.[125] Such a density matrix will in general not

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121 Whitaker 1996, p. 290.

122 Omnès writes, 'Mathematically, at least in the main cases, [decoherence] appears as an *almost* complete diagonalization of the reduced density operator in a specific basis. Physically, it entails the disappearance of *most* macroscopic interference, thus providing an answer for the Schrödinger's cat problem.' [My italics.] (Omnès 2000, pp. 206-7.)

123 Healey 1998, p. 97; Bub 1997, pp. 220-3, passim; Home 1997, pp. 83-6; d'Espagnat 1995, pp. 177-85; Penrose 1994, pp. 323-8; Bell 1990; Leggett 1987b; Herbert 1985, pp. 152-5.

124 Home 1997, p. 84.

125 A density matrix (or density operator or statistical operator) is a general and very useful way of describing either a single system, or an ensemble of different systems all possessing the same wave function (i.e. a pure ensemble), or an ensemble of different systems that *don't* all possess the same wave function (i.e. a mixed ensemble or a 'mixture'). See von Neumann 1955, Ch. IV; Belinfante 1975, pp. 2, 123-5; Penrose 1994, pp. 316-21; Whitaker 1996, pp. 286ff; Goswami 1997, pp. 522-5. The usefulness is in the way the density matrix manages to weave in both classical and quantum probabilities

be diagonal.[126])

The suggestion that the discrepancy between pure and mixed states cannot be detected *in practice*, and so doesn't matter, has been likened to children's pragmatic reasoning: it isn't bad to do forbidden things, only to get caught doing them. As Herbert points out, it is true that after phase randomisation by the environment quantum probabilities behave numerically the same as classical probabilities, but that's not to say that randomisation of phase, although necessary for collapse, is *sufficient* to bring it about. Although phase randomisation is evidently *present* in all measurement situations, it does not by itself *constitute* a measurement. It can hardly by itself convert a situation in which, say, a quantum entity takes both paths in a split-beam experiment (quantum ignorance), into one in which it takes only one path (classical ignorance).[127] Healey writes:

> Indeed, both physicists and philosophers have been warning against just this way of misunderstanding the significance of this kind of mixed state for the past twenty years.[128]

Bell somewhat derisively called decoherence a 'FAPP' [for all practical purposes] solution to the measurement problem, writing that 'the obvious interpretation... would be that the system is in a state in which the various [wave functions] somehow co-exist... This is not at all a probability interpretation, in which the different terms are seen not as co-existing, but as alternatives'.[129]

Zurek himself now accepts this, and opts for what he calls an 'existential interpretation' of quantum mechanics, claiming that the practically instantaneous collapse of the wave function pursuant to decoherence occurs in each branch of an Everettian 'relative state' universe (described below).

Fortunately, this is a debate we do not need to enter into here. That's be-

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into a single mathematical description of a system. It can handle an ensemble of many particles. Moreover, the states of physical systems (which of course are made up of many particles) are more often mixed states than pure states. In practice we are often unable to distinguish between the two. The idea then is that the operationally appropriate mathematical description of the system is one that in fact does not distinguish between them.

126 If the (reduced) density matrix describing the ensemble *were* diagonal, that would enable us to speak of a measurement involving a collapse of the wave function having taken place, amounting to a solution of the measurement problem. A pure state (superposed state) would have been reduced to a statistical mixture, the sizes of the diagonal elements giving the probabilities of the various results for the value of the measured system. (See Whitaker 1996, p. 287.) That is to say, as Healey puts it (1998, p. 96), 'each component wave in the mixture corresponds, via the [eigenstate-eigenfunction] link, to a state in which the measured system has some determinate value for the measured quantity and the measuring apparatus has recorded that value'.

127 Herbert 1985, pp. 154-5.

128 Healey 1998, p. 97.

129 Bell 1990, p. 36.

cause my rejection of the approach has a different basis.

It is that the main demerit of decoherence is much the same as that of GRW. We see that (like GRW), the decoherence interpretation starts off by presupposing the orthodox conception of state. In it, matter is still essentially described as waves, and the principle of linear superposition continues to apply to quantum systems, with all the problems attendant on that principle, including the interference of possibilities, EPR entanglement and instantaneous collapse of the wave function even over spacelike distances. Thus, the interpretation accepts as givens all or most of the drawbacks of the standard interpretation (drawbacks at least from an Einsteinian realist viewpoint). It then proceeds to try to address what is, in effect, an *in-house* problem of the standard interpretation, namely the measurement problem. But that enterprise is hardly likely to speak to the theorist who rejects the premise on which the entire project is based – namely the orthodox conception of state.

Consistent Histories

Another strand to the idea of decoherence as a solution to the measurement problem is the notion of 'consistent histories', introduced by Griffiths.[130] The underlying idea is to ask, what is it that is real? In this connection, recall that in the Copenhagen interpretation a measurement generally reveals the value a dynamical quality has *after*, but not *before*, the measurement. Yet, as d'Espagnat points out,[131] Griffiths wants measurement to reveal properties that *already* existed. It turns out that in Griffiths' conception, what is real, or rather, all that *can* be real, are certain histories, or successions of events, collectively named *consistent* histories – those histories that are not actually forbidden by quantum mechanics. Each individual history has a 'weight', which is much like a probability., given by the standard quantum rules. The term 'consistent history' refers to a *family* of individual histories. The family has a consistent history if and only if the weight of the family is equal to the sum of the weights of the individual histories.[132] Think of the two-slit experiment (§1.2[e]). With both slits open, the probability distribution of the ensuing interference pattern is not the sum of those when either slit is open. Hence, in the interpretation, a photon's path through either slit A or slit B when both slits are open is not a possible history. Hence the photon being at a particular position can't be said to be true or false. On the other hand, take a particle travelling through a Stern-Gerlach apparatus. If a detector clicks along one of the two legs of the apparatus (and if a check reveals that the two paths do indeed constitute consistent histories, as they would in this case), then we know, according to the 'histories' interpretation, that just

130 Bub 1997, p. 232.
131 D'Espagnat 1995, p. 232.
132 Whitaker 1996, pp. 293-4.

prior to the click, the particle was actually located on the particular path leading to that detector. In that case, at no time did the particle have a 'grotesque' wave function according to which it was in a superposed state, and classical probability theory was applicable all the time in respect of its position.

Quite generally, the 'consistent histories' idea is to conform to the restrictions of the Copenhagen interpretation regarding what we are permitted to say about quantum systems in various experimental situations, without some of the drawbacks of that interpretation, especially the 'cut' between the observer and what is being observed.[133]

Many difficulties arise. One is the question of what the consistent histories really are. Do they represent *possibilities, one* of which is realized? If so, by what mechanism? Why is there a unique datum at the end of a measurement? This is the measurement problem of the orthodox interpretation. To evade the problem, a further move is usually made to Everett's 'relative state' (or 'many worlds') interpretation, where determinateness is claimed only in some relative sense. But this move, too, is problematic.[134]

Roland Omnès, who introduced decoherence to the idea of consistent histories, is naturally enough troubled by the problem, since that's just the problem which the decoherence programme tacked on to the histories interpretation was supposed to solve. In 1994, in his ninth thesis (of twenty-one by 1997) summarizing his interpretation of quantum mechanics,[135] he tries to bypass it, and even to turn it into a virtue, writing:

> The theory is unable to give an account of the existence of facts, as opposed to their uniqueness to the multiplicity of possible phenomena. This impossibility could mean that quantum mechanics has reached an ultimate limit in the agreement between a mathematical theory and physical reality. It might also be the underlying reason for the probabilistic character of the theory.[136]

That may be. However, a more obvious reason for the impossibility springs to mind. Omnès returns to the impossibility in 1995, characterizing it as the 'last remaining problem of quantum mechanics', and declares the inability of histories to solve it 'a triumph'.[137] By 1999 he is content to describe it as a 'false problem', writing, 'There is no problem of objectification because the relation between a theory and physical reality is no part of a theory... the supposed problem of objectification cannot be stated in a logically significant way. It is not a problem of physics and only a problem of interpretation, which means that the analysis

133 Bub 1997, pp. 232-4.
134 Bub 1997, p. 2.
135 Bub 1997, p. 232.
136 Omnès 1994, p. 507, cited in Bub 1997, p. 232.
137 Omnès 1995, p. 621.

to which one must proceed is not a matter of theoretical physics but rather of semantics: a philosophical exercise, so to speak'.[138] But other proponents of 'histories', such as Halliwell, are not so heroically sanguine, frankly admitting that the idea fails to solve the measurement problem.[139]

D'Espagnat offers a devastating critique of Griffiths' version of the 'histories' conception.[140] His conclusion is that the approach can be said to be inconsistency-free only at the cost of 'basic alterations to our normal way of thinking'.[141] However, we do not need to go into the details here.

My own more modest criticism of the 'histories' approach (and why I think it needs to be rejected) is simply that it seems unnecessarily restrictive for a supposedly realistically motivated theory to start off from the Copenhagen conception of state, i.e. to conform to the restrictions of the Copenhagen interpretation concerning what one is permitted to say about quantum systems in various experimental situations. This is for the reasons already stated in the two preceding examples. To do so seems a little like altering the size of one's foot to fit the size of one's shoe. The possibilities are severely cut down right from the start. (Perhaps this is why Omnès remarks that 'the language of histories... does not pretend... to include every aspect of reality but only what is relevant for understanding it'.[142]) In any case, the restriction has the result that there are many possible histories (in fact, nearly all of them) that are excluded by the theory – because they aren't consistent histories, i.e. they aren't histories conforming to what one is allowed to say according to the Copenhagen interpretation. We have seen, for instance, that a photon's path through either slit A or slit B in the two-slit experiment when both slits are open is not a possible history. But of course there may be some interpretation, perhaps involving backward causation, in which the photon does have a path even in such a case, and the possibility should not be excluded on *a priori* grounds. For instance, in Bohm's causal interpretation a photon does have such a path. Moreover, if the decoherence interpretation is fatally flawed for the reasons already given, then so is the 'histories' interpretation just to the extent that it relies on decoherence.

Relative State (Many Worlds)

Everett's 'Relative State' interpretation of quantum mechanics (RSI) is motivated by the measurement problem, and its point of departure is to take the formalism of quantum mechanics very seriously indeed – which is to say, in this case, at *face value*. It has been knocking around since 1957 in many versions and is well-

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138 Omnès 1999, pp. 243, 256.
139 Whitaker 1996, p. 297.
140 D'Espagnat 1995, pp. 232-46.
141 D'Espagnat 1995, p. 238.
142 Omnès 1999, p. 154.

known, so I shan't need to describe it in any detail here.

Everett set out to show how both the linear Schrödinger evolution of the wave function and the eigenvalue-eigenstate link could be maintained consistently, without needing a collapse postulate.[143] He assumed that the universe as a whole, including the observers in it, is completely described by a 'universal wave function'. He wrote:

> We thus arrive at the following picture. Throughout all of a sequence of observation processes, there is only one physical system representing the observer, yet there is no single unique *state* of the observer (which follows from the representations of interacting systems). Nevertheless, there is a representation in terms of a *superposition*, each element of which contains a definite observer state and a corresponding system state. Thus with each succeeding observation (or interaction), the observer state 'branches' into a number of different states. Each branch represents a different outcome of the measurement and the *corresponding* eigenstate for the object-system state. All branches exist simultaneously in the superposition after any given sequence of observations.[144]

In this picture, the transition from the possible to the actual – the collapse – is taken care of very simply. There is no collapse, nor any need for one. That's because all the elements of a superposition (the 'branches') are actual, and equally real. Moreover, the fact that the branches are orthogonal states in Hilbert space means that no branch can affect another branch. That also implies that no observer will ever be aware of any 'splitting' process.

Everett claims that his solution of the problem of the collapse of the wave function is analogous to the solution provided by relativity to Newton's problem of deciding which among all the different inertial frames is objectively the rest frame. The answer is of course that there is no objective rest frame. Likewise, in the many worlds interpretation no possible history is favoured over any other.[145]

RSI makes the same predictions as the orthodox Copenhagen interpretation.[146] But its proponents claim it goes further, in that it provides an explanation of the probabilistic nature of quantum mechanics, which is left unexplained and irreducible in the orthodox interpretation. On the many worlds ontology, the statistical indeterminacy of quantum theory arises only because the missing information that would have enabled us to make determinate predictions about

143 Bub 1997, pp. 223-4. (The eigenvalue-eigenstate link is the assumption that an observable of the system has a value if and only if the state of the system is an eigenstate of the observable, or an eigenstate of the projection operator representing the property. [Bub 1997, p. 239.])

144 Everett 1957, p. 459.

145 Shimony 1989, p. 392.

146 DeWitt 1970, pp. 164-5.

the future states of quantum systems is 'hidden' in the other worlds to which we have no access.[147] The universe is fully deterministic after all, provided that by 'universe' we mean the entire ensemble of branching worlds. Our impression of indeterminacy of the world on the quantum scale arises only because we can't see the whole picture, our individual consciousnesses being confined to just one world.

A bonus of this interpretation, as Lockwood points out[148], is that one gets a realist interpretation of quantum mechanics without nonlocal interactions.

RSI has been challenged on many grounds, perhaps the most important of which are the following:

(a) People have objected to its ontological profligacy (reminiscent of David Lewis' 'ontological slum' in the philosophy of language).

(b) Although it 'solves' the problem of the collapse of the wave function by rendering the collapse an illusion, it replaces the problem of the collapse by a pair of new problems:

  (i) the problem of explaining how the illusion of collapse comes about; and

  (ii) the problem of accounting for the splitting of the universe into an infinity of 'parallel' universes. The splitting seems to be irreducible, not susceptible to scientific explanation (just as the collapse of the wave function is irreducible in the Copenhagen interpretation of quantum mechanics).

(c) It gives no principled answer as to when the splitting occurs.[149]

(d) It gives no theory to show how a perceiving being would divide up the world into orthogonal states. Penrose for one writes that there's nothing in the many-worlds interpretation to choose between, e.g. the orthogonal states $|L \leftarrow\rangle$ and $|L \rightarrow\rangle$ as opposed to $|L\uparrow\rangle$ and $|L\downarrow\rangle$. Without a theory of how to divide up the world into such alternatives, the many-worlds interpretation gives us no reason to expect that we couldn't be aware of linear superpositions of golf balls or elephants in totally different positions. Mere *orthogonality* doesn't do it.[150] To try to overcome this, proponents postulate a special relationship between the system-apparatus combined wave function and the conscious observer's state of awareness. But this move merely shifts the problem from physics to the more speculative area of theory of mind, as

147 Davies 1982, p. 138.
148 Lockwood 1996, p. 164.
149 Earman 1986, p. 225.
150 Penrose, 1994, p. 312.

Homes points out.[151] Abner Shimony makes the same point, writing that 'in any vector space of dimension greater than one there are infinitely many ways to choose a set of mutually orthogonal vectors, and each way provides a means for expressing an arbitrary vector of the space as a superposition'. We may choose a set of mutually orthogonal vectors each representing a state in which the macroscopic dynamical variable A has an indefinite value. In that case, 'the universe characterized by the same wave function $\Psi(t)$ as before ramifies into branches, but now not in a way that is congenial to our imaginations, for each of the equally real branches exhibits an indefinite value of the macroscopic variable A...', and furthermore each observer reflects the indefiniteness of A. But branches of this kind are alien to our experience. As observers we never see a pointer on a dial somehow suspended between pointing up and pointing down... nor a cat that is neither dead nor alive.[152]

Notice that essentially the same problem also arises in the standard interpretation of quantum mechanics. It is just the measurement problem – the very problem the RSI interpretation set out to solve! Take Schrödinger's cat in a box, where the cat is killed by a device if the detector receives a photon (in state $|\alpha\rangle$) but not otherwise (photon in state $|\beta\rangle$). Suppose that we ask the question, 'is the cat dead or alive?' It is usually claimed that the formalism of quantum mechanics enables us to predict that the cat will be found in one of the two classical states, dead or alive, with equal probability of each when we open the box and make an observation. (That's because in this particular case the two superposed states describing the system are orthogonal; thus there is equal possibility of either one emerging as real.) 'However', Penrose writes,[153] 'merely to know that the density matrix has the form of an equal mixture of these two states certainly does *not* tell us that the cat is either dead or alive (with equal probabilities), since it could just as well be either "dead plus alive" or "dead minus alive" with equal probabilities! The density matrix alone does *not* tell us that these two classically absurd possibilities will never be experienced in the actual world as we know it. As with the "many-worlds" type of approach to an explanation of **R** [the collapse of the state-vector], we seem to be forced, again, into considering what kinds of states a conscious observer... is allowed to perceive. Why, indeed, is a state like "cat dead plus cat alive" not something that a conscious external observer would ever become aware of?'

(e) It may be questioned on general grounds whether the speculative meta-

151 Homes 1997, pp. 93-4.
152 Shimony 1989, p. 393.
153 Penrose 1994, p. 326.

physics of the interpretation provides a genuine answer to a physical problem.[154] In particular, is the interpretation anything more than a semantic model for probability statements associated with the measurement process? The point is that in the last few decades philosophers have given illuminating analyses of many modal concepts in terms of 'possible worlds'. Now, probability too is a modal concept,[155] and it, too, has been described in terms of possible worlds.[156] The suspicion is that, when all is said and done, RSI is little more than this kind of conceptual analysis. This suspicion is strengthened by de Witt's claim that '*the mathematical formalism of quantum theory is capable of yielding its own interpretation*'. (Emphasis in the original.)[157]

(f) And finally, John Bell has expressed his unease about the fact that the reality of all the alternative worlds removes the distinction between potentiality and actuality, a distinction central to all decision making. 'If such a theory were taken seriously it would hardly be possible to take anything else seriously.'[158]

To me, RSI is a good illustration of the mistake of explaining a mystery by appealing to a bigger mystery. It is also a good illustration of the danger of taking the orthodox formalism too seriously. The only reason for maintaining the reality of infinite branches of the universe, and thereby violating the methodological rule of not multiplying entities beyond necessity is, as Shimony observes, 'unwillingness to curtail the range of validity of standard quantum mechanics'[159] – which is something I've already remarked on. It is true that RSI has a certain simplicity, cogency and even a mad appeal if one is dead set on conforming to the restrictions of the Copenhagen interpretation. But this thesis is an extended argument for the view that that's just what we don't need to do.

Modal Interpretation

The measurement problem of the orthodox interpretation arises through its eigenvalue-eigenstate link (the assumption that an observable of the system has a determinate value only in the event that the quantum state of the system is an eigenstate of the observable, or an eigenstate of the projection operator representing the property). As we've seen, the eigenvalue-eigenstate link implies the false result that when the final state of a system is a superposed one of system +

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154 This point is after Hughes 1989, p. 292.

155 Van Fraassen 1980, Ch. 6. He describes probability as 'The New Modality of Science'.

156 See e.g. Bigelow 1976.

157 DeWitt 1970, p. 161.

158 Cited in Shimony 1989, p. 393.

159 Shimony 1989, p. 393.

apparatus, the apparatus records no determinate result upon measurement.

The 'modal' interpretation of quantum mechanics, originally proposed by van Fraassen, tries to deal with the measurement problem by rejecting the assumption, or link. It assumes that the linear dynamical equations of motion of standard quantum mechanics are always 'exactly right', and that there are certain particular properties of physical systems whose values are determinate even in the event that the quantum state of the world fails to be an eigenstate of the operators associated with them.[160] In this way a measurement device can be given both a classical and quantum theoretic description,[161] and the need for a collapse is avoided. One denies 'neither the determinism of the total system evolution nor the indeterminism of outcomes', but says that 'the two are different aspects of the total situation'.[162] The underlying idea is that quantum states, unlike classical states, constrain *possibilities* rather than actualities (hence the 'modal' in its name). More explicitly, the state (which is *within* the scope of quantum mechanics) gives only the probabilities for the actual occurrence of events (which occurrence is *outside* the scope of quantum mechanics). In other words, the state merely *delimits* what can and cannot occur – it delimits possibility, impossibility and probability of occurrence – but it doesn't say what *actually* occurs. 'The transition from the possible to the actual is not a transition *of* state, but a transition *described* by the state.'[163] Here is van Fraassen's succinct description of the interpretation:

> The modal interpretation can be summed up in part by saying that in salient respects it is *as if* the Projection Postulate, and the ignorance interpretation of mixtures, were true. To attribute a (dynamic) state is to assert a statistical hypothesis – that is, to assert a related cluster of probability judgements. Those probabilities must be probabilities of *something*; contrary to von Neumann, we take that *something* to be not states but events, and take an event to consist in some observable having some value.[164]

In some more recent versions by other authors (Bub 1997, Kochen 1985, Dieks 1989, Healey 1989), the converse assumption, the eigenstate-eigenvalue link, is also rejected, in different ways. The idea is that precise values are ascribed to particular quantities in circumstances other than those obtaining at the end of a measurement interaction, thereby rejecting the orthodox interpretation principle entirely, ending up in that regard much like Bohmian Mechanics which also rejects both assumptions.[165]

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160 Albert 2000, pp. 146-7.
161 Hughes 1989, p. 315.
162 Van Fraassen 1991, p. 273.
163 Van Fraassen 1991, p. 279.
164 Van Fraassen 1991, p. 327.
165 For details, see Bub 1997, *passim;* Healey 1998, pp. 100-3.

All the versions have the common feature that an observable can have a determinate value even if the quantum state is not an eigenstate of the observable. By this means they preserve the linear, unitary dynamics for quantum states without needing the projection postulate to explain the determinateness of pointer readings and measured observable values in quantum measurement processes.[166]

Without going into details, many of the versions, including van Fraassen's, are contextual.[167] All versions appear to be nonlocal and essentially non-relativistic.[168] It is always possible, even if unlikely, that something can be done about the latter. However, it is the former – the nonlocality – that renders this type of interpretation unacceptable in my opinion. I'll have more to say about nonlocality in my discussion of Bohmian Mechanics below, which is of course also expressly nonlocal, as well as non-relativistic.

Bohmian Mechanics

Bohm's 'hidden variable' or 'causal' interpretation of quantum mechanics (or 'Bohmian Mechanics' as it has also come to be known), is yet another theory that takes the wave function very seriously, but its motivation is realist rather than Copenhagen. It has been around even longer than Everett's Relative States interpretation: ever since 1952 in fact, and is no less well-known, so I shan't need to describe the idea in any detail. However, a brief description seems in order. (For a more detailed description, see Albert 1992, Chapter 7.)

The key idea for Bohm was de Broglie's notion of a pilot wave that tells a particle how to move (§1.1). In Bohm's theory, the wave (or field) is called the *quantum potential*. It is obtained by splitting the Schrödinger equation into two equations. One of the two ensures that total probability is conserved. The other has the structure of the Hamilton-Jacobi equation, but with the difference that, in addition to the classical potential energy, it contains an extra term, namely the quantum potential,[169] given by

166 Bub 1997, p. 178.

167 Recall that a *contextual* hidden variable theory is one in which the truth value of each of the quantum-mechanically recognized eventualities is not determined by the complete state φ, but the outcome of each experimental test of an eventuality is determined by φ together with some features of the experimental arrangement. In contrast, a *non-contextual* hidden variable theory is simply one in which the complete state assigns a definite truth value to each of the quantum-mechanically recognized eventualities, i.e. to each one that corresponds to a projection operator. (After Isham 1989, p. 382.)

168 D'Espagnat 1995, p. 303; Bub 1997, p. 244.

169 After Whitaker 1996, pp. 248-9.

$$Q = -\frac{\hbar^2}{2m}\frac{\nabla^2|\Psi|^2}{|\Psi|^2},$$

where Ψ is the quantum field derived from Schrödinger's equation and m is the mass of the particle in question.[170] Bohm emphasizes that the 'primary and fundamental meaning of Ψ is that it is a field, which determines the quantum potential', through the relationship expressed by the above equation.[171]

The quantum potential has the job of 'reading' the environment and 'reporting back' to the particle, as it were. It carries 'information' and is *potentially* active everywhere. (Note that in Bohm's conception, this potential [i.e. both its imaginary and real components] is not just a mathematical object, but is just as real and objective as the fields of Maxwell's theory, propagating as a guiding field in the configuration space of the particles according to Schrödinger's equations of motion.) The potential is *actually* active, however, only when and where this energy enters the particle.

According to Bohm, this implies that 'an electron, or any other elementary particle, has a complex and subtle inner structure (e.g., at least comparable to that of a radio)'.[172]

The ontology of the theory is straightforward enough, at least at first glance.[173] There are particles.[174] These particles are much like classical particles, in that they have definite positions and trajectories. In addition to the particles, there is a real physical object described by the wave function, which tells the particles how to move – but which is itself not made up of particles. As Bohm explains, even though an electron *is* a particle, it is 'always accompanied by its fields, which are essential to understand what it is'.[175] In this way, everything in the quantum world is *both* particle and wave, the two interconnected aspects existing simultaneously, rather than being complementaries as in the Copenhagen

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170 Bohm & Peat 1987, p. 89.

171 Bohm 1987, p. 87.

172 Bohm 1987, p. 89.

173 Albert enthuses, 'This is the kind of theory whereby you can tell an absolutely low-brow story about the world, the kind of story (that is) that's about *the motions of material bodies*, the kind of story that contains nothing cryptic and nothing metaphysically novel and nothing ambiguous and nothing inexplicit and nothing evasive and nothing unintelligible and nothing inexact and nothing subtle and in which no questions ever fail to make sense and in which no questions ever fail to have answers and in which no two physical properties of anything are ever "incompatible" with one another and in which the whole universe always evolves *deterministically* and which recounts the unfolding of a perverse and gigantic conspiracy to make the world *appear* to be *quantum mechanical*.' (Albert 1992, p. 169.)

174 This is in Bohmian particle theory, which is the one we are talking about. In Bohmian field theory, fields always have definite values. (Maudlin 1994, p. 117.)

175 Bohm 1987, p. 86.

interpretation. Thus, to the question 'wave or particle?', Bohm's answer is 'wave *and* particle'.[176]

The dynamics of the theory is deterministic: the system of particle plus its set of fields is causally determined (hence the name 'causal interpretation'). However, Bohm writes that the quantum potential 'depends on the "quantum state" of the whole system in a way that cannot be defined simply as a pre-assigned interaction between all the particles.' According to Bohm, *'this is the most fundamentally new ontological feature implied by the quantum theory'*.[177]

Bohm & Hiley characterize the theory in terms of Bell's 'beables', these here being the overall wave function and the coordinates of the particles. They write:

> This theory is formulated basically in terms of what Bell has called 'beables' rather than of 'observables'. These beables are assumed to have a reality that is independent of being observed or known in any other way. The observables therefore do not have a fundamental significance in our theory but rather are treated as statistical functions of the beables that are involved in what is currently called a measurement.[178]

In addition to the theory's contextuality and formulation in terms of 'beables', there are several noteworthy aspects to it. Two, in particular, stand out.

First, there is no recourse to measurements or observers. The measurement problem is solved by rejecting the eigenvalue-eigenstate link,[179] and by making the assumption that all measurement results are readings in position space. 'A definite outcome in an individual measurement is determined by relevant ontological position variables, which have well-defined values at all instants.'[180] Bohm himself writes in that regard, 'It is important to emphasize that we have in this way treated the measurement process as a single whole, without any break or "cut" between classical and quantum mechanical levels.'[181]

Second, even though the theory is deterministic, the usual quantum-mechanical probabilities arise in it because the theory presupposes an uncertainty about the exact initial positions of the particles, which is assumed to be proportional to $|\Psi|^2$.[182] *Given* such initial uncertainty, the dynamics of the

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176 Bell 1987, p.112.

177 Bohm 1987, p. 93.

178 Bohm & Hiley 1993, p. 41. The 'beables' of a theory are those elements which might correspond to elements of reality – to things which exist – quite independently of observation. Observation and observers themselves must be made out of beables. (Bell 1987, p. 174.)

179 Namely the assumption that an observable of the system has a value if and only if the state of the system is an eigenstate of the observable, or an eigenstate of the projection operator representing the property.

180 Home 1997, p. 94. See also Bub 1997, p. 193.

181 Bohm 1987, p. 99.

182 Maudlin 1994, p. 119.

theory will continue to maintain it. Thus, the precise value of an individual particle corresponding to a given wave function is essentially unknowable or uncontrollable.[183] For this reason, the theory does not enable the prediction of a unique trajectory. It does enable a unique *retrodiction* of trajectory, though.[184] Therefore, we can know more about the history of a quantum mechanical system than we can in principle predict about its future.

The theory has a rich complement of problems. Some of the better-known ones are:

(a) Contextuality, arising from the coupling of the ontological velocity or trajectory of particles with the associated Ψ-field. The post-measurement value of, say, momentum is generally different from its pre-measurement value owing to an entanglement of the wave function of the measured particle with the measuring-apparatus state. One would expect that in a hidden-variable deterministic theory, measurement of an observable would yield the same value independently of the context. But this isn't the case in Bohm's theory.[185] Thus, despite its determinism and reliance on hidden variables, the theory nonetheless illustrates a kind of 'Bohrian' quantum wholeness, because, as Hooker puts it, the quantum potential 'must be re-specified for each different quantum mechanical representation of the measured system (for each different measuring situation, as Bohr would say)'.[186]

(b) Express nonlocality. This arises from the fact that Ψ appears both in the numerator and denominator of the equation above. Consequently Q is unchanged when it is multiplied by an arbitrary constant. That means that Q is independent of the intensity of the quantum field, not diminishing with distance, but depending only on its form.[187] So even very distant features of the environment can affect the movement of a particle in a major way, for example affecting the trajectories of particles.[188] This *express* nonlocality is ironic, since arguably, the lesson of EPR/Bell and Wheeler's 'delayed choice' experiments (indeed of all specifically quantum-mechanical experiments) is that nonlocality lies at the heart of the interpretative problem of quantum mechanics. (I defend this claim in §5.1.2.) Einstein, for one, found nonlocality (or non-separability) the most objectionable feature of the Copenhagen interpretation – even in a relatively benign *non*-express form.

(c) The theory entails superluminal signalling, and for that reason is obviously non-relativistic in a more serious way than the relativistic generalizations

183 Bohm 1987, p. 91; Maudlin 1994, p. 119.
184 Aharonov & Albert 1987, p. 225; Home 1997, p. 44.
185 See d'Espagnat 1995, pp. 289-92; Home 1997, pp. 44, 50.
186 Hooker 1989, p. 237.
187 Bohm & Peat 1987, p. 89.
188 See e.g. Home 1997, p. 50; Maudlin 1994, p. 135; Whitaker 1996, p. 249.

of conventional quantum mechanics. The superluminal signalling comes about because the theory is not only deterministic but also 'strongly objective', in the sense that every material particle in the world has a perfectly determinate position at all times, and their motions are completely deterministic,[189] and it is known (Bell's theorem) that no strongly objective theory can reproduce all of the predictions of quantum mechanics without superluminal transmission of signals, at least in the absence of backward causation.[190]

(d) The theory picks out a preferred reference frame. The results of measurements of a spacelike separated pair of particles are determined not only by the states of the particles and the detector-settings, but also by which measurement occurs first in the preferred frame.[191] This is also a problem for the standard interpretation, albeit in a less severe form. See §4.6.

(e) There is the theory's peculiar notion of 'state', which is connected with the above-mentioned features of the theory. Even though the theory is 'strongly objective', a distinction is made in it between the *full* objective state and the *empirical* objective state, this distinction being closely reflected in Bohm's well-known distinction between 'implicit' and 'explicit' orders. I shall not go into details here.[192] A closely related problem is that the theory contains empty waves, or 'inactive wave packets', as Bohm puts it,[193] which can be discarded 'for all practical purposes', but which can still re-emerge at some later time to causally alter particle trajectories.[194] Clearly, the notion of 'objective' is far from easy to characterize unambiguously in the context of quantum-mechanics.

(f) Last but not least, Bohmian mechanics makes no different testable pre-

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[189] This definition of objectivity is complicated, however, by a related issue: does the objective theory ('objective' in the above way) permit the description of phenomena independently of the specifics of the experimental conditions of observation? This latter was Einstein's criterion of 'objective reality'.

[190] See d'Espagnat 1995, p. 279. It needs to be added, though, that although the configuration space trajectory of a Bohmian particle isn't Lorentz-invariant, the *statistics* of measurement outcomes will nonetheless be the same in every Lorentz frame. (Bub 1997, p. 242.)

Maudlin (1994, p. 121) tries to make light of the non-Lorentz invariance of Bohm's theory by arguing that since nonlocality is inevitable in *any* theory which recovers the predictions of quantum theory, it does not reflect adversely on Bohm's theory to have the nonlocality in plain sight. This is to ignore the distinction made above between strong and weak objectivism. Nonlocality is a poor copesmate to determinism *cum* strong objectivism, as the essentially non-relativistic character of Bohmian mechanics clearly shows. Standard quantum theory can easily be cast into relativistic form just because it is only weakly objective. One needs to do more than just look at isolated aspects of a theory.

[191] See Maudlin 1994, pp. 123, n. 12, 212-15.

[192] For details, see d'Espagnat 1995, pp. 282-3.

[193] Bohm 1987, p. 95.

[194] See e.g. Bohm 1987, pp. 95-7; Bohm & Peat 1987, p. 94; Selleri in Barut *et al.* 1984, pp. 217, 221ff; Home 1997, p. 44; Maudlin 1994, p. 120.

dictions from standard quantum theory. It is well-known, as d'Espagnat points out, that the formalism of the theory is more complicated than that of standard quantum theory, and the theory itself is less flexible and adaptive to new problems. However, even in the areas to which it is applicable, nobody has been able to derive new testable predictions from it by way of compensation.[195]

This is quite a list. Many of the problems identified with the other interpretations already discussed remain, and new ones are thrown up. We see that the theory is fundamentally dualistic, like the standard interpretation, but without the latter's complementarity. Waves and particles are in it on a more or less equal, and *objectively real*, footing, which is supposed to be a strength. But that is precisely why the theory fails to deliver on separability – which was at the heart of the dispute between Einstein and Bohr. That is also the reason for the theory's non-Lorentz invariance. Every gain in some particular respect is offset or more than offset by a loss in another respect.

Some try to make light of these problems by pointing to the objective realism of the theory, and the fact that it manages to overcome the measurement problem.[196] But we've already seen that the latter is only an in-house problem of the standard interpretation. As for the former, the objective realism offered by the theory is of a particularly unsatisfying kind, with its contextuality, strong nonlocality and attendant relativistic non-covariance.

Einstein wasn't impressed.[197] Nor was Bohm overly impressed himself, writing, 'I saw clearly at the time [1952] that the causal interpretation was not entirely satisfactory.'[198]

Above, we have looked at six different interpretations of quantum mechanics, and found that none provides a genuine advance on Bohr's interpretation, all being retrograde steps in one way or another having regard to the nature of the interpretative problem of quantum mechanics (described above), which is ultimately to do with the *non-separability* or *wholeness* exhibited by interacting quantum systems. The predicament in which we find ourselves is similar to that of C.A. Hooker in 1989, after he had reviewed Bell's argument and certain unsatisfactory alternative approaches to Bohr's interpretation of quantum theory, such as Fine's 'prism' proposal[199] and quantum logic. Hooker noted that Bohr had managed to connect the occurrence of Planck's constant with the 'indissoluble wholeness of composite quantum systems' and 'the physical nature of measure-

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195 D'Espagnat 1995, p. 279. For some related remarks, see also Home 1997, p. 50, and Albert 1992, p. 176, note 18.

196 We've seen in §1.4 that Maudlin does so. Maudlin, in turn, appears to have taken his cue from Bell (1987, p. 115).

197 Whitaker 1996, p. 252.

198 Whitaker 1996, p. 255.

199 In Fine 1986, Ch. 4.

ment', and to tie in all three with 'an account of human conceptual capacities so as to form a tightly coherent story of the development of physics'. He added that 'even the most radical alternative approaches do not succeed in going beyond these features, and often prove less methodologically or conceptually unsatisfactory' (which is just what we've found).[200] In particular, none of the alternative responses reviewed by him

> provides any satisfactory physical understanding of the peculiar wholeness exhibited by either EPR-type systems or by measuring instrument/measured system situations, or indeed by interacting quantum mechanical systems generally – the very wholeness pointed to by both Bohr and Einstein as the essential distinguishing feature to be addressed.[201]

Hooker concluded that the painstaking formal analysis of Bell's arguments by numerous investigators since Bell have not advanced our *physical* understanding beyond that already clearly at issue in the original discussion by Einstein and Bohr.[202] In view of the unexplained wholeness of interacting quantum systems, Hooker speculated that perhaps Bohr has said all that there is to be said, and maybe we ought to simply stick to his interpretation. But that position, too, he finds 'disappointing', since from Einstein's point of view the difficulty with it is twofold:

> First, it does not lead to any detailed physical account of what is happening in quantum processes, but rather places an absolute barrier to the conceptualisation of such accounts at a level which is beyond, and even prior to, the actual doing of empirical science. In this way it generates a certain kind of unrevisable dogmatism. And for precisely this reason it fails to provide any deep guidance as to how to incorporate new features into physical theory, e.g. the discovery of further quantum 'particles', the reconciliation of the physics of gravity, electromagnetism and the strong and weak nuclear forces and so on. Second,... the mere appeal to the exchange of quanta cannot itself explain even why the measurement of one observable should randomise the outcomes for observables corresponding to non-commuting operators, let alone explain the particular quantum mechanical conditional probabilities for the probabilities of finding the measurement values of other observables... This then leaves no grounds for a smug Boorean [sic] dogmatism, but rather suggests that there is some deeper physical, and perhaps methodological and conceptual, insight which as yet eludes our grasp.[203]

We now move on to investigate a novel possibility for the gaining of such insight.

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200 Hooker 1989, p. 243.
201 Hooker 1989, p. 244.
202 Hooker 1989, p. 241.
203 Hooker 1989, p. 243.

3.6 'How can quantum mechanics be like that?' A proposal of a very different kind

h is a measure of our ignorance of the future

(Dennis Sciama, 1958)

Back in 1958 already, Dennis Sciama remarked on the surprising lack of consensus on how quantum mechanics should be interpreted, expressing his opinion that violent controversy about a scientific problem is a sign that we're missing some simple but essential consideration. The polemic tries to fill in for the missing point, but of course it cannot succeed in doing so. In his paper Sciama puts forward a simple and elegant proposal as to what that missing consideration might be.[204]

He first assumes that the world is really deterministic ('that we can calculate the state of a system at time *t* if we know enough boundary conditions referring to times other than *t*.'). However, the world that Sciama postulates also differs from classical mechanics in being so constructed that half the necessary boundary conditions for arbitrarily accurate predictions must refer to the past and half to the future of the moment *t*. (Sciama calls such a system a 'mixed system'.)

How would such a system appear to observers like ourselves who are only acquainted with the past? If the observer tries to calculate the state of the system at a time *t* in his/her future, he will find that he cannot do so because he does not know all the necessary boundary conditions. To the observer the system will appear to contain certain indeterminate elements. What sort of a theory will the observer devise to account for his observations? A theory making use of a probability calculus. In effect, he will be forced to average over all those future boundary conditions that are compatible with his present knowledge (though he may not know that this is what he is doing). Sciama's suggestion is that *we* are just such observers, and that this probability calculus is nothing other than quantum mechanics!

> In this way the correctness of quantum mechanics can be reconciled with a deterministic universe. In the language of von Neumann, there are hidden variables; they escape his ban [i.e. von Neumann's proof of the impossibility of hidden variables of the kind sought by Einstein] because they refer to the future.[205]

A consequence of the view presented above is that quantum mechanical probability is only subjective, arising from our ignorance of some of the determining

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204 Sciama 1958.

205 Sciama 1958, p. 78.

conditions (one half of them, i.e. all of the *future* boundary conditions). Thus the quantum mechanical formalism itself must be a consequence of some more basic theory. What might be the details of such theory? Sciama doesn't know, but he makes an interesting observation in connection with that question. Planck's constant h is a measure of the amount of our ignorance of some of the determining conditions. Thus, h is a measure of our ignorance of the future. At the present the numerical value of this constant is arbitrary. However, if quantum mechanics is deducible from a more basic theory, then presumably h will be expressed in such a theory in terms of quantities fundamental to the basic theory. Such a relationship could be tested experimentally, thereby subjecting the theory to test.

It is hard to imagine a better brief introduction to Price's atemporal proposal regarding the meaning of quantum theory. Price's proposal *is* Sciama's proposal writ large.

Summary

In this chapter the EPR gedankenexperiment and its assumptions are described in some detail, as is Bohr's response. The ensuing debate serves well to show the exact nature of the interpretational problem, highlighting the unexpected features of the problem that any interpretation must take into account.

The debate also prepares the ground for the introduction of Bell's impossibility proof, a mathematical theorem which appeared to unambiguously resolve the Einstein-Bohr debate in favour of Bohr. The proof is described. The 'impossibility' referred to the impossibility of the existence of local hidden variables of the kind Einstein sought. Bell's proof seemed to provide at long last the means of resolving the dispute between Einstein & Bohr. His proof appeared to vindicate Bohr by showing that local causality had to be abandoned. Our interest in Bell's proof is not just academic. It is rather in seeing how Price's advanced action proposal, with its local causality, fares vis-à-vis Bell's prohibition of local causality.

This chapter has also critically reviewed six prominent current attempts at interpreting quantum mechanics, namely GRW, decoherence, consistent histories, modal, many worlds, and Bohmian causal. It has been argued that all six, whether in the spirit of Bohr or Einstein, are fatally flawed. The reason is to do with the nature of the solution (or rather, *non*-solution) offered by each to the interpretative problem, having regard to the nature of the interpretative problem itself.

The chapter concludes with an account of a very suggestive idea by Sciama.

We now go on to look at Price's proposal.

4

A Neglected Route to Realism About Quantum Mechanics: the 'View from Nowhen'

[T]he most promising and well-motivated approach to the peculiar puzzles of quantum mechanics has been almost entirely neglected... because the nature and significance of our causal intuitions have not been properly understood.

(Huw Price, 1996)

In the preceding chapter we saw that Bell's proof appears to demonstrate that no local realist theory can reproduce all the predictions of quantum mechanics. As Bell himself put it, 'the reasonable thing just doesn't work'. Is there a way out? Well, as with any so-called 'proof' in physics, it is necessary to look at its assumptions. That is just what Price does, first in a 1994 paper of the above title ('A neglected route to realism about quantum mechanics'), and then in his 1996 book, *Time's Arrow and Archimedes' Point*. In this chapter, we examine the philosophical core of Price's proposal. (In the next chapter we shall go on to see just how it might connect with the formalism of quantum mechanics.) Our purpose in the present investigation remains the same as that stated at the beginning of the first chapter. We are looking for some common factor connecting the problem of the direction of time with that of the interpretation of quantum mechanics.

The philosophical core of Price's proposal has three elements: (a) Price's analysis of the role of the independence assumption in microphysics; (b) his analysis of backward causation in terms of a weaker convention for assessing counterfactuals than the usual one; and (c) his variant of the 'agency' theory of causation. In each element, Price questions a deep-rooted intuition arising from our temporal and anthropocentric perspective, in line with his belief that we ought to think about time's puzzles from a vantage point that is 'outside' time to avoid being misled by the temporal asymmetries of our own natures and habits of thinking.

The chapter begins by focusing on the independence assumption of physics (known as the 'molecular chaos' assumption in statistical mechanics). It is a very general assumption about initial conditions. It says, in effect, that the properties of interacting systems are independent *before* they interact, though not afterwards owing to the interaction, which is why we may expect correlations between separated systems after they have interacted but not before. The independence assumption is thus an explicitly time asymmetric assumption. It is an expression of a powerful intuition arising out of our lack of experience of *macroscopic* pre-interactive correlations. However, as Price points out, there is no evidence of the lack of such correlations on the *microscale*, and we have no experiential warrant for extending the principle there. The evidence is rather *for* such correlations. Accordingly, Price proposes relaxing the independence assumption on the microscale in the direction of the future. The effect is to provide a loophole for the kind of correlations between separated systems predicted by Bell's theorem and subsequently experimentally observed.

The viability of the proposal to relax the micro-independence assumption in the direction of the future depends on being able to avoid causal paradox. In the second element, Price points to a loophole first proposed by the philosopher Michael Dummett, which showed that even if later events do causally affect earlier events, paradox is avoided if it is impossible to find out, *before* the occurrence of the later cause, whether or not the claimed earlier effect has occurred. That is, to avoid paradox, we need to subscribe to the convention for assessing counterfactual dependency that says, 'hold fixed only that portion of the past which is accessible in principle', rather than the stronger mode which says, 'hold fixed the entire past'. Dummett's loophole finds a natural home in quantum mechanics. It turns out that the backward causation required to make sense of the EPR-Bell type of correlations on the advanced action hypothesis is a special case of the weaker, 'hold fixed the *accessible* past' kind of backward causation. Price shows in the case of a photon passing through a pair of rotated polarizers how evidence (or rather *lack* of evidence) of its state when in-between the polarizers is consistent with it being correlated with not only the *past* polarizer but also the future polarizer it is yet to encounter, as would need to be the case for symmetry, and consistent with backward causation playing a part in determining its state. According to Price, such a view not only avoids causal paradox and restores symmetry; it also opens the way for a much more classical picture of quantum mechanics than has been thought possible.

The third element is Price's perspectival variant of the Humean conventionalist theory of causation. Its philosophical interest is that it manages to combine both conventionalism and objectivity in a natural and convincing way, permitting an objective content to the claim that there is not only forward but also backward causation. Here are the details.

4.1 A Faustian choice

Price begins his paper, 'A neglected route to realism about quantum mechanics', by remarking that 'the most profound conceptual difficulties of quantum mechanics stem from the work of J.S. Bell in the mid-1960s'. 'In effect', says Price, 'Bell is telling us that 'Nature has offered us a metaphysical choice of almost Faustian character. We may choose to enjoy the metaphysical good life in quantum mechanics, keeping locality, realism and special relativity – but only so long as we are prepared to surrender our belief in free will!' In other words, there *is* a way out. If everything is predetermined – including not only the spins of particles but also the experimenter's 'decision' to select one measurement axis in preference to others (even if this is made on the basis of flipping coins or apparently random radioactive decays) – then the mystery of how the EPR particles manage to have their spins correlated in accordance with the quantum rule disappears, and Bell's proof is evaded. In that case neither particle needs to 'know' what the spin of the other is going to be at the other's detector so as to be able to arrange its own spin accordingly, and no faster-than-light influences are needed. *Everything* has already been predetermined at the birth of the universe (or at any rate, at some earlier state of the system known as the *common cause*), and the deterministic equations of motion of each spin vector and experimenter simply carry them to their predestined fates.

Recall that the *absence* of such 'cosmic conspiracy' or 'superdeterminism' was one of the three elements of Bell's assumption of Einsteinian locality (that particular element being known as the *independence assumption*).[1] But if that assumption is false and superdeterminism true, then free will appears to be a myth, since in that case all our decisions are predetermined. Bell himself at times contemplated giving up the independence assumption, but considered it too costly an option.[2]

It seems clear that our theoretical picture of the microworld depends crucially on how we regard the independence assumption.[3] We may opt to retain the independence assumption (and thus our belief in free will) but in that case we

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1 Described in brief in 'Bell's assumptions' in the last chapter. In superdeterminism, both the experimenter's choice of measurement setting and the particle's spin are already predetermined in the *past* in such a way that the two will be correlated no matter which setting the experimenter selects.

2 See Bell 1987, p. 154, and for further remarks, pp. 100-3, 154. For a discussion, see Price 1996, pp. 236-8.

3 The independence assumption is known in statistical mechanics as the *molecular chaos* assumption. Consider the molecules of gas in a sealed jar. The molecular chaos assumption states that before the molecules collide, they move *independently* of one another, i.e. to all intents and purposes *randomly*. Since they move independently, we don't expect them to form structure spontaneously within the jar, e.g. gather in one corner. If they did, entropy would spontaneously decrease.

must be prepared to give up our belief in local Einsteinian realism. On the other hand, as Price emphasizes, *without* the independence assumption there seems to be no reason to think that quantum mechanics commits us to nonlocality, Bell's theorem notwithstanding.[4] Suppose we decide to give it up, or at least to relax it. Price has pointed out that there are in fact *two* ways of relaxing the independence assumption. One way is the obvious one of relaxing it in the direction of the *past*, as contemplated by Bell. The other way (which is rather less obvious) is to relax it in the direction of the *future* – something that Bell seems to have been aware of, but which he apparently never clearly distinguished from the former.[5] If we choose the latter, the way is open, according to Price, to enjoy the (metaphysical) advantages of locality, Einsteinian realism *and* free will. Another advantage is the preservation of symmetry in our theory. The trick is to interpret the same formal possibility (giving up the independence assumption) in terms of backward causation, technically known as *advanced action*.

Price's proposal, then, is to modify our usual causal intuitions by admitting advanced action. But are we justified in modifying them? To what extent are they required by physical theory? How reasonable is the proposal to relax the independence assumption in the direction of the future? Seeing that so much hangs on the independence assumption, it may be as well to take a closer look at the assumption itself, before considering the merits of relaxing it one way or the other. We shall now proceed do so. In the rest of this chapter I shall be largely 'interpreting' Price – saying what he means, and what his proposal entails.

4.2 The independence assumption

In chapters 2, 3 and 4 of his book *Time's Arrow and Archimedes' Point*, Price considers the three main arrows of time in physics, namely the thermodynamic, radiative and cosmological arrows. In each case he argues persuasively that the project of trying to show how the arrow in question arises has been riddled with some very persistent mistakes, the most basic of which is what he calls the *double standard fallacy*. Models relying in an essential way on double standards have been able to creep to the centre-stage of contemporary theoretical physics, virtually unnoticed and unchallenged because they arise in a very natural way from our ordinary causal intuitions – which are powerfully anthropocentric. In particular, the asymmetry of causation is anthropocentric in origin. Price argues (Chapter 9) that these very same intuitions are also in large part responsible for the present impasse in the interpretation of quantum theory. Had the nature and significance of our causal intuitions been properly understood (together with the

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4 Price 1996, p. 125.

5 Price writes (1996, p. 233), 'Bell himself seems to have been aware of both versions, and to have regarded both as incompatible with free will, but it is doubtful whether he saw them as clearly distinct.'

real lessons of the nineteenth-century debate about time-asymmetry, Price adds),[6] the most promising and natural approach to the puzzles of quantum theory would not likely have remained overlooked. In chapters 5-7 Price turns to the task of explaining how such a sorry situation could have arisen and persisted for as long as it has.

The culprit, according to Price, turns out to be a deeply rooted and very pervasive temporally asymmetric assumption of contemporary physics. It is intimately connected with our intuitions about the nature of causation. The assumption seems an obvious one – indeed, self-evident on the macroscopic scale, borne out by our everyday experience. From the macroscale it is extrapolated as a matter of course to the microscale, where, however, we have no observational evidence for its applicability, and where, furthermore, it seems to conflict with the underlying time-symmetry of the laws of physics.

The assumption is that events depend on earlier events in a way that they do not on later events. In everyday speech, we say that earlier events *cause* later events; we generally do not say that later events cause earlier events. In physics, the same idea is expressed by saying that the properties of interacting physical systems are independent *before* they interact, though not *afterwards*, since interaction can obviously give rise to correlations between particles of the system, for example in their velocities.

> It seems intuitively obvious that interacting systems are bound to be ignorant of one another until the interaction actually occurs; at which point each system may be expected to 'learn' something about the other.[7]

This is a general assumption about initial conditions. Price calls this assumption the *principle of the independence of incoming influences* ('PI^3'). The principle is explicitly time-asymmetric of course, since according to it only systems that have *already* interacted (i.e. in the observer's past) can be regarded as correlated owing to their interaction, whereas systems which are as *yet* to interact (i.e. where the interaction lies in the observer's future) cannot be regarded as correlated *now* owing to the results of such future interaction. Consider a photon passing through a polarizer. According to the usual interpretation of quantum mechanics, the photon's spin-state *after* its interaction with the polarizer reflects the orientation of the polarizer. *Before* any interaction, though, the two are quite unconnected. In physics, including quantum physics, we expect post-interactive correlations, but not pre-interactive correlations. (What could be a more natural and obvious assumption to make?)

When the general independence assumption PI^3 is extrapolated to the mi-

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6 Namely, the impossibility of deriving an asymmetric result from a symmetric argument. For a discussion, refer to the Introduction to the present work, pp. 5-11.

7 Price 1996, p. 120.

croscale, as in the photon-polarizer case above, Price calls it 'μInnocence', that being a kind of sub-variety of the broader principle PI³. (I shall usually call both varieties simply the 'independence assumption', distinguishing the two as 'macro'/'micro'-independence assumption if the context requires.)[8]

Boltzmann's *H*-theorem is a well-known example of the application of the independence assumption in molecular physics. The *H*-theorem provides a generalized notion of entropy, and is taken to show that entropy always increases with time. It considers the effects of collisions on the distribution of molecules of a gas. The crucial assumption in that theorem is that the molecules of a gas move essentially *independently* of one another, i.e. *randomly, before* they collide. In other words, they lack *pre-interactive* correlations of a relevant kind, even though they may exhibit post-interactive correlations as a result of their collisions. Because they move independently before they collide, we don't expect them to spontaneously form new structure when they meet. If they did, the entropy of the system would spontaneously decrease. Instead, owing to the randomising effect of the collisions the entropy increases until the system reaches thermodynamic equilibrium. At that point all pre-existing order (in the sense of organization) in the system has been destroyed. In physics, the independence assumption is also known as the *molecular chaos* assumption.

There is an important difference in the correlations associated with the macroscopic and microscopic versions of the independence assumption. Consider the former. Macroscopic correlations arising out of past interactions generally lead to a state of higher entropy (increased disorder) in the world, as in the above example. (When viewed in backward time, they exhibit countless entropy-reducing correlations [spontaneous increase in order], which is just why a film run in reverse looks so weird.) An asymmetry thus exists between the past and the future of the world. In this sense, the *macro*-independence assumption is 'observable', though only indirectly.

Things are different in a significant way on the microlevel. Take a gas in a sealed container in a state of thermodynamic equilibrium. In this case, too, *individual* molecules show correlations after interaction, and for that reason we do not expect them to be independent of one another after they have interacted even though we do expect them to be independent of one another before they have interacted (just as with macroscopic systems). Yet the molecules of the gas *in the aggregate* do not show entropy-increasing/decreasing correlations in *either* temporal direction (save for short-lived fluctuations). That shows, says Price, that the correlations associated with the macroscopic and microscopic versions of the

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8 In the philosophical literature, in contrast to the scientific literature, the time-asymmetry described by the independence assumption has come to be known as the *fork asymmetry.*

independence assumption are quite different.[9] The micro-independence assumption is a free-standing principle for which there seems to be *no* observational evidence, even indirectly. The entropy increase pursuant to the *H*-theorem and the related asymmetry is no evidence for *micro*-independence, because the entropy increase pursuant to the *H*-theorem occurs only because the system is explicitly made to *start off* in a state of relatively low entropy, instead of in a state of thermodynamic equilibrium (if I've understood Price). In this sense, the system is a *macro*-system, and the entropy increase is (indirect) evidence only for macro-independence.[10]

The significance of this difference in the correlations associated with the macroscopic and microscopic versions of the independence assumption is that, although we may not reasonably expect *macroscopic* pre-interactive correlations (because they would be entropy *decreasing* correlations), we are not thereby required to rule out the possibility of *microscopic* pre-interactive correlations – because *no violation of the second law* is involved in the latter case.

Price argues that owing to the powerful intuition expressed by the independence assumption (founded on the above lack of entropy-reducing *macroscopic* pre-interactive correlations), we generally fail to properly appreciate that the asymmetry of causation is anthropocentric in origin. It is true that physicists generally do dismiss this asymmetry on the grounds that it is subjective (or anthropocentric), because they assume that the laws of physics are time-symmetric. Nonetheless, the perception of an asymmetry of causation continues to exert a very powerful, albeit hidden influence on their intuitions. Even physicists are human, after all. It is the main reason, according to Price, why the time-symmetric approach to quantum theory (a theory of the *micro*level as opposed to the macrolevel) has received almost no serious attention. It is thought that pre-interactive correlations would be preposterous, because they would require 'miraculous' correlations between systems which are about to interact. But Price points out that post-interactive correlations are equally preposterous (they seem unexceptional only because entropy was 'miraculously' low in the past): the only reason post-interactive correlations don't *seem* miraculous is because 'we already take for granted the very asymmetric principle at issue, namely that interaction produces correlations "to the future" but not "to the past"'.[11] In this way, although the usual assumption of micro-independence does not play a positive role in physics, it does get to play an important *negative* role.

Take EPR and Bell's theorem, and the spin correlation predicted by quan-

9 Price 1996, p. 121.
10 For Price's own illustration and remarks on why micro-independence is not observable, see Price 1996, p. 122. See also Ch. 2 of that work for a comprehensive and illuminating account of the Second Law.
11 Price 1996, p. 122.

tum theory between the separated twins. Bell's theorem depends explicitly, as we have seen in Chapter 3, on the assumption that the properties of the interacting physical systems such as the spin of the incoming particle at *A* and the setting of the distant magnet at *B* are independent. *Given* the assumption of micro-independence, this is uncontentious, of course. *Without* that assumption, though (in the words of Price), 'we would expect to find different hidden states in otherwise similar systems which were going to be involved in different interactions in the future. After all, this seems to be just what it would mean for systems to be correlated in virtue of future interactions, as well as past ones'.[12] Yet it is only *because* of the assumption of micro-independence that it is assumed that a single hidden state ought to be able to reproduce all the quantum predictions of quantum mechanics for any possible next measurement – something Bell showed couldn't be done (given micro-independence). Consequently, almost everyone lost interest in what may still be the best way of going forward, namely hidden-variable theories.

Or consider Boltzmann's *H*-theorem. The theorem crucially depends on the assumption that the molecules of a gas move *independently* of one another, i.e. *randomly, before* they collide (the molecular chaos assumption). In other words, they lack pre-interactive correlations of a relevant kind. Now, it might be argued that the independence assumption did play a 'positive role' in this case, because it enabled Boltzmann to derive his asymmetric result, his 'proof' of the generalized second law of thermodynamics (for gases) that entropy always increases. But of course, as is well known, Boltzmann managed the sleight of hand (his 'derivation') only by unwittingly sneaking in the time asymmetry (in the form of the molecular chaos assumption) into his premises to start off with – else he could not possibly have derived a time-asymmetric consequence from time-symmetric laws. Boltzmann himself came to appreciate that since the *H*-theorem is a statistical argument, it works equally well in *both* directions of time. As Price writes, '... we have no right to assume that it is an objective matter that entropy *increases* rather than *decreases*, for example. What is objective is that there is an entropy gradient over time, not that the universe "moves" on this gradient in one direction rather than the other'.[13] People were nonetheless seduced for a long time into thinking that Boltzmann had indeed achieved the impossible. To this extent, the role of the assumption was negative.[14]

Although the independence assumption is explicitly time-asymmetric, it is not thought to conflict with the time-symmetry of the laws of physics. The time-asymmetry implicit in the assumption is simply traced back to temporally

12 Price 1996, p. 126.
13 Price 1996, p. 48.
14 The above is my own illustration of the negative role of the independence assumption, informed, though, by my reading of Price.

asymmetric boundary conditions rather than to any intrinsic or lawlike time-asymmetry of the system itself that would violate time-symmetry. In other words, the laws themselves are time-symmetric, and only the boundary conditions ('initial' and 'final' conditions) are not. But this reply doesn't work in the case of microphysics, according to Price. The asymmetry inherent in the micro-independence assumption doesn't arise from the asymmetry of the boundary conditions. It is rather the other way around – our assumption that there is an asymmetry in the boundary conditions arises because we accept this principle.[15] If the micro-independence assumption *were* simply a matter of boundary condition, we would have no reason to accept it, since it is not supported by any observational evidence (being unlike the macro-independence assumption in this regard). For this reason, the asymmetry inherent in the micro-independence assumption cannot be a matter of mere boundary conditions. Instead, the assumption of micro-independence is a free-standing principle in its own right. To this extent, there is a conflict between the principles of *T*-symmetry and micro-independence. The latter principle cannot be accommodated within the usual picture of time-symmetric physics and asymmetric boundary conditions. We resolve the conflict in favour of *T*-symmetry, on the grounds both of symmetry, and because it receives strong empirical support from quantum mechanics.[16]

That then leaves us with the real puzzle of the entire business (as Price emphasizes): why was entropy so miraculously low in the past? And even *that* puzzle, as big as it is, isn't the entire puzzle, as Price also points out. Even the fact that entropy was miraculously low in the past is not sufficient to account for EPR and Bell's theorem, i.e. the baffling spin correlation predicted by quantum theory between the separated twins – unless we are either prepared to assume the kind of 'cosmic conspiracy' or superdeterminism already mentioned, or then settle for the standard interpretation's *non*-account.

Price's proposal is to relax the independence assumption in the direction of the future on the microlevel, and thus restore time-symmetry on that level, and thereby provide a handle on EPR/Bell. We shall examine that proposal presently. Before we do so, it will be useful to take a closer look at relaxing it in the direction of the past.

4.3 Relaxing the independence assumption in the direction of the past (superdeterminism)

Relaxing the independence assumption in the direction of the past is equivalent to the assumption of superdeterminism. In the context of EPR and Bell, that amounts to assuming not only that the experimenter's choice of measurement

15 Price 1996, p. 123.

16 Price 1996, pp. 117-18.

axis has been predetermined by the prior state of the larger physical system (which includes the pair of particles, the detectors and the experimenter), but also that such choice of axis has been strongly correlated in that prior state (the common cause) with the spins of the incoming particles so as always to accord with the quantum rules. In that case, classical 'instruction sets' (single hidden states) then suffice to give the quantum probabilities (these sets being the initial states of the particles' spins, synchronized at the time of the common cause with everything else in the world in such a way as to ensure a correlation with the settings of the detectors in accordance with the quantum rules in every subsequent experiment). Bell's proof is evaded, because his theorem depends crucially on the assumption that quantum systems are not correlated with the settings of detectors prior to their interaction. However, the fact that it is evaded fades into insignificance before the larger questions then confronting us. (Given superdeterminism, even Bell's theorem itself was already 'written' in the boundary conditions of the universe at its origin, and Bell himself was a mere automaton, as is the experimenter selecting the measurement axes, as are we all.)

Elaborating a little, the initial state of the world would have to be such as to bring about the consequence that any experimenters' selections of axes and the spins of the incoming particles in EPR-type experiments just *happen* to be correlated for ever afterwards in exact accordance with the predictions of quantum theory. It is because of the existence of this element that the determinism in question is called 'superdeterminism', as opposed to ordinary (Laplacian) determinism. In the context of EPR and spin, superdeterminism is invoked to explain the second of the two seemingly different kinds of spin correlations present in the EPR experiment (the two constituting the quantum-mechanical general properties (1) and (2) discussed in our summary of Bell's proof). The first is the ordinary kind of correlation expected in even classical mechanics, e.g. the *exact lawlike* correlation found when the spins of singlet-state particle pairs are measured along parallel axes (and more generally, the *statistical* correlation expected even classically when a particle-pair are emitted with random spin, and the measurement axes are set at random orientations). These correlations are explained on the basis of classical deterministic laws and initial conditions. The second is the new, apparently *intrinsically probabilistic* correlation predicted by quantum mechanics when the measurement axes are no longer always parallel. We've seen how Bell proved that quantum theory predicts that the spins are correlated more *strongly* than any ordinary classical theory can explain. This correlation, too, is lawlike according to quantum theory, but it conforms to an apparently *sui generis* statistical law characteristic of quantum mechanics (*sui generis* in the sense that in the standard interpretation, *nothing* (save the existence of an intrinsically statistical quantum law) exists to explain the statistical character of quantum mechanics; there are no hidden variables: the world is intrinsically statistical – not even a Laplacian superbeing, even if he could make measurements without disturbing the system, could predict with certainty whether the spins of the EPR/Bell twins will agree or disagree in any single case).

If we are given superdeterminism, though, even the *strong* correlation can be explained classically, with no new principles being required (save as below). It, too, arises simply as a consequence of the classical deterministic laws of physics and the initial configuration of the world – which was even *more* special than we've hitherto thought. (Penrose has estimated[17] that [even in the absence of superdeterminism] the initial state of the universe must have been 'fine tuned' to an accuracy of one part in $10^{10^{123}}$. If superdeterminism is added, this number would surely have to go up by many orders of magnitude.) That being said, it needs to be added that the latter kind of determinism (as opposed to the former) is something that no ordinary Laplacian determinism could countenance. That's because the weirdness of the *explanans* is even greater than that of the *explanandum*. Imagine repeatedly and vigorously shuffling a deck of cards, but finding that the shuffling fails to significantly disturb the order of the deck. It is as if the person doing the shuffling and the deck of cards somehow constituted a single system, 'preset' in the creation of the world to be joined in this way, perhaps through a Boltzmannian cosmic-scale 'fluctuation' (technically known as a 'Poincaré recurrence'), i.e. by pure chance, or perhaps simply for the private amusement of a malevolent Laplacian deity who gets a cheap thrill from the mystification of earnest, law-seeking scientists and philosophers. Even in the latter case, the only entirely *new* principles that would be needed would be those to explain the deity itself (or whatever other account we give of the relevant aspect of the initial conditions). Nevertheless, even in the days of Newton and Laplace, when the world was indeed thought to be perfectly deterministic, such 'superdeterminism' would have been generally considered unacceptable.

But why, exactly? If ordinary Laplacian determinism arises simply as a result of both laws and initial conditions, and it turns out that superdeterminism also arises simply as a result of laws and initial conditions (with no new principles being needed), then how do the two differ? Aren't both just cases of Laplacian determinism, and if so, why have the different *categories* of determinism? The answer is that there *is* something left over, and the categories reflect two crucial differences between ordinary Laplacian determinism and superdeterminism. One concerns the status of free will in the two pictures, and the second the status of what we take to be laws of nature. Consider the latter first.

4.3.1 How do superdeterminism & classical Laplacian determinism differ as regards laws?

This is not the place for an account of the nature and status of laws in scientific

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17 Penrose 1989.

explanation. But even without such account, I think we may say that the crucial difference between determinism and superdeterminism lies in an account of what laws are in each doctrine. There are some constant conjunctions or correlations in the world that we naturally and reasonably take to be a result of the operation of *laws*, even if they don't seem to fit into an existing mould, such as that provided by the classical picture. An example is the statistical correlation between the separated twin particles exhibited in Bell's version of (Bohm's version of) the EPR thought experiment. The correlation is formalized in the quantum measurement rules, which constitute a part of the laws of quantum mechanics. The correlation was in fact predicted by those laws. But suppose that superdeterminism is true. In that case we could be quite mistaken in thinking that the laws of quantum mechanics are *laws* at all, at least in so far as they differ from the classical laws. It could be that they merely *seem* so, as a consequence of the existence of the most peculiar past boundary conditions (and of course the existence of other, specifically non-quantum laws, such as those of, say, Newtonian mechanics). These boundary conditions are 'most peculiar' in the sense that everything that happens in the world, including the experimenter's choice of which measurement axis to select in the EPR/Bell experiment, is pre-ordained by the initial conditions of the universe to be such that the correlations and violation of the Bell inequality emerge upon repeated runs of the experiment every time. In this way, the cosmic conspiracy perfectly *mimics* the operation of a law, in this case a quantum law. However, the correlation is *factlike*, not *lawlike*. (Here is Hume's scepticism regarding cause and effect with a vengeance!)

The other possibility is that the correlation is factlike, not lawlike in the sense that the all the EPR/Bell correlations so far recorded are the result of a long-lived statistical fluke. There is no superdeterminism at work. In that case, too, all the correlations to date would have simply arisen from the existence of very remarkable boundary conditions, and wouldn't reflect the operation of any specifically quantum law. However, if only a statistical fluke is at work, there is no reason to expect it to continue. How, then, are we to account for the fact that the correlation, or 'constant conjunction' (in the statistical sense) 'keeps on keeping on'? The statistical-fluke explanation aside, there seem to be only possibilities. One is that the correlation reflects the operation of a specifically quantum law. The other is that there exists a monstrous and inexplicable cosmic conspiracy in the arrangement of the initial conditions of the world, the effect of which is to make us *think* that a specifically quantum law is at work. (God has a malicious sense of humour.) This, I think, is the relevant difference between the determinism of Laplacian determinism and the determinism of superdeterminism. In the latter, the initial conditions are unlikely in the sense that they attribute too much importance to us in the cosmic scheme of things,[18], whereas in the

[18] In Douglas Adam's spoof, *The Restaurant at the End of the Universe*, Zaphod Bee-

former, the initial conditions are unlikely merely in the sense identified by Penrose and Price.

4.3.2 How do superdeterminism & classical Laplacian determinism differ as regards free will?

Now let's see how superdeterminism might differ from ordinary Laplacian determinism in terms of *free will*. It seems that the former is incompatible with free will whereas the latter needn't be. Superdeterminism seems incompatible with free will for the obvious reason that the kind of determinism in it would not seem to allow it. If the state of the incoming particle already reflects my yet-to-be-taken decision regarding the detector setting (because both the state and the decision were 'set in concrete' in the initial conditions of the world), then in what sense is the decision to select that setting free?[19]

Classical Laplacian determinism, on the other hand, does seem to allow for at least the possibility of free will. As Price explains, there is a long philosophical tradition called 'compatibilism' that maintains that free will and classical Laplacian determinism are not mutually exclusive.[20] Of course, this tradition might be wrong – but then again, it might not be. The argument about the fatalist objection to Laplacian determinism is the philosophical twin of the fatalist objection to the truth value of statements about the future. Suppose that statements about the future already have determinate truth values. (This does not seem to be such an unreasonable assumption to make for a philosophically literate physicist, seeing that most seem to favour the relativity-inspired four-dimensional 'block universe' view of temporary metaphysics.) If statements about our future actions are 'already' true or false, then there are at least two possibilities regarding free will. One is that the ordinary fatalist argument is sound, and there is no free will. The other is that the argument is fallacious, and there is free will notwithstanding the fact that statements about the future have truth values. It seems that the majority of philosophers think the fatalist argument is fallacious and we have free will even when statements about the future have determinate truth values. There are also those who believe that the only way to save free will is by denying truth value to statements about the future.

However, the difference between ordinary determinism and superdeterminism as regards free will seems to be that in the latter, extra layers of explanation of quite an extraordinary sort are required, and the possibility of free will is

blebrox was the most important man in the universe because it turned out that the universe was created just for his benefit. Well, in the case of superdeterminism, it turns out that the universe was created just to fool us. (Cf. the age of the world and the fossil record.)

19 Cf. the Augustinian doctrine of predestination.

20 Price 1996, p. 237.

pushed back that much further. Future events are not only determined but super-determined by a cosmic 'conspiracy' of monstrous scope. That conspiracy, too, needs explaining. And given that there is such a conspiracy, what chance that free will should manage to evade its dragnet? Not much, according to Bell. Here is how he saw superdeterminism and free will:

> It may be that it is not permissible to regard the experimental settings a and b in the analyzers [magnets] as independent variables, as we did. We supposed them in particular to be independent of the supplementary variables λ, in that a and b could be changed without changing the probability distribution $\rho(\lambda)$. Now even if we have arranged that a and b are generated by apparently random radioactive devices, housed in separate boxes and thickly shielded, or by the Swiss national lottery machines, or by elaborate computer programmes, or by apparently free willed physicists, or by some combination of all these, we cannot be *sure* that a and b are not significantly influenced by the same factors λ that influence A and B [the measurement results]. But this way of arranging the quantum mechanical correlations would be even more mind boggling than one in which causal chains go faster than light. Apparently separate parts of the world would be conspiratorially entangled, and our apparent free will would be entangled with them.[21]

Price, too, observes that Bell's fatalist argument is quite distinct from usual causal determinism, in that if the state of the quantum object 'already' reflects the measurement setting, then we are not free to choose that setting. Furthermore, superdeterminism would need, according to Price, a 'universal mechanism of quite extraordinary scope and discrimination, in order to maintain the required correlations' – a 'vast hidden substucture underlying what we presently think of as physical reality... the cure seems worse than the disease'.[22]

The position, however, as regards both the fatalism and the need for a hidden substructure is very different if the independence assumption is relaxed in the direction of the future, at least according to Price.

4.4 Relaxing the independence assumption in the direction of the future (advanced action)

Suppose, then, that we relax the independence assumption in the direction of the future, as advocated by Price. In that case, an experimenter's selection of measurement axis can affect the *prior* state of the physical system. In particular, the mere selection by an experimenter of a measurement axis can determine the incoming particle's spin, in violation of the assumption of micro-independence. This is a proposal for backward causation, or *advanced action*, as it is technically

21 Bell 1987, p. 154.

22 Price 1996, pp. 237-8.

called. Given advanced action, we can regard the *fate* of the particle (i.e. whether its spin is measured along the A, B or C axes, and the result obtained) as a relevant causal property of that particle, even *before* that measurement, right back to its initial singlet state before it separated from its twin. We shall take a closer look at how this might work later on. In this way of looking at the matter, the fate of the particle is one of the *boundary conditions* relevant to an understanding of its behaviour, and a partial determinant of it.[23] Such a possibility has been mooted by several investigators prior to Price. O. Costa de Beauregard, in particular, has been a strong advocate of such an explanation of EPR since 1953.[24]

So Price is proposing that the *future* measurement result of a particle's spin, at, say, $t = 1$ in the space-time region **A**, can be a determining factor of that particle's spin at the origin at the earlier time $t = 0$. In fact, he is proposing that the future measurement result is a hidden spin property of the particle of some kind at $t = 0$, in the sense that there is a definite, albeit unknown and inaccessible, *initial value* of the spin at $t = 0$, which is determined at least in part by that later measurement.[25] Now, *if* a measurement result of a particle's spin at the

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23 A boundary condition defines some given state of a system, which may be taken as the starting point for the law-like evolution of the system to some other state. Recall that there is a dichotomy in physics between laws and boundary conditions. Newton's laws of motion, for example, do not predict how any given system will actually move. The equations have many solutions. To find a solution that is applicable to a specific case, the equations need to be supplemented by information about the actual values of the relevant dynamical variables for the *starting* state of the system (known as the initial conditions, or initial boundary conditions, or just boundary conditions). Once the initial conditions have been stipulated, everything follows with clockwork precision: the system evolves from the initial state to some later state that we take to be its final state. Both the initial and final states are known as boundary conditions. The initial boundary conditions lie in the *past* with respect to what is taken as the direction of the evolution of the system, and so they are known as *past boundary conditions*, while the final ones lie in its future, and are therefore known as *future boundary conditions*.

24 In a series of papers Costa de Beauregard has argued that information from the measurement of particle 1 travels backward in time along the world line of particle 1 to the singlet-state origin of the particle-pair, and then forward in time to particle 2, arriving there at the instant the information left 1. See e.g. Costa de Beauregard 1972, 1977, 1978, 1979, 1985. See also C.W. Rietdijk 1978; J. Rayski 1985.

There is nothing outlandish in invoking the notion of backward causation to try to account for EPR. For example, in his reply to the EPR paper, Bohr (1983) noted that the influence between the two connected systems separated by a spacelike interval is not of any *mechanical* kind: no energy is transmitted between them. Stapp, in turn, having quoted Bohr's remark, says that the space-time locus of the physical connection between the two systems, within the quantum mechanical formalism itself, is rather *via a path that runs backward in time to the space-time region where the pair originated.* (Stapp 1991, p. 8.)

25 Though it may not be clear how such a property is to be described. Nonetheless, to talk of the future measurement value as a 'property' of the particle at the earlier time $t = 1$ is consistent with the role ascribed by Bell to the 'hidden' parameters λ in a local realist theory. Bell writes, 'In a complete physical theory of the type envisaged by Einstein, the hidden variables would have dynamical significance and laws of motion; our λ can then be thought of as initial values of these variables at some suitable in-

later time $t = 1$ at the space-time region **A** can be a determining factor of that particle's spin at the origin at the earlier time $t = 0$, that same measurement result can equally also be a determining factor in the spin of its *twin* at the origin at $t = 0$ – since at the origin the pair are joined in a singlet state in which their spins must be perfectly correlated, even classically. There is no need anymore for either particle at the origin to possess a complete instruction set (a *single* hidden state) covering *every* contingency – including all possible detector settings for the future measurement of its twin's spin – the actual measurement details of which it must somehow take into account in 'knowing' what its own spin must be when measured. Instead, the initial hidden state is simply allowed to vary where necessary in accordance with *future* measurements made on its twin. So Bell's theorem is evaded, as are other 'no hidden variable' theorems. All these theorems depend on the assumption of micro-independence. Without micro-independence, though, we'd expect to find different hidden states in systems described by identical wave functions which were going to be involved in different interactions in the future.[26]

> According to the advanced action hypothesis, the hidden variables typically depend on the nature of the *actual* next measurement (or interaction generally). Had this measurement been different, the present values of the hidden variables might well have been different. Hence we should not expect the *actual* variables to reproduce the results of *merely possible* measurements.[27]

For each particle, then, the *relevant* details – those details it needs to know about the spin of its twin at the distant detector – are already present as an actual, but inaccessible, property of its twin in the singlet state at the origin, and therefore a property of *both* twins, since the singlet state is by definition a joint state. For that reason, the measurement result of the first particle's spin at space-time region **A** can be a determining factor of the second particle's spin at space-time region **B** (and vice versa). For the same reason there is no need for any superluminal influences.[28] The *mathematics* remains the same as in Bell's proposal (re-

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stant'. (Bell 1983b, p. 404.) See Chapter 3.

26 After Price 1996, pp. 125-6.

27 Price 1996, pp. 252-3.

28 This may be to labour the obvious, but in the *absence* of advanced action, and given the peculiar spin-correlation results predicted by quantum theory and obtained in real experiments, EPR requires that each particle somehow carries to its own detector not just a set of 'instructions' for measurement results for each of the three alternative switch settings of its own detector A, B, or C, but a more comprehensive set of instructions for the three settings that also takes into account, in the sense of partly depending on, the alternative switch settings A′, B′, or C′ and measurement results obtained at the other detector for its twin. Price's bringing in of future boundary conditions and advanced action has the result that the instruction sets each particle must carry to its detector are whittled down to something like the usual classical ones, where each particle needs to be directly responsible only for its *own* spin. As for its own spin details,

laxing the independence assumption in the direction of the past), but we merely give it a different *metaphysical gloss*, to use Price's words (and my emphasis).[29]

The upshot is that we appear to have both locality and counterfactual definiteness, at least to the extent required by EPR. Instead of Bohr's indeterminate reality, we have 'a reality which, while fully determinate before a measurement is made, is partly constrained by the nature of that measurement'.[30]

Despite this unexpected latter aspect, Price considers that we have pretty much the kind of reality that Einstein sought. And the latter aspect seems to vindicate Bohr, too, since one of his main contentions was that what we find reality to be is in part a product of the fact that *we have looked* (made a measurement). Price writes that the upshot is that 'the two great antagonists of quantum theory end up arm-in-arm'.[31] Of course it might be said that we've had to incur a cost, which was to bring in advanced action. But even the cost may be illusory because of the many advantages of a revitalized Einsteinian model utilizing advanced action.

4.5 Advanced action and avoidance of causal paradox

An obvious objection to backward causation is that not only does it seem to invite causal paradox, but that causal paradox sometimes seems unavoidable. In matters of deliberation, for example when trying to make up our minds about the best course of action to follow, it even seems to be a matter of *definition* that earlier events cannot be counterfactually dependent on later events, in the sense that we obviously cannot affect what we already *know about* at the time of deliberation, as Price points out.[32] We *hold fixed* what we already know or believe to be the case, and then hypothetically add each of various options available to us, and try and figure out what might follow from them. (Yes, we do have that skeleton in the cupboard; now what to do so as not to be found out?) The temporal orientation of our reasoning follows that of our own temporal perspective, and there's nothing we can do about it, because that's just the way things are.

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those are determined by the relevant boundary conditions (together with the laws), as in classical mechanics, save that *future* boundary conditions are as relevant as past boundary conditions in such determination. Hence, in an ensemble of particles prepared in the same state according to quantum theory (described by the same wave function) we'd expect to find considerable variation in their actual (but hidden) spin states.

29 Price 1994, p. 304. On p. 306 Price writes that both Bell's proposal and the advanced action proposal 'amount to the suggestion that quantum mechanics shows that what is in the world is simply a particular pattern of correlations (a pattern that classical physics had no business to exclude a priori). Whether we choose to interpret this pattern in terms of predetermination or backward causation thus turns out to be in an important sense beside the point – the brute physical facts are the same in either case'.

30 Price 1996, p. 125.

31 Price 1996, p. 230.

32 Price 1996, 'The role of counterfactuals', p. 169.

Price's reply is that it may be a matter of definition that we obviously can't affect what we already *know about* at the time of deliberation, but it certainly isn't a matter of definition that anything that lies in the past is *knowable*. Instead, it seems that the relation between temporal location and epistemological accessibility is *contingent in both directions,* and as a matter of fact rather underdetermined by our actual experience. For all we know, there may well be some of the past to which we don't have access, even in principle, and some of the future to which we do have access. The exact epistemological boundaries seem to be an empirical matter to an extent that is easily overlooked, as we shall see. As it turns out, the conventionalist can make sense of an objective issue about the 'correlational structure' of the world in a way that other accounts find very difficult. For once the subjective and metaphysical layers of the conventionalist account have been peeled away, a physical kernel remains, a kernel that is 'profoundly relevant to some of the deepest problems in contemporary physics'. This kernel is a possible *world structure* that has been almost completely ignored by contemporary physics.[33] We'll examine this structure and its relevance in §4.9. Our immediate task, though, is to see how the conventionalist view is sufficient to ensure that no causal paradox arises in the advanced action interpretation, and also how fatalism is avoided and thereby any causal paradox entering via the 'back-door' of fatalism, to the extent that fatalism is taken as implying the possibility of causal paradox.

4.5.1 Advanced action and causal paradox analysed in terms of two different conventions for assessing counterfactuals

It was briefly noted in the last chapter (§3.2, 'Bohr's reply to EPR') that there are two possible conventions for assessing counterfactual dependency (CFD), namely a stronger mode, which says, 'hold fixed the entire past', and a weaker mode which says, 'hold fixed only that portion of the past which is accessible in principle'.[34] The latter, weaker mode entails some revision of the usual understanding of CFD. It turns out that backward causation is paradoxical only if assessed in terms of the stronger convention, but not the weaker one.

Examples of backward causation assessed in terms of the stronger convention abound in the literature. A common one is travelling back in time, say along Gödelian closed timelike curves, to kill one's own younger self, thereby causing oneself to vanish in a puff of mad logic. The paradox is of course that if the younger you is killed, there can be no older you to come back and kill you. Only if you are not killed, can you be killed.

Examples in the literature of backward causation assessed in terms of the

33 Price 1996, pp. 174-7.
34 Price 1994, p. 328; Price 1996, p. 176.

weaker convention are rarer. The crucial thing that these latter cases have in common (beside the claim that later events can affect the past) is that it is not possible to *find out* whether or not the claimed earlier effect has occurred, *before* the occurrence of the (alleged) later cause. It is this property that ensures their avoidance of paradox, as the philosopher Michael Dummett already concluded more than thirty years ago.[35] He gave the example of a tribe who conduct a ritual dance to ensure the success of a hunt which has already taken place, but from which none of the hunters have yet returned (the hunt, as it turns out, was a successful one). Dummett showed that there is no inconsistency in the tribe's beliefs, provided that it's impossible to find out whether the hunt has been successful before the ritual is performed. That is, it is impossible to directly *falsify* the claim that the ritual worked.[36] Now, in the context of quantum theory, it turns out that the kind of backward causation required to make sense of the EPR-Bell type of correlation on the advanced action hypothesis is a *special case* of the weaker, 'Dummett' variety of backward causation i.e. 'hold fixed only the accessible past'.[37]

Here is an example of backward causation assessed in terms of the weaker ('hold fixed only the accessible past') convention that is more directly relevant for our purposes.[38] Suppose that a photon is approaching one of the detectors in our Bell-experiment. We would normally say that the photon's state of polarization depends on its past initial state. But suppose that the claim is made that its polarization *now* partly depends also on the details of the *next* measurement about to be made on it, including the setting of the detector it is approaching – but which it has not yet encountered. How could this claim be shown to be misconceived? Simple, it might be thought – measure its polarization *before* it reaches the detector and *then* set the future polarizer to conflict with the (then) known polarization of the photon, and so show that the claimed correlation is false. (A large number of experiments might be necessary.) But that wouldn't do, because if we were to do so, the in-between measurement *would be* the next measurement, and the setting of the other detector would be irrelevant. The *relevant* detector is now the one placed in between the photon and the original detector. Consequently, the measurement cannot be expected to show the polarization the photon would have had at the original detector if it had been allowed to get there unimpeded. Instead, the claim now applies to the in-between detector, and we are no nearer to showing that the claim is misconceived than we were. Dummett's loophole admits this case, too. The later measurement that's supposed to reveal a paradox is just the measurement that *changes* the past.

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35 See Price 1996, pp. 171-7 for a discussion.

36 Dummett 1954 and 1964.

37 See Price 1996, p. 174 on why it's a special case, and why Dummett's example is a little misleading.

38 After Price 1996, p. 175.

> By the time the earlier effect has been detected, its later cause has already taken place. In effect, then, quantum mechanics thus builds in exactly what we need to exploit the loophole in the bilking [paradox-revealing] argument.[39]

This example suggests to Price that the admission of limited backward dependence isn't as alien as might be thought. In particular, it seems that our actual use of counterfactuals is already sufficiently flexible to handle the kind of cases, such as the one above, that Dummett's loophole admits.

4.5.2 Advanced action, fatalism and avoidance of causal paradox; implications for free will

In §4.3.2 we saw that superdeterminism (relaxing the independence assumption in the direction of the past) seems to entail fatalism.[40] Wouldn't a story in terms of advanced action (relaxing the independence assumption in the opposite direction) be equally 'fatal', since the two ways of relaxing the independence assumption are formally parallel? And isn't fatalism usually taken to imply at least the possibility of causal paradox? Well, consider again the state of the photon approaching one of the detectors in our Bell-experiment (as we did above). The claim is made in the advanced action story that its polarization *now* partly depends also on the details of the *next* measurement to be made on it. For example, the next measurement may be made only tomorrow, yet is can be a determinant of the state of the photon today. So what is the state of the photon *now*, today (in the frame of some specified observer)? Is it polarized horizontally or vertically along a given axis? Is there a true/false answer to this question *now* in the advanced action story ('true' in the sense of 'already determined by future events', thereby suggesting fatalism)?[41] Suppose there is. In that case, and provided that we also think of 'already determined' as implying 'accessible', then we certainly have the basis of a paradox-generating thought experiment, according to Price. However, we've also just seen in the preceding discussion that 'determined' (in the sense of already being true), doesn't imply accessibility. That being the case, we needn't worry about paradox in those cases when the something that is true belongs to the class of things we *can't* find out about (i.e. the class of things covered by Dummett's loophole).

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39 Price 1996, p. 247.

40 Our question was: exactly how does superdeterminism differ from ordinary Laplacian determinism in terms of free will? We answered by saying that that the former is surely incompatible with free will whereas the latter needn't be.

41 Aristotle considered a closely related question in Ch. 9 of *De Interpretatione*. If statements about tomorrow are already true or false today, then is the future already fixed and unalterable?

But the possibility remains that even without implying the possibility of paradox, the advanced action story implies fatalism. In that case, advanced action and free will would be physically incompatible. The first thing to note, says Price, is that even if so, that wouldn't rule out advanced action; rather, theoretical simplicity might require instead that there be no free will. The second thing to note is that it is true that relaxing the independence assumption in the direction of the past and in the direction of the future are formally parallel. So it is likely that statements about the future have truth values just as statements about the past have truth values, and 'some of these statements [about the future] concern events or states of affairs which do stand in a relation of constraint, or dependence, with respect to certain of our present actions.'[42] He continues:

> However, what gives direction to the relation – what makes it appropriate to say that it is our actions that 'fix' the remote events, rather than vice versa – is that the actions concerned *are* our actions, or products of our free choice. The fatalist's basic mistake is to fail to notice the degree to which our talk of (directed) dependence rides on the back of our conception of ourselves as free agents. Once noted, however, the point applies just as much in reverse, in the special circumstances in which the bilking argument is blocked.[43]

It is plausible to argue, according to Price, that if we do relax the independence assumption in the direction of the future, the relevant earlier states *remain under the control of the experimenter* who selects the measurement axis – and that the measurement settings therefore remain free variables (in the most useful sense of that term) as required by Bell. Hence, fatalism is avoided.

4.6 No violation of the spirit of special relativity

Another important advantage of a revitalised Einsteinian realist view, according to Price, is that it is not in violation of the spirit of special relativity. Interpretations of EPR (such as Bohr's) in which the wave function is held to be a *complete* description of the state of the system run into a consistency problem. The problem is to do with the fact that these interpretations seem committed to admitting that there are spacelike (faster-than-light) 'influences' between the separated particle pair.[44] For example, a measurement of (say) the position of one of the

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[42] Price 1996, p. 246.

[43] Price 1996, p. 246. Price's brief comments here on fatalism in the specific context of advanced action may be usefully supplemented by the very accessible analysis of the doctrine of fatalism in Smart 1989, pp. 149-64.

[44] Stapp, for example, emphasizes that Bohr's response to the EPR argument acknowledges that the second system is *disturbed* by the measurement made on the first system (entailing the existence of faster than light influences between the systems). See Stapp 1991. I've already argued that Bohr would disagree with the 'disturbance' claim.

particles at space-time location **A** can affect the wave function of the combined system **A** and **B**, as is revealed when measurements are made on both particles, as Bell first showed. (The probability values for a particular result are correctly obtained by the usual quantum rules, and they generally differ from the expectations of EPR.) This is the case, as we've seen, even when the measurements are spacelike separated, i.e. when each lies outside the other's light cone. Now, when the pair of measurements are separated by a spacelike interval, a difficulty arises in determining which measurement occurred *first*. According to special relativity the question of which measurement 'really' occurs first is not physically meaningful, because the answer to it depends on the observer's frame of reference.[45] This is well understood, and generally not problematic. But it seems to generate a problem for the standard interpretation of quantum mechanics.

That problem is of direct relevance to the EPR/Bohr debate. For example, when we talk of the spacelike 'influence' from the initial measurement at **A** affecting the wave function at **B** (collapsing it), we normally presuppose that the measurement at **A** really did occur first, and collapsed the wave function at **B**. That accords with the (non-relativistic) way we are used to applying and talking about quantum mechanics.[46] But of course that way of talking is not really correct, just because it is non-relativistic, and the problem for the standard interpretation flows from this fact. Consider the following question: If there are spacelike influences, just *when* does the influence arrive? Take the EPR *gedanken-experiment*. Suppose that a position measurement is made at detector **A** and a momentum measurement at detector **B**, the two measurements being spacelike separated (as they are for example in the *real* Alain Aspect photon polarization experiment). In that case, according to special relativity, the answer to the question of which measurement occurs first depends on the observer's state of motion. For example, an observer moving sufficiently fast to the right of the origin along the path of the particles, considers that the *position* measurement at detector **A** occurred first, whereas an observer moving to the left of the origin (sufficiently fast) would consider the *momentum* measurement at detector **B** to have occurred first. Thus, the observer moving to the right would regard the following sequence of events as taking place: (1) the initial position measurement at **A** results in the total system moving from its original combined state ψ into one in which both particles have position-definite states; (2) the later momentum measurement at **B** results in the particle at **B** moving into a momentum-definite state, with the particle at **A** remaining in a position-definite state. (The second, later measurement does not result in a momentum-definite state for *both* particles because the first [position] measurement destroyed the momentum-correlation

45 See e.g. Penrose 1989, p. 287.

46 Recall that Schrödinger's equation, for example, is non-relativistic.

between the two particles.[47]) The opposite sequence of events would take place from the point of view of the observer moving to the left: (1) the initial momentum measurement at **B** results in the total system moving from its original state ψ into one in which both particles have momentum-definite states; (2) the later position measurement at **A** results in the particle at **A** moving into a position-definite state, with the particle at **B** remaining in a momentum-definite state. Consider the state of the particle at **B**. It is evident that the two observers will disagree on whether there was ever a time when (say) a position measurement on the particle at **B** would have given a result that both would have agreed on beforehand. (When the observer moving to the right considered that the particle at **B** was in a position-definite state, the other observer considered that it was in a momentum-definite state, and vice versa.) They'll disagree in a similar way about the results of a momentum measurement. Since there is no agreement between the observers, it seems that EPR's criterion of physical reality is not satisfied (§3.1). Or at least, as Price writes, 'The two perspectives yield different accounts of the "complete truth" concerned.' He emphasizes, 'Note that this isn't like special relativity itself, where the theory does provide a picture of the objective reality which underlies the frame-dependent properties of mass, length, and time.'[48]

The problem may be avoided in the context of the standard interpretation by supposing that the spacelike influence selects a privileged reference frame according to which the influence is instantaneous (i.e. in which the two events are simultaneous), or alternatively by supposing that a separate wave function applies in the case of each observer. However, the existence of a privileged frame would seem to be in conflict with special relativity, according to which all inertial frames are equivalent. It is also inconsistent with the Hamiltonian eigenvalue form of quantum mechanical equations in the other Lorentz frames. (The eigenvalue-eigenstate link [§1.2] breaks down.) As for the second option, it seems to bear little resemblance to the usual standard-interpretation understanding of the wave function.[49] There is, of course, no conflict with the overt causality of special relativity, as Penrose points out, because no actual *message* can be sent by the instantaneous influence,[50] but there is an essential conflict with its *spirit* in our

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[47] Price 1996, p. 205.

[48] Price 1996, p. 205. Cf. Penrose 1989, pp. 201, 303-4 regarding the Andromedan space fleet example. In the latter there is an interchange of space and time, in the EPR example, an interchange of position and momentum.

[49] Cramer 1986, p. 657.

[50] Relativity does not prohibit velocities greater than c as such, but only insofar as these velocities refer to the transfer of energy. The phase velocity of de Broglie waves (matter waves), for example, is c^2/v (where v is the velocity of the associated particle), and thus always greater than c. However, they transport no energy, because the energy is restricted to the *particle* aspect of the model (represented by the group velocity instead of the phase velocity). Since de Broglie waves transfer no energy, they

picture of physical reality. Penrose writes that this is 'a severe puzzle', which theorists of quantum reality have not been able to resolve.[51] Likewise, there seems to be a conflict with the spirit of the standard interpretation of quantum mechanics, with its eigenvalue-eigenstate link.

The physicist John Cramer emphasizes that the contradictions in the standard interpretations do not have consequences on the observational level because the wave function collapse is not an observable event. The collapse is a pseudo-event in the Copenhagen interpretation, asserted to occur when the state of knowledge changes. 'It is only when we require that the Copenhagen interpretation give an account of the collapse of some unique overall state vector and require that this account be interpretationally consistent with other established laws of physics that we reveal an interpretational paradox. The paradox is not a new one. It is the Einstein-Podolsky-Rosen paradox, but it is restated here in the language of the Copenhagen interpretation itself.'[52]

Price concurs with such views, concluding:

> In sum, the EPR argument continues to present grave problems for a complete description view of quantum mechanics, despite the apparent failure of the argument in its original form. The original argument assumed locality, and Bell's Theorem is generally taken to establish that this assumption is untenable. But nonlocality is not a problem for hidden variable theories alone. It is difficult to see how it can be accommodated by a complete description view, without rejecting one of the fundamental principles of special relativity, that there is no such thing as absolute simultaneity.[53]

That being the case, surely the natural way of proceeding is to go for a more Einsteinian view of quantum theory, obtained by relaxing the independence assumption in the direction of the future, with the consequence that there are *no* spacelike influences (in the sense of influences propagating outside light cones).

How might the standard interpretation try to accommodate the above nonlocality with the interpretation's 'complete description' claim. In an influential review article, Abner Shimony admits that 'there is a tension between the theory of relativity and the causal interpretation of correlated actualisations of potentialities'. In view of the tension, he asks, 'Should we relativize the identifications of cause and effect to the frames of reference?' He answers in the negative:

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cannot be used to send superluminal messages.

[51] For discussion, see Penrose 1989, pp. 286-7; Price 1996, pp. 204-5, 248; Aharonov & Albert 1981, *passim*; Cramer 1986, pp. 656-7.

[52] Cramer 1986, p. 657. Cramer notes that there has been some recognition of this dilemma among the founders of quantum mechanics. Dirac, for one, remarked apropos this problem: 'It is against the spirit of relativity, but it is the best we can do... . We cannot be content with such a theory [i.e. quantum mechanics as it is].' (Quoted from Cramer 1986, p. 657.)

[53] Price 1996, p. 205.

> The wiser course is to say that quantum mechanics presents us with a kind of causal connection which is generically different from anything that could be characterized classically, since the causal connection cannot be unequivocally analysed into a cause and an effect... This kind of causal connectedness between two events with space-like separation has no classical analogue, and no classical analogue should be expected, since quantum-mechanical potentiality has essentially broadened the concept of an event.[54]

Notice that Shimony explicitly rejects the first of the two options mentioned above (going to a preferred frame), and does not at least explicitly accept the second (that a separate wave function applies in the case of each observer). But his view, which is in the spirit of Bohr and Wheeler, seems to be tantamount to an implicit acceptance of the latter. There are no easy options. We shall touch on this problem again in Chapter 6.

4.7 Restoring symmetry on the level of hidden variables

In the photon example in §4.5.1, it seems that we reject advanced action at the cost of endorsing an objective temporal asymmetry. This appears to be a *symmetry argument* in favour of the advanced action view. The existence of such an argument would be a strong *prima facie* argument in favour of advanced action. Before looking at this argument, it may be as well to take a quick look at some other attempts (misconceived in Price's view) at restoring symmetry.

4.7.1 Misconceived attempts to restore symmetry

There is nothing novel of course about the use of advanced action to try to restore symmetry. Notably there are the time-symmetric Wheeler & Feynman 'absorber theory' of electrodynamics (radiation), and Cramer's 'transactional interpretation' of quantum mechanics, the latter generalizing Wheeler & Feynman's idea to quantum mechanics. The general problem addressed by each theory, at least for the present purposes, is, how are we to account for the temporal asymmetry of phenomena in the world of our experience, given the time-symmetry of the underlying laws of physics?[55] Take Wheeler & Feynman ('WF'). The temporal asymmetry that interested them was that of radiation. Why is radiation always retarded and never advanced? This problem is a particular case of the above general problem. Other particular cases of the general problem are the problems of accounting for the temporal asymmetries manifest in thermodynamics and cosmology.

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54 Shimony 1989, p. 387.

55 In fact, Wheeler & Feynman's motivation for the theory was rather different. It was to produce a theory of charged elementary particles which avoided certain prob-

WF secure symmetry in the case of radiation by requiring all radiative emitters and absorbers to be *individually* symmetric in time, in that each individual emitter and each individual absorber is associated equally with both retarded and advanced wavefronts. Seemingly contrary to appearances, both emitters and absorbers are centred on coherent wavefronts, half-retarded and half-advanced in each case (radiating equally into the 'past' and the 'future'). That being the case, no intrinsic difference exists between so-called emitters and so-called absorbers. There is an intrinsic time-*symmetry* to the actual microscopic radiative processes. It's true that radiative processes *look* unsymmetrical to us. But the WF model predicts that they *ought* to look unsymmetrical: calculation based on the model reveals that an asymmetry of retarded and advanced wavefronts will be found to exist in the world of experience because of interference effects.[56] WF bring in thermodynamics to their model to determine the *direction* of the predicted asymmetry, i.e. to fix the direction of the arrow of radiation. (The direction must be *retarded*, not advanced; the arrow must point in the *correct* direction, namely that of the 'future' and not the 'past'.)

Let's look at this part of the idea in more detail. In constructing their model, WF used the static pseudo-Euclidean (or Minkowski) universe with a uniform distribution of electric charges as their cosmological model. Because it is static, that model doesn't have a built-in cosmological arrow of time. This resulted in an ambiguity in WF's calculation of the reaction of the entire universe to the motion of a single charge shaken in the laboratory. Their claimed result was that all radiation in the universe ought to be fully retarded, just as is observed. They

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lems of infinities connected with the electromagnetic 'self-action' of charged particles. The quantum theory of the day was beset by a problem of infinities. The problem was connected with the quantum field-theoretical conception of the vacuum, and could be traced back to the action of the quantum particle on itself. The WF conception avoids the problem by forbidding charged particles to interact with their own radiation. Instead, the elementary interaction between the emitter and absorber is fundamental. Radiation is a two-step process – for every emitter there must be an absorber. In the absence of an absorber, a charge couldn't radiate at all.

56 When an atom radiates, or a radio station broadcasts, only half of the emitted wave or signal strength travels outward in the conventionally expected (i.e. 'retarded') manner in space and time, to be absorbed by surrounding atoms, or to be received by surrounding radio sets. The other half travels backward in time (the 'advanced' half) to be absorbed by other atoms or radio sets *before* it was sent – each of which in turn is stimulated to send out a return wave or signal. The initial emission sets off an exponential cascade of forward and backward in time waves or signals. It turns out upon calculation, however, that the stimulated return (advanced) signals have the cumulative effect of reinforcing the initial half-strength (retarded) wave or signal until it becomes a full-strength signal. Thus a full-strength retarded wave or signal is received by the surrounding atoms or radio sets after all, just as we find to be the case in our experience. Furthermore, it also turns out that all the advanced waves that could be bothersome in the sense of causing discrepancies with what's actually observed are cumulatively cancelled out through mutual interference.

arrived at this result as follows:[57]

The net disturbance from the charge is

$$F = \frac{1}{2}(retarded) + \frac{1}{2}(advanced).$$

The reaction of the universe, as calculated by WF, is

$$R = \frac{1}{2}(retarded) - \frac{1}{2}(advanced).$$

The total disturbance T leaving A is therefore

$$T = F + R = (100\% \text{ retarded}).$$

Simple substitution enables us to see that this is the case. It is also evident that this fully retarded character of the waves moving outward from a disturbed charge is due to the fact that the reaction R of the rest of the universe cancels out the awkward advanced part of F and augments the retarded part of it to the full 100% retarded value.

The ambiguity referred to above resulted from the fact that since the WF universe is time-symmetric, it is possible to reverse the direction of time in the above calculation and get T = (advanced). We can reverse the direction of time in the model by simply reversing the sign of the time coordinate. This leaves the universe unchanged but interchanges the retarded and advanced waves. This means that we have a new response from the universe which is the exact opposite of the old response. In other words, we can just as easily get the result that the total disturbance is advanced, instead of retarded as it ought to be. The new solution is just as valid as the old one. We thus have *two* possible electromagnetic arrows of time in the WF picture, pointing in the opposite directions, with nothing *within* the system to determine the choice of one over the other. (This is of course perfectly consistent with Maxwell's electromagnetic equations, which are indifferent to the distinction between past and future.[58])

57 After Narlikar 1973, pp. 82-3.

58 More particularly, Maxwell's equations don't tell us whether the waves arrive *before* or *after* they're transmitted. If we consider the equations alone, it's perfectly possible for the waves to go backward in time as well as forward in time. This is evident when we examine the form of the general solution of Maxwell's equations. The general solution is the following linear combination of the advanced and retarded solutions of Maxwell's equations:

$$F_{\mu\nu} = kF_{\mu\nu}^{adv} + (1-k)F_{\mu\nu}^{ret},$$

where k is an arbitrary constant which depends on the boundary conditions applying in

WF acknowledged that there are two solutions of equal status in their picture. They tried to eliminate this ambiguity by bringing in additional considera-

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any particular case, and $F^{adv}_{\mu\nu}$ and $F^{ret}_{\mu\nu}$ are four-tensors of the electromagnetic field (being the advanced and retarded solutions, respectively, of Maxwell's equations).

The advanced solutions of Maxwell's equation, in contrast to the retarded solutions, do not appear to correspond to physical reality, in that we don't seem to find any electromagnetic waves in the world that arrive before they're sent. (When a charge is accelerated, the disturbance in the field it creates, described by Maxwell's equations, always seems to ripple *outwards*, and eventually reaches the other charge which *then* responds to it, itself disturbing the field in turn. The disturbance never seems to reach the second charge *before* it is created by the acceleration of the first charge.)

There are at least three possible ways of reconciling the existence of advanced solutions in Maxwell's equation (and in wave equations generally) with the apparent absence of advanced action in the world. The three ways overlap to some extent.

(a) The first is to discard the advanced solutions, on the grounds that they are unphysical, corresponding to nothing in reality. No waves ever arrive before they're sent. This corresponds to setting the boundary condition k at $k = 0$ in the equation.

(b) The second is to accept the basic premise of the WF absorber theory of radiation, namely that wave-mediated interactions really consist of equal parts half-advanced and half-retarded waves, and that interference effects cancel out all the half-advanced waves, while at the same time reinforcing the half-retarded waves so as to make them fully retarded – which is supposed to explain why we never see advanced waves. The WF position corresponds to the choice of boundary condition $k = \frac{1}{2}$.

(c) The third way is simply to interpret the advanced solution as describing the *absorption* of electromagnetic waves, as Stephenson recommends (the retarded solution of course continuing to describe the emission of electromagnetic waves). That way, the characteristic negative energy of the solution has the appropriate effect of increasing the energy of the absorber. (If you take away negative energy from the positive energy of the absorber/emitter, as an advanced wave does, you in effect add positive energy to the absorber—which is consistent with the absorption of an ordinary retarded wave!)

Take a single charged particle in some volume V, bounded by a surface S. There will normally exist two kinds of electromagnetic waves within volume V: (1) waves emitted by the charge that is within the volume, and (2) waves emitted by charges outside the volume, which waves have entered the volume by crossing surface S. In Stephenson's proposal (and the same is true of Price's proposal), the *retarded* solution corresponds to waves of the first kind, and the *advanced* solution to waves of the second kind—waves that are absorbed within V. Maxwell's general equation then simply describes a system in which waves of both kinds are present, i.e. a system undergoing both emissions and absorptions. (The interpretative position remains unchanged even if the volume V is the entire universe, with all the charges within V. By taking volume V to be the entire universe, we're in effect stipulating a certain boundary condition, namely that there are no charges other than those within V that are emitters of radiation. Now, the constant k in the general equation is usually set at $k = 0$ to reflect this choice of boundary condition. It immediately follows from the general equation that all the radiation within V is of the retarded kind, as in (a) above. This particular boundary condition is known as 'Sommerfeld's radiation condition'. It amounts to the postulate that there is no incoming radiation from 'infinity', but that we allow outgoing radiation to infinity. Yet this case doesn't seem to differ essentially from the above one in which V contained only a single charge. There are still absorptions of retarded waves occurring within V, emitted by other charges within V, which can be reinterpreted as emissions of advanced waves, and there is nothing to prevent us from drawing individual boundaries S around every charge. The ratio of the two kinds of emissions now involves an aggregate of emissions by many charges, instead of by a single charge.)

tions from 'outside', namely the *thermodynamic* arrow of time. Take the T = (retarded) solution. In this solution, the initial state of a system of absorber particles was one of *rest*, and thus of order, i.e. low entropy. When these absorber particles absorb incoming retarded radiation, they are excited into motion and begin to collide with each other. Thus the end-state of the system is one of reduced order and increased entropy, the whole process representing a transition from order to disorder. Now take the second, equally valid solution, T = (advanced). In this solution, the absorber particles are in motion *before* they absorb incoming radiation, only coming to rest after they do so. (That's because the direction of time is reversed in this solution.) Thus the end-state of the system is one of increased order and *reduced* entropy, the whole process representing a transition from disorder to order, i.e. a transition that's the reverse of the usual one described by thermodynamics. In both pictures though, the electrodynamic and thermodynamic arrows of time point in the same way.

However, the advanced solution, while not impossible in principle on purely thermodynamic grounds, is ruled out as overwhelmingly *improbable*. As Davies puts it, 'The existence of retarded "radiation" is assured by the thermodynamic properties of the absorbing medium. The time direction of electromagnetic radiation is determined by the time direction of entropy increase in the universe.'[59]

WF conclude 'the 'irreversibility of the emission processes is a phenomenon of statistical mechanics connected with the asymmetry of the initial conditions with respect to time'.[60] As to the origin of this remarkable asymmetry they do not speculate, remarking merely, '[o]bviously the universe is a special system with respect to the origin of which probability considerations cannot freely be applied.'[61]

Price identifies two main difficulties for the WF theory. The first concerns their argument to derive the apparent temporal asymmetry of radiation from that of thermodynamics. The problem with the argument is that it relies on a temporal double standard. WF rule out the advanced solution as overwhelmingly improbable. But exactly the same argument would also rule out the retarded solution. That, too, is overwhelmingly unlikely on purely statistical grounds. So the WF argument begs the question.[62]

To see why Price says this, consider first the expanding ripples emitted by a stone dropped in the centre of a circular pond. Crudely speaking, the ripples are damped by friction at the edges of the pond by the earth banks. Their energy is absorbed by the molecules of the banks and converted into random motions of these molecules. The converse or backward process – a cooperative 'anti-

59 Davies 1974, p. 144.
60 Wheeler & Feynman 1945, p. 170.
61 Wheeler & Feynman 1945, p. 171.
62 Price 1996, p. 68.

damping' at the edges such that a coherent converging ripple is produced – is statistically exceedingly unlikely. In the backward process, as Price explains,[63] the random motions of the molecules of the banks would everywhere have to spontaneously give the right sort of 'nudge' to the adjacent water, a nudge that would need to be perfectly correlated with all the other similar nudges at other points at the edges of the pond. Yet what is the probability *on statistical grounds alone* for the occurrence of the correlated damping events at the edges in the forward process? Exactly the same as for the backward process. *Statistically,* an expanding ripple is exceedingly unlikely too. The probabilities are equal in both directions. By a parallel argument, the probability of the arrangement of absorber particles for incoming radiation is the same as that needed for outgoing radiation. Take an outgoing light wave from a source i located at the centre of an opaque spherical box that is absorbed by the walls. (The box can also model a perfectly absorbing universe.) What is the probability on statistical grounds alone for the occurrence of the correlated atomic excitations at the walls of the box as the light wave is absorbed? It is identical to the probability of the converse process (the correlated spontaneous de-excitation of the atoms of the walls such that a coherent converging wave is emitted by the atoms of the walls and absorbed by the central 'source'). As Zeh for example notes, the phenomena actually observed to occur in nature are just as improbable as those ruled out by the statistical argument.[64] So if the statistical argument rules out the advanced solution, it also rules out the retarded solution.[65]

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63 Price 1996, p. 54.

64 Zeh 1992, p. 13.

65 It has been pointed out by several authors that a more realistic calculation ought to involve *expanding* cosmological models, since we appear to live in one. However, there's a problem when we bring in expanding models into the picture. Narlikar (1973, p. 82) writes that calculations involving such models have been carried out by various people, all with similar broad conclusions. In ever-expanding big bang models we get T = (advanced), which is just the opposite of what we ought to get. The reason isn't hard to see. To get the correct answer, we need the reaction

$$R = \frac{1}{2}(retarded) - \frac{1}{2}(advanced),$$

from the future half of the universe. The trouble is, in the big bang models, matter density diminishes to zero in the future, and so there isn't enough matter (future absorbers) to produce the required R. On the other hand, in these models there *is* enough matter in the highly dense past half of the universe to produce the opposite reaction,

$$R = \frac{1}{2}(advanced) - \frac{1}{2}(retarded),$$

which is why the unwelcome solution T = (advanced) arises in these models.

Only in those expanding models that obey the 'perfect cosmological principle' (i.e. the universe is not only homogeneous and isotropic in space, but it is also unchanged in its large-scale appearance in time), and in which, therefore, matter-density always

It might be thought that *given* that there is a stone/retarded wave front in the first place, there is *then* a very high probability of the correlated events. That's true, but it doesn't help for two reasons. The first is that it just pushes the problem back to the initial conditions, as we've seen. Without assuming an objective beginning to the world (i.e. begging the question), why were the initial conditions special at one end but not the other in the first place, so as to produce stones/retarded wave fronts but not their converses? Second, and even more seriously for the WF proposal, its particular assumptions in fact *ensure* that the argument of boundary conditions works both ways. That's because WF take both advanced and retarded waves to *actually exist*. A source always emits *both* in equal proportion. *Given* that both exist, the correlated 'anti-damping' converging wave from the walls is no longer unlikely. The boundary conditions make it certain that it exists. The WF argument is thus in trouble because the upshot of the argument is (in effect) to 'show' that the net wave from an accelerated charged particle must be fully retarded *and* fully advanced.[66]

Price is right in my opinion. We now turn to the second difficulty for the WF theory, as identified by Price. He raises doubts concerning WF's justification for their claim that the advanced and retarded components of their waves are really *distinct*.[67] I'd like to take issue with Price on this point, as it touches on issues raised in Chapters 5 and 6.

WF represent the original retarded wave between a source *i* and the absorber(s) *j* as the sum of two equal components, the half-retarded wave from *i* and the combined half-advanced wave from the absorber(s) *j*. But according to Price, if we are to be justified in adding these components, we must have grounds for taking them as *distinct* in the first place. The claim that they *are* distinct can be criticized on the grounds that to derive a response of the appropriate magnitude from the absorber (given conservation of energy and momentum), the argument requires a full-strength retarded wave from *i* right from the start, not just a half-strength one. So at this stage of the argument the full-strength retarded wave needs to be sourced at *i*. But by the time the argument reaches its conclusion, one-half of the fully retarded wave from *i* is being accounted for as an advanced wave from the absorber. The upshot is that, unless we assume what we set out to derive, we end up with only a 75% retarded wave between *i* and *j*, not the 100% required retarded wave.

There are two possible responses to this objection. The first is that it in-

remains constant, is the correct (T = [retarded]) solution obtained. In general, models in which there is continuous creation of matter fare better than those with a big bang creation event. (Narlikar 1973, p. 83.)

For an extended attempt to attribute both thermodynamic irreversibility and time-retardation to the structure of the universe, see Hoyle & Narlikar 1974.

66 Price 1996, p. 69.

67 Price 1996, pp. 69-70.

volves artificially analysing the complete WF transaction into its time components. But because of the backward causation which is an essential ingredient of the transaction, that may not be done without paradox. No part of the transaction may be treated in isolation and be said to occur before some other part. It is the *entire theoretical picture* that tells us how we ought to treat the transaction. The picture that's created through Price's artificial analysis (treating the transaction as if it were of an ordinary kind without backward causation) may not be used to argue that the WF picture is paradoxical.

Even so, there is a way of breaking up the WF transaction into its temporal elements in order to overcome Price's specific objection (though not to eliminate causal paradoxes altogether). It is the second response. Here it is.

We admit that the absorber j doesn't emit a 1/2-advanced wave, as WF might like it to. However, that needn't worry them. That's because it does emit both a 1/4-advanced wave and a 1/4-retarded wave. The 1/4-retarded wave travels to absorber j_2, which in turn emits both an 1/8-advanced wave and a 1/8-retarded wave. The 1/8-retarded wave travels to absorber j_3, which in its turn emits both a 1/16-advanced wave and a 1/16-retarded wave,... etc. Now, all the advanced waves generated in this way travel back to the first absorber j (call it j_1) reaching it simultaneously. There is a linear superposition of all these waves at j_1. It turns out upon calculation (given sufficient absorbers[68]) that the sum of all the advanced waves from all the absorbers gives at j_1 a 1/2-advanced wave or as close to it as we want – which is just what's needed for the WF picture. Note that the problem isn't in the *reinterpretation* of the 1/2-advanced wave from j_1 as a 1/2-retarded wave once it's been legitimately generated by j_1, but only in giving it *causal efficacy* at j_1.[69]

68 And even in the absence of sufficient absorbers, as it turns out; see later in the argument.

69 It may still be objected that even if the above does give the correct 1/2-advanced wave from j_1 (and thus a 100% retarded [reinterpreted] wave from i to j_1), the above adds up to only a 1/4-retarded wave plus a superposed 1/4-advanced wave between j_1 and j_2. Once again the picture is missing half the required wave strengths. This objection would be invalid, though, because now the missing waves are supplied by j_1. That's because the above process provides a fully retarded (reinterpreted) wave from i to j_1 (which is now a completed transaction), not just the 1/2-retarded wave that we started out with. As a consequence, to avoid violation of the conservation laws, j_1 is accelerated some more. Notice that this occurs at the same time as it emitted the first 1/4-retarded wave. As j_1 is accelerated, it emits another 1/4-retarded wave to j_2, giving a 1/4 plus a 1/4-retarded wave at j_2. As for j_2, it in turn now needs to emit a 1/4-advanced wave between it and j_1 (to preserve the conservation laws). There is now both a 1/2-advanced and a 1/2-retarded wave between j and j_2 (or rather, a superposed 1/4 + 1/4-advanced wave and a superposed 1/4 + 1/4-retarded wave). And so on for ever.

At no stage of the argument do we need to assume a fully retarded wave at j_1 in order to get the correct response from j_1. The correct response comes naturally from the sum of all the lesser correct responses. The nature of advanced action that's involved ensures that everything happens at the right times for consistency, and preservation of causality. So also for the responses of all the other *j*s.

A possible counter-objection to the above objection would be the following one – which was in fact made by Price to the author.[70] Consider the advanced wave from j_2, between i and j_1. Wouldn't that destructively interfere with the advanced wave from j_1 (and the retarded wave from i), by the same principle which gives destructive interference before i? If so, the series 1/2 + 1/4 + 1/8... won't add up to 1.

The counter-objection is correct in the sense that there is such destructive interference and the series won't add up to 1, if all else is relevantly the same. For example, if we consider only the effect of the advanced wave from j_2, there'll be a loss of total (reinterpreted) retarded wave strength of 1/8 between j_1 and i. We may wish to attribute this loss to an inadequate absorber response. However, that's not a problem for the WF picture because all else is not relevantly the same. As it happens, the loss between j_1 and i is offset by an equal gain between $-j_1$ and i, where $-j_1$ is the next absorber after i in the series, i.e. when we extend the backward j series beyond i into the past null cone. (There is the series $j_n, \ldots j_3$. $j_2, j_1, i, -j_1, -j_2, -j_3, \ldots -j_n$.) Instead of a full cancellation (destructive interference) of the two advanced waves between $-j_1$ and i (these being the advanced waves from j_1 and i), there is now only partial cancellation of them. This creates advanced effects which have the net effect of restoring the missing radiative damping force on i, i.e. making up the deficit left by the passage of the advanced wave from j_2. The advanced wave from j_1 is built up to full strength between j_1 and i despite its partial cancellation by the advanced wave from j_2.[71]

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70 Private correspondence 5/6/97.

71 The problem posed by the partial cancellation of the advanced wave from j_1 by the advanced wave from j_2 is in principle the same as the problem that would arise if we were to apply the WF account to a system in which there weren't enough absorbers in the future null cone of the system to absorb all radiation. The future universe is transparent to radiation, either in part or whole. That's precisely the problem taken up by WF in 1945. They came up with a self-consistent solution of absorber theory in which there are fully retarded fields acting on any particular charged particle although the future null cone is transparent (Wheeler & Feynman 1945; see also Davies 1974, pp. 149-51). Consider again our opaque spherical box with a source i located at its centre. This time there is a single passage or opening cut into the wall. The WF solution describes circumstances – the heating up of the outer surface of the antipassage (a region of the wall of the box opposite the passage) – in which it appears as if 'negative energy is passed along the future null cone from the antipassage to i'.

Davies writes that the uncancelled advanced field of i [partly uncancelled in our

The WF absorber theory has been generalized by John Cramer to quantum mechanics.[72] Like the WF theory on which it is based, Cramer's *transactional interpretation* of quantum mechanics belongs to the family of advanced action interpretations, and is based on time-symmetric Lorentz-Dirac electrodynamics.[73] Cramer begins by taking the retarded wave described by the wave function to be physically real, at least to the extent that that the formalism contains wave functions represented in position space (as opposed to e.g. momentum space). He then tries to restore symmetry by adding an advanced wave function. Quantum events are described as 'handshakes' executed through an exchange of these advanced and retarded waves. Thus, the theory's basic element is an emitter-absorber interaction of the WF type, which replaces the collapse of the wave function of the standard interpretation. Cramer writes that the transaction may be regarded as a 'two-way contract between the future and the past for the purpose of transferring energy, momentum, etc, while preserving all of the conservation laws and quantization conditions imposed at the emitter/absorber terminating "boundaries" of the transaction'.[74] The transaction also erases all residual traces of the advanced waves. The completed transaction describes the exchanged particle. The transaction is explicitly nonlocal because the future is

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case] will appear as a disturbance coming in from infinity. That disturbance strikes the outer surface of the antipassage before *i* is set in motion, heating it up. The field will be absorbed as usual, and in being absorbed will generate response fields. The 1/2-advanced response field from the outside surface will bring the incoming 1/2-advanced field from infinity up to a fully advanced field while the 1/2-retarded response field will produce the following effects: (I quote)

(1) Cancel the 1/2 advanced field of *i* through the thickness of the antipassage wall; no disturbance therefore propagates through the wall.
(2) Cancel the 1/2 advanced field of *i* inside the cavity, thus removing any advanced effects inside the cavity.
(3) Act on *i* to make the force of radiative damping up to full strength.
(4) Make the 1/2 retarded field of *i* up to fully retarded; a test particle placed outside the passage will actually experience a fully retarded field.
In the direct interparticle action interpretation there can be no question of *energy* propagating out through the passage and away to infinity. The energy removed from *i* by the radiative damping force appears (1) on the inside face of the box where this is intact, (2) on the outside face of the box where it is not intact.
The latter phenomenon is all the more unusual for its occurring *before i* has been set into motion. (Davies 1974, p. 150.)

We cannot escape from paradox (advanced effects) altogether. But that's not surprising since the entire transaction really does need to be treated as a whole.

72 Another attempt at generalizing the WF theory to quantum mechanics is in Hoyle & Narlikar 1974. The book is an extended attempt to attribute both thermodynamic irreversibility and time-retardation to the structure of the universe. Much of the first half of the book is concerned with the task of showing how those quantum phenomena that are usually taken to arise from the zero-point fluctuations of the quantized electromagnetic field can be explained in a time-symmetric way in terms of the response of the universe.

73 For a comprehensive account of the interpretation, see Cramer 1986, pp. 647-87.

74 Cramer 1988, p. 229.

affecting the past, at the level of enforcing correlations. An equivalent and alternative interpretation of the process is in terms of a *four-vector standing wave* which exists between emitter and absorber. (Just as a familiar three-space standing wave is a superposition of waves travelling to the right and left, the present four-vector standing wave is a superposition of advanced and retarded components.[75]) Cramer points out that his interpretation closely follows the formalism of quantum mechanics, being indeed suggested by it:

> From one perspective the advanced-retarded wave combinations used in the transactional description of quantum behaviour are quite apparent in the Schrödinger-Dirac quantum formalism itself, so much so as to be almost painfully obvious. Wigner's time-reversal operator is, after all, just the operation of complex conjugation, and the complex conjugate of a retarded wave is an advanced wave. What else... could the ubiquitous ψ^* notations of the quantum wave mechanics formalism possibly denote except that the time-reversed (or advanced) counterparts of normal (or retarded) ψ wave functions are playing an important role in a quantum event? What could an overlap integral combining ψ with ψ^* represent other than the probability of a transaction through an exchange of advanced and retarded waves?...[76]

Cramer's theory has been criticized, notably by Maudlin, on several grounds.[77] The details won't concern us here, however. That's because, according to Price, the theory addresses an empty problem, at least to the extent that it depends on the WF conception of temporal asymmetry. In the quantum-mechanical case the asymmetry is of the *wave function,* which Cramer makes symmetric by adding a second wave function.[78] ~~So does that of WF~~. Both Cramer's and WF's attempts at interpretation are misconceived according to Price because *the world of radiation is already symmetric in time*. For example, for every potential emission of radiation there is a potential absorption of radiation – the latter being the temporal inverse of an emission. In a similar way, the asymmetry of the wave function is unproblematic.

A useful illustrative analogy Price gives is that of banking. In banking, for every potential withdrawal, there is a potential deposit – a withdrawal being thought of as the temporal inverse of a deposit. There is no asymmetry in the process of banking itself, even though the description of the process assumes a temporal orientation, moneys always appearing in the account balance *after* deposits and *before* withdrawals. The asymmetry is rather a product of how we apply the notions of cause and effect. In particular, there is no alternative struc-

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75 Cramer 1986, p. 663.

76 Cramer 1988, p. 229. See also Eddington 1928, pp. 216-17n.

77 Maudlin 1994, pp. 198-200.

78 However, Price suspects that Cramer's theory doesn't depend in any essential way on the WF conception, being amenable to reinterpretation along more congenial lines, just like the WF theory itself. (Price 1996, p. 75.)

ture that banking could have had – but turns out not to have.[79] Likewise, there need be nothing asymmetric in taking the radiation associated with emitters to be fully retarded. Radiation is simply retarded (outgoing) with respect to its point of emission, and advanced (incoming) with respect to its point of absorption. If the temporal framework is reversed, the labels are interchanged – emissions are construed as absorptions, and vice versa – but the above description remains true.

For this reason, there is no need to postulate the WF mechanism to secure radiative symmetry. Radiative symmetry already exists owing to the inherent time-symmetry of the radiative processes – emissions are matched by absorptions. Absorption is simply the temporal inverse of emission, and 'symmetry does not require that the two kinds of events be rolled into one'.[80] Rather than there being two equal components (retarded and advanced) to the waves associated with *each* emitter (and absorber, since in the WF theory each absorber is also an emitter), there is just one component – retarded (i.e. *outgoing*) in the case of emitters, and advanced (i.e. *incoming*) in the case of absorbers. On this view, the advanced solutions of Maxwell's equations characterize absorptions, and so do exist in nature. There is symmetry in this sense between absorptions and emissions. Price notes that this point has been made particularly clearly by Stephenson,[81] who pointed out in 1978 that '[o]scillating electrons are just as good at absorbing energy as they are at radiating it'.[82]

In other words, *symmetry* doesn't require that radiative emitters be *individually* symmetric in time. Symmetry is also secured if the class of emitters of retarded radiation turns out to be 'mirrored' by a class of absorbers of advanced radiation.[83] Radiative asymmetry in the real world simply reflects a statistical imbalance between large coherent sources and sinks of radiation – and *not* any asymmetry in the radiative processes themselves. Large coherent sources of radiation such as suns are common, but large coherent sinks or absorbers of radiation are uncommon.[84] In other words (as Price never tires of emphasizing), the real problem is to account for the asymmetric cosmological boundary conditions of the universe.

That's not to say, however, that there are *no* coherent sinks, or that coherent sinks are even uncommon, because every individual absorber is a sink for radiation. In particular, at the microscopic level there is perfect symmetry as regards

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79 Price 1996, pp. 58-9.
80 Price 1996, p. 74.
81 Stephenson 1978, p. 924.
82 Price 1996, p. 271, n. 12.
83 Price 1991b, p. 972.
84 Though Thomas Gold has argued in a now classic paper that empty space is in fact just such a sink – and the sink is ubiquitous. See Gold 1962. However, Price would probably say that the sink is not a coherent one.

sources and sinks: 'we have both coherent sources and coherent sinks'.[85] As Gold emphasized to Feynman at Cornell in 1963, '... every photon is emitted and also absorbed. Choosing the retarded fields does not create any asymmetry.'[86] That being so, *particle physics is temporally symmetric as it is*.[87] At the macroscopic level we only notice the sources, however, because only they combine in an organized way in sufficiently large numbers'.[88]

Returning to Price's banking analogy, it is as if a bank account were to gain its funds from a few large deposits, but lose them to many small withdrawals. There would be an asymmetry here, but the asymmetry wouldn't lie in the banking processes themselves. The microtransactions would be symmetric, microwithdrawals being the temporal inverse of microwithdrawals. The macroscopic asymmetry would arise only because the 'microdeposits clumped together in an orderly way to form macrotransactions, whereas microwithdrawals did not... To explain the macroasymmetry we would need to look at the bank's connections to the outside world – at why there was large-scale organization of deposits, but no corresponding organization of withdrawals'.[89]

Insofar as WF were concerned with securing symmetry, it would seem that they were dealing with a non-existent problem. The reinterpreted WF argument shows, argues Price, that the radiation field may be determined either by its past sources (emitters) or by its future sinks (absorbers), the two representations giving equivalent results. Consequently, even though Wheeler & Feynman were confused, the mathematical core of their theory can be *reinterpreted* in such a way as to show that they had the right idea after all – but merely tried to use it in the wrong way.[90] Others investigators, such as Cramer, have simply propagated Wheeler & Feynman's error, according to Price. In particular, it's not at all clear that Cramer's proposal depends in any essential way on the WF conception of temporal asymmetry. Cramer's interpretation, too, seems to be amenable to reinterpretation in a similar way.[91]

We shall now take a look at what Price takes to be the *real* problem of

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85 Price 1996, p. 71.

86 Gold 1967, p. 67. This book is the report of a meeting held at Cornell University in 1963.

87 At least, within well-known limits involving the behaviour of neutral kaons, as Price notes (1996, p. 74).

88 Price 1996, p. 71.

89 Price 1996, pp. 71-2.

90 Price 1996, p. 7. For a counterargument, see Ridderbos 1997. She argues (correctly to my mind) that Price's 'reinterpretation' of the mathematical framework of the WF theory is rather an alteration of it, as is evidenced by the fact that Price claims to have got rid of the need for a fully absorbing universe *without* the need for the concomitant explicit appearance of advanced effects. (See also the last paragraph of §4.9 in the present work, and §5.2.3, §5.3.)

91 Price 1996, p. 75.

asymmetry thrown up on the microlevel by the standard interpretation. (See also §2.5.2.)

4.7.2 An asymmetry on the microlevel according to the standard interpretation

According to Price, in the standard model of quantum mechanics the polarization of a photon after it has passed through a polarizer reflects the orientation of the polarizer. Its direction of polarization is effectively reset by the polarizer so as to match that of the polarizer. Price asks us to consider a video clip which depicts diagrammatic representations of photons of, say, non-polarized light passing through a polarizer, a single photon at a time, depicted according to the standard interpretation.[92] In each case, *after* a photon has passed through the polarizer (assuming it's managed to do so), it has a polarization exactly matching that of the polarizer, whereas *before* it reached the polarizer, its polarization was, in general, not correlated with the polarizer. (In effect, in the usual account, each photon's polarization is *rotated* by its passage through the polarizer.) However, the same doesn't apply when the video clip is played in reverse. In reverse, each photon *arrives* at the polarizer with a polarization perfectly matching that of the polarizer, and *after* its passage through the polarizer the two are generally no longer correlated with it.

The sequence of events as represented in the forward video clip in the case of each photon seems perfectly natural to us, and of the kind that we would expect to occur in the real world given the independence assumption – in the sense that the photon's polarization is correlated with the polarizer *after* it has passed through it, but not *before*.[93] The reverse sequence of events, on the other hand, seems highly unnatural and we wouldn't expect it to occur in the real world. In contrast, a video clip of just two or three interacting billiard balls would seem natural and respecting of both time-symmetry and the laws of physics *whichever way* it was run, whether forward or in reverse. (Even with a large number of billiard balls, that would be the case if they were arranged on the billiard table in a random configuration with random initial motions, and we ignored friction.)

The point is that even in the case of a *single photon*, there appears to be an *asymmetry* that enables us to tell the future from the past (that is, if our diagrammatic representation depending on the correctness of the standard interpretation is any guide). We 'explain away' this asymmetry in the obvious way – by

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92 Price 1996, p. 183.

93 As for the details and the statistical character of the rotation, I don't think anyone would regard that as just what we would expect to occur. Recall that the probability of each photon passing through the polarizer and thereby being 'rotated' is given by the quantum rule, $P = \cos^2\theta$.

extending the asymmetric macro-independence assumption to the microlevel. We say the polarizations of the incoming photons are random because they *haven't yet interacted* with the polarizer, whereas after they have interacted with the polarizer, their polarizers are no longer random. We fall back on initial conditions.

Now, it is true that in the world of our ordinary experience, the presence of post-interactive correlations and absence of pre-interactive correlations is usually ascribed to the initial conditions (associated with thermodynamic asymmetry). By extending the independence assumption to the microlevel, we're assuming that the kind of asymmetry exhibited by photons and other quantum particles can be explained in the same way. This is problematic, according to Price, for two reasons:

(a) There is *no* observational evidence of any kind for that assumption on the microlevel, as opposed to the macrolevel. Instead, we simply take it for granted that there are post- but no pre-interactive correlations. As Price puts it, 'We don't *observe* that the incoming photon is not correlated with [the] polarizer through which it is about to pass. Rather, we rely on a tacit meta-law that laws enforcing preinteractive correlations would be unacceptable.'[94]

(b) The above photon/polarizer correlation after interaction doesn't depend on the thermodynamic history of the system of photon and polarizer, or any larger system of which they might form part. Price asks us to imagine a sealed black box containing a rotating polarizer and thermal radiation that has always been in equilibrium with the walls (see §4.2). Even then, we'd expect the photons comprising the radiation to establish *post*-interactive correlations with the orientation of the polarizer whenever they pass through it. 'The presence of these postinteractive correlations does not require that entropy was lower in the past.'[95] By symmetry, then, the fact that entropy doesn't increase toward the future doesn't *ipso facto* rule out *pre*-interactive correlations.

Perhaps it ought to be mentioned that our expectation of post-interactive correlations in the box but not pre-interactive ones (given the standard picture), does depend on entropy having been lower *outside* the box, i.e. in the larger system of which the box is a part. If not, we couldn't tell which way the photons were travelling in the box in the first place – and so wouldn't be able to assign the descriptions 'post-'/'pre-interactive' to any correlations. In other words, we first need to impose an arrow of time on the contents of the box from the outside – and the outside has an arrow of time only because entropy has been lower there

94 Price 1997, p. S241.
95 Price 1997, p. S237.

in the past. However, this doesn't affect the substance of Price's point – which only depends on the assumption that the contents and inner walls of the box have always been in thermodynamic equilibrium.

In any case, Price argues for the above reasons that the asymmetry exhibited in the standard picture by photon interactions seems to be on a different footing from that exhibited by macroscopic systems. It is one which receives *no* support from the kinds of evidence and reasoning that we usually give for accepting a macroscopic asymmetry (arising from an asymmetry in boundary conditions), but, rather, it seems to be taken on board as a lawlike principle in its own right. This is problematic because it seems to endorse a genuine objective asymmetry in microphysics. To see this more clearly, consider another photon example.

Take a variant of the above photon case, one in which the photon passes through two polarizers before it reaches us. Call them the 'past' and the 'future' polarizer. The usual intuitive view is that the state ϕ of the photon in the interval between the polarizers doesn't depend on the orientation of the future polarizer, the one the photon hasn't yet reached. Price asks us to consider this intuition in the light of the (micro) independence assumption, and the 'hold fixed what is accessible' convention for assessing counterfactuals. We reasonably assume that what is accessible to us when the photon is travelling *between* the polarizers is, at most, the state of the photon at the *past* polarizer or in the region before it. An advocate of the 'hold fixed what is accessible' convention will then read the independence assumption as follows:[96]

(I) 'With the history prior to the past polarizer held fixed, changes in the setting of the future polarizer do not imply changes in the value of ϕ in the region between polarizers.'

Does this intuitive view involve a temporal asymmetry? To find out, we need to ask whether the intuitive view also endorses the *temporal inverse* of the above independence assumption. (It should, if no asymmetry is involved.) Here is the temporal inverse:

(II) 'With the course of events after the photon passes the *future* polarizer held fixed, changes in the setting of the *past* polarizer do not imply changes in the value of ϕ in the region between the polarizers.'

But there is little intuitive appeal in the latter proposal. It seems obvious, considered from the point of view of our own temporal perspective, that the state ϕ of the photon after it has passed the past polarizer but before it has reached the future polarizer depends very much indeed on the orientation of the past polar-

96 Price 1996, p. 187.

izer. Accordingly, we say that the photon's polarization has been 'rotated' in the course of the photon's passage through the polarizer to match the orientation of the polarizer (as it is sometimes informally put in the context of the standard interpretation). Accordingly, the standard interpretation – which presupposes our temporal perspective – rejects (II). However, it accepts (I). It follows that it endorses an objective temporal asymmetry. The advanced action proposal, on the other hand, rejects both (I) and (II), restoring symmetry.

According to Price, what is said above about photon asymmetry applies quite generally in microphysics. It doesn't rest on some peculiarity of the photon case. The standard interpretation of quantum mechanics provides any number of examples of the above kind of asymmetry. That's because the standard interpretation of quantum mechanics embodies an *objective asymmetry,* since in it the wave function is localized *after* but not *before* a measurement interaction. Price writes that in his examples he has been relying 'more on naive physical intuitions than on the quantum mechanical accounts of the interactions between photons and polarizers. The standard quantum mechanical accounts fits the naive picture, however...'

4.7.3 Restoring symmetry on the microlevel

Price's prescription for restoring time-symmetry is to reject the micro-independence assumption and allow correlations between photons and the polarizers in both time directions. The basic error, according to him, in the usual representation of photon spin is that it doesn't allow for the possibility that the spin-state of the photon *after* it passes through the polarizer might depend on the orientation of some future polarizer it will encounter.

Such a possibility may seem counterintuitive at first – but we need to bear in mind that there is no evidence for the usual contrary assumption. On the other hand, if the possibility is granted, the asymmetry vanishes.

In the above example of a photon that passes through a pair of polarizers before it reaches a spin detector, it seems intuitively obvious that the spin state ϕ of the photon in the interval between the polarizers depends only on the orientation of the first polarizer: in no way does it depend on the orientation of the second polarizer – the one the photon hasn't reached yet.[97] But Price correctly points out that this asymmetrical view is wholly hypothetical, since the region between the polarizers is effectively inaccessible, as we saw in §4.5.1. We can never *observe* (even in principle) the claimed non-correlation of the incoming photon with the polarizer through which it is about to pass. ('Hold fixed only the accessible past', was Dummett's loophole.)

[97] Price 1996, p. 187.

On the other hand, if we allow that the photon's spin-state between the polarizers depends on the orientations of *both* polarizers – as it does in an advanced action view – symmetry is restored. Hence Price's claim that the advanced action account amounts to restoring symmetry on the level of hidden variables – a powerful *prima facie* argument, according to him, in favour of the advanced action view.

How could such a simple argument have been overlooked, asks Price. Perhaps, he suggests, because we tend to think about these matters in terms of counterfactuals, and tend to think about counterfactuals in terms of the stronger 'hold fixed the entire past' convention. However, if we want to use counterfactuals, we need to recognize the possibility of using the weaker 'hold fixed the *accessible* past' convention. Once we do, 'it will be difficult to see why this course was not chosen from the beginning.'[98]

I have already argued (§2.5.2) that even in the standard interpretation, the photon is correlated with both polarizers, and there is therefore no objective asymmetry in that interpretation. However, that does nothing to diminish the problem of the interpretation of the wave function and its collapse in the standard picture. Even though Price's argument doesn't work, it nonetheless serves to highlight the natural way in which the question of the 'state' of an unobserved quantum-mechanical system is amenable to an advanced action interpretation.

4.8 Price's picture of an advanced action world vis-à-vis our causal intuitions

We have now examined Price's strategy for evading Bell's proof whereby he introduces advanced action into microphysics. We now need to see whether that strategy accords with our common causal intuitions. To do this, we need to step back and examine the notion of causation. (So far we've talked about the *direction* of causation, but little about the concept of causation itself.) What do we really *mean* when we say that one event causes another? What does backward causation mean? What would it mean to say that there is both forward and backward causation? Clearly, there is an asymmetry of dependence in all such talk, which asymmetry parallels the time-asymmetry of physical processes generally. We want to know where these asymmetries come from, given that the laws of physics are very largely blind to the direction of time. We start with Price's account of the asymmetry of dependence, and then seek to elaborate on it.

Take the time-asymmetry of physical processes first. The usual answer as regards the time-asymmetry, explains Price, is that it comes from the asymmetry of the boundary conditions.[99] The universe is thought to have been in a very

98 Price 1996, p. 179.
99 Price 1996, p. 116.

special state after the big bang, as we saw in the Introduction. Even though the laws themselves are time-symmetric, the boundary conditions are not – and the evolution of a system is determined not only by the laws but also the boundary conditions (the actual values of the relevant dynamical variables for the *starting* state of the system), the latter being necessary to pick out a particular solution to the laws from among all the possible solutions.

But there is a conflict between this picture and another time-asymmetric principle in physics which we've already discussed. That is the assumption of micro-independence. This assumption has the status of a free-standing principle in its own right, as we saw in §4.2, 'The independence assumption', in the sense that the asymmetry inherent in it doesn't arise from the asymmetry of the boundary conditions. Rather, it is the other way around – we say that there is an asymmetry of boundary conditions because we accept this (asymmetric) principle. For this reason the latter principle cannot be accommodated within the usual picture of time-symmetric laws and asymmetric boundary conditions. The conflict is resolved, according to Price, by allowing *T*-symmetry to win. We do so because not only is *T*-symmetry a *symmetric* principle, and *ipso facto* more desirable than an asymmetric principle, but it also receives strong support from quantum mechanics. (Certain aspects of quantum mechanics are best explained by relaxing the micro-independence assumption in the direction of the future and permitting backward causation). Moreover, there is no observational support for the principle of micro-independence, i.e. for any temporal asymmetry in pre- and post-interactive correlations in microsystems.

But if physics obeys *T*-symmetry, why does all causation appear to be from the past to the future? And how is backward causation to be accommodated in our picture of *T*-symmetric physics and an *apparently* one-way direction of causation? Even if theoretical adequacy requires the postulation of not only forward but also backward dependence in the world (giving up the principle of micro-independence), with a consequent restoration of causal symmetry just to the extent that both kinds of dependence exist, the *perceived* direction of causation nonetheless remains from the past to the future. How is that to be explained? What do we really mean, anyway, when we say that one event causes another?

To try and answer these questions, let us follow Price and begin by identifying the basic elements of our causal intuitions. The most basic element of all, says Price, seems to be *the temporal asymmetry of dependence*:[100] Events generally depend on what happens at *earlier* times, but not on what happens at *later* times. Closely related to this is the temporal asymmetry of *agency*: human actions influence later events but not earlier events. Likewise, the closely related concept of causation seems to involve a striking temporal asymmetry. It seems beyond

100 Price 1996, p. 133.

question that effects occur *after* their causes, but not *before* them. The big mystery is, why does causation show this strong temporal bias?

There have been two extreme strategies to try to reconcile the time-symmetry of laws with the temporal asymmetry of causation, as Price recounts in Chapter 6 of his book. The first is to deny the temporal symmetry of the microworld. There are at least two ways in which we could do that. One is to deny, following for example Penrose, and Prigogine, that the laws of physics are time-symmetric. The other is to maintain what Price terms a 'hyperrealist' view of causation, by giving causation some kind of *meta*physical role, in the sense that it is something over and above the concerns of physics – something quite as real as those aspects of the world with which physics concerns itself, but not reducible to those aspects (and presumably, not investigable by the methods of physics).[101] There seems little to recommend such views, and we shan't concern ourselves with them any further here.

The second extreme strategy is to eliminate altogether the notion of causation from physics, by relegating it to the dustbin of erroneous philosophical ideas – initially plausible but shown to be false by the progress of science. This kind of view ('causal eliminativism') is common among physicists, conscious of the fact that the underlying time-symmetry of physics appears to leave no room for asymmetric causation. The view has been famously espoused by Russell.

> The law of causality, I believe, like much that passes for muster among philosophers, is a relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm.[102]

One possible way of evading complete causal eliminativism and reconciling the sheer *usefulness* of causal talk with the symmetry of the laws of physics is to adopt a *conventionalist* strategy – the view that the asymmetry of causation and dependence is true by definition, and, as such, not inconsistent with the symmetry of laws, being merely a matter of convention. This kind of reply to eliminativism about causation is inspired by the philosophy of David Hume, who regarded the notions of causation, usually conceived of in terms of a necessary connection between cause and effect, as nothing more than a matter of constant conjunction – the latter being a temporally symmetric notion, as Price reminds us.[103] We simply use the terms 'cause' and 'effect' to mark, respectively, the earlier and later of a pair of events related in this way.

The main problem of conventionalism is that it is fine in so far as it goes – but *more* needs to be said (as Hume himself noted in the parallel case of personal

101 Price 1996, pp. 154-5.
102 Russell 1963, p. 132.
103 Price 1996, p. 153.

identity). How are we to account for the fact that some constant conjunctions just 'keep on keeping on', ensuring that the world doesn't fall into a heap? There must also be an objective element to causation. The particular problem posed by a Humean conventionalist account of causation for Price's proposal, however, is that if causation itself is purely subjective, in the sense of conventional, then so must be the *direction* of causation. And if so, 'how could there be anything to the claim that quantum mechanics shows that there is backward as well as forward causation?' So there are two related problems to be overcome if one wants to talk of backward causation within a conventionalist account of causation.

We now turn to a more sophisticated version of Hume's conventionalist strategy, which takes account of both these problems. This is Price's own version of the conventionalist solution, which he calls the *perspectival* view of causation. The basic motivation for this view, according to Price, is that there just doesn't seem to be enough genuine temporal asymmetry in the world to account for our intuitive judgments about 'what causes what'.[104] That's why the solution needs to be of a *conventionalist* kind (the first element of the required solution). Yet the usual conventionalist account won't do because, as noted above, there is more to be said. There must also be some *objective element* of the world which accounts for the usefulness of our talk of cause and effect, despite its conventional character, and which also needs to be reflected in our account (the second element of the required solution). In particular, it must permit an objective content to Price's backward causation proposal.

The first move, then, in Price's perspectival view of causation is to accept the conventionalist analysis that the asymmetry is anthropocentric; it is really to do with ourselves: the reason why we see asymmetry everywhere is because we are, as it were, 'always looking through an asymmetric lens'.[105] Accordingly, to make his first move work, Price now needs to find some asymmetric feature of our own circumstances, appropriately connected with causation and dependence, that could play the part of the asymmetric 'lens' – which would enable us to say that the asymmetry of causation is a projection of some internal asymmetry in ourselves onto the world, rather than being a real asymmetry in the world itself. He finds such a feature in the notion of *agency*. The crucial thing, according to Price, is that we are agents. We are concerned to bring about certain states of affairs in preference to other possible states of affairs. To achieve this, we deliberate about our actions, and this is a thoroughly time-asymmetric process. On this view, causes are potential means, and effects their potential end, and the asymmetry of causation simply reflects the asymmetry of the means-end relation. The essential asymmetry of agency belongs to the internal perspective of the agent, or the *experience* of being an agent.

104 Price 1996, p. 158.
105 Price 1996, p. 158.

> The origins of causal asymmetry thus lie in our experience of doing one thing in order to achieve another – in the fact that in the circumstances in which this is possible, we cannot reverse the order of things, bringing about the second state of affairs in order to achieve the first. This gives us the causal arrow, the distinction between cause and effect. The alignment of this arrow with the temporal arrow then follows from the fact that it is normally impossible to achieve an *earlier* end by bringing about a *later* means.[106]

The above 'agency' asymmetry of causation is quite consistent with the world as revealed by physics being time-symmetric. Price's strategy in his 'agency' view of causation is as follows. He first notes that there is not enough temporal asymmetry 'out in the world' to form the basis of our intuitive judgments about what causes what. Why not then simply suppose (he continues) that the apparent asymmetry and temporal orientation of both time and causation owe their origin to ourselves, arising from the subjective asymmetry of our perspective? Since the latter isn't objective, there is no real conflict between it and the temporal symmetry of physics. The perspectival approach dissolves the apparent conflict. And such a view is consistent with the direction of time and causation being inextricably related.

So far, so good. But Price also claims that quantum mechanics shows that there is backward as well as forward causation, and that this is a 'thoroughly objective' matter. The assertion of thorough objectivity seems incompatible with the subjectivity of the perspectival view. If the asymmetry of causation is perspectival, what room is there for the objectivity of even forward causation, let alone *backward* causation? As Price notes, '[f]ar from admitting advanced action..., this approach tends to reject even the ordinary kind of "retarded" or "forward" causation'.[107] If we want to advocate both backward causation and a perspectival view of causation, how do we avoid throwing out the baby (of backward causation) with the bath water (of objective dependence *simpliciter*), as Price puts it, leaving no objective content to the advanced action view?

To find out, we turn to the role of counterfactuals in deliberation. We first consider what would constitute objective *forward* causation, given the perspectival view, before turning to objective *backward* causation. As we've remarked above, agents are concerned to bring about certain states of affairs in preference to other possible states of affairs. They have, as Price says, a choice of various options, and base their choice not directly on the options themselves, but on what might be expected to follow from them. 'Thus, a typical deliberative move is to take what is given or fixed – or rather, in practice, what is known of what is taken to be fixed – and then to add hypothetically one of the available options,

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106 Price 1996, p. 157.
107 Price 1996, p. 181.

and consider what follows, in accordance with known principles or laws. The temporal orientation of this pattern of reasoning follows that of the agent's perspective.'[108] That is to say, we are constrained by that which presents itself to us as fixed from our perspective as agents.

What is known to be fixed, roughly speaking, is the past. Yet this constraint ('the past is fixed'), is governed by the contingent fact of our orientation as agents. We can perhaps imagine agents with a time sense opposite to our own, who would regard both dependence and causation as going, in our terms, from future to past. It is evident, from an atemporal perspective, that there is no matter of *fact* as to who has got it right. And yet it is also evident, given the meanings of the terms we ordinarily use, that it is straightforwardly true that we can affect the future but not the past.

Price seems right. This account successfully combines both of the seemingly incompatible elements of the asymmetry of dependence referred to earlier in the present section, namely its conventionalism, and objectivity. As regards the first of these elements, the perspectival account claims that our talk of causation is simply a *projection* from the kind of perspective we have as agents in the world.[109] In this sense, it is conventional. As regards the second element, it claims that it is our *de facto* temporal orientation as agents that *requires* us to choose the relevant convention that we do. This is the objective element in the perspectival account. We're not *simply* talking about words. From *within* a particular temporal perspective, it is an objective matter that we cannot achieve an 'earlier' end by bringing about a 'later' means. That's not so very different from the answer to the question, are lemons sweet or sour? We say that they're sour, of course. But we could easily have been constructed so as to find them sweet. Yet it seems wrong to say that they're *really* tasteless (rather than sour). The best option, according to Price is to say that the question doesn't make sense from the perspective we adopt when we consider the possibility of differently equipped tasters. To say that lemons are sweet makes sense only from 'within' some taste perspective. And from *within* such a perspective, it is an objective matter whether lemons are sweet or sour. For example, from within our own perspective, it is simply true that lemons are sour.[110]

So much for objective forward causation in the perspectival view. That view manages to combine both of the seemingly incompatible elements of the asymmetry of dependence (its conventionalism, and objectivity), even if it does leave open the question of just what accounts for our internal asymmetry by virtue of which we are ourselves enabled to play the part of the asymmetric 'lens' referred

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108 Price 1996, p. 169.
109 Price 1996, p. 158.
110 Price 1996, pp. 169-70.

to earlier.[111] But how does the possibility of *backward* dependence enter this picture?

Well, we have seen that the apparent asymmetry of situations involving human agency lies in our common experience of our inability to affect the past. We express such inability for example by commonly utilizing an asymmetric principle of counterfactual dependency in our talk. For example, we say that the counterfactual

(1) If the battle of Hastings had not taken place in 1066, then the Sellar & Yeatman book, *1066 and All That* would not have been written

is true, but the counterfactual

(2) If Sellar & Yeatman had not written their book, then the battle of Hastings would not have taken place

is false.

We have also seen that there are two possible conventions for assessing counterfactuals. One is to hold the entire past fixed. The other is to hold fixed only the *accessible* past. *Both* conventions are thoroughly compatible with the perspectival view of dependence, and ordinary usage doesn't clearly distinguish the two. Indeed, as Price points out, there seems to be a systematic indeterminacy in our usual notions of causal and counterfactual dependence.[112] It is the latter convention which provides the loophole for objective *backward* dependence. If, pursuant to that convention, our constraint is to hold fixed only the accessible past, then what is to prevent the *non*-accessible past being subject to both forward and backward dependence, should such be required for theoretical adequacy in view of otherwise difficult-to-explain experimental facts?

In the next section we shall consider what a world containing backward dependence would look like from a temporal perspective, given Price's account. We shall see that such a world would not be the simple time-reverse of a world with ordinary forward dependence. It would be rather a world containing a particular

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111 Try to imagine agents with a time sense opposite to our own. Is the perspectival view compatible with the actual physical existence of such agents? What objective fact could there be that makes us an agent of the one kind as opposed to the other? A plausible hypothesis, according to Price, is that 'our own existence and temporal asymmetry is ultimately explicable in terms of the thermodynamic asymmetry of the universe in which we live. This explanation does not presuppose an objective *causal* asymmetry, however'. (Price 1994, p. 321, n. 24.) In other words, the suggestion is that our own 'internal' asymmetry in the above regard may be explicable in term of an objective physical asymmetry existing in the world. This presupposes that the thermodynamic asymmetry of the world is not itself a projection of our own internal asymmetry. We shall return to this question in the next chapter.

112 Price 1996, p. 176.

pattern of correlations, and that pattern would be an objective matter. The pattern would be best described in terms of a world containing both retarded and advanced action. (When we so describe it, certain puzzles, such as the results of Bell's experiment, become understandable.)

What are the advantages of this kind of 'conventionalism' for understanding causal asymmetry? The most important advantages appear to be the following:

(a) The causal asymmetry doesn't reflect any further ingredient in the world, over and above that described by physics. It doesn't multiply the objective temporal arrows, as Price puts it.

(b) The perspectival view secures theoretical adequacy by managing to reconcile the subjective and objective aspects of causal asymmetry within a single theoretical picture.

(c) It leaves room for a violation of the dominant causal order, as indeed seems to be necessary for a local realist interpretation of quantum mechanics.

According to Price, the perspectival view leaves only limited room for a violation of the dominant causal order. However, as we shall see, the room is bigger than one might at first think – bigger even than Price seems to think.

What are the disadvantages? I can do no better than quote Price:

> The great disadvantage of this approach may seem to be that it makes causal asymmetry an anthropocentric matter. My view is that we should acknowledge this consequence, but deny that it is a disadvantage. Its effect is merely to put causation in its proper metaphysical perspective, as something like a secondary quality. As in the case of the more familiar secondary qualities, the shift in perspective may make us feel metaphysically impoverished, in losing what we took to be an objective feature of the world. The feeling should be short-lived, however. After all, if what we appear to have lost was illusory anyway then our true ontological circumstances are unchanged – and yet we will have made a direct gain on the side of epistemology, as we came to understand the source of the illusion.[113]

Callender for one feels impoverished. Though sympathizing with the conventionalist strategy, he also complains that adopting Dummett's loophole makes the asymmetry of dependence 'wholly a matter of the information available to the agent at the time'. That doesn't seem right to him. His ignorance about regions of the past doesn't incline him to think that such regions are 'open' like the future is 'open'. The fact that he doesn't know the state of an incoming photon only implies that he can't run a bilking argument against the supposition that his subsequent measurement of the photon affects its earlier state.'[114] It doesn't oblige

113 Price 1996, p. 160.
114 Callender 1998, p. 157.

him to take the step to believing in backward causation. More needs to be said. Callender is quite right. If one wants to go further, wider considerations need to be brought in. It is only our entire theoretical picture that can give us warrant for such a notion – and that picture generally rules out such notions in respect of macrophysical phenomena (save perhaps in those cases where the borderline between the microphysical and macrophysical is obscured, e.g. in the Schrödinger's cat gedankenexperiment, and the like). However, that leaves microphysical phenomena. We also need to make a clear distinction between even the most extreme kind of practical ignorance and in-principle ignorance of the kind evident in quantum mechanics. The latter kind of ignorance is best demonstrated in the Wheeler 'delayed-choice' type of experiments described in §1.2(e) and briefly discussed in §§3.5 & 5.1.2. It is difficult to see, in the absence of superdeterminism, how there could ever be an objective 'fact of the matter' about *the* state of the system in such experiments prior to measurement. Indeed, the entire point of the delayed-choice aspect of the experiments is just to ram home the lesson of quantum theory that there *cannot possibly be any such fact*. Questions about the state of the photon when it is in between the polarizers are in principle no different from questions about the 'state' of the unmeasured systems in the delayed-choice experiments, e.g. the delayed-choice split-beam experiment. According to Wheeler's Austin school (with its Copenhagen understandings), there is no fact of the matter. According to the advanced action proposal, there is a 'fact' of the matter, but the nature of the fact is unexpected, and there are those who find it unsettling.

4.9 How objective is the advanced action proposal?

And here we come to the big payoff.
(Huw Price, 1996)

In 1994 Price wrote that it is far from obvious what the advanced action proposal actually amounts to in physical terms. That's because the relation between causation and physical theory is itself obscure and philosophically problematic.[115] The above advanced action proposal should be taken, in the first instance, as an illustration of a *general strategy*, the point simply being that Bell's results no longer stand in the way of a local hidden variable theory for quantum mechanics. That's because, as Price put it in 1996, 'Dummett's strategy allows us to unlock a little of the past'.[116] Price's argument both in 1994 and today is simply that in *philosophical* terms, Bell's loophole (relaxing the micro-independence assumption) can be a much more attractive option than it's usually been taken to be, and hence that the general strategy it embodies has been un-

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115 Price 1994, p. 305.
116 Price 1996, p. 189.

justly neglected.

Price admits that all argument for a model utilizing advanced action is likely to be indirect, relying on non-observational considerations, such as its simplicity, symmetry, elegance, the failure of alternative models to produce the goods, and above all its avoidance of nonlocality and the possibility it offers of completing Einstein's project. After all, the 'logical space' that has been found for the possibility of such an interpretation lies entirely in the gap between the past and the *accessible* past (Dummett's loophole), and so we can't expect to 'see' advanced action *in action*, as it were. Nonetheless, it is important to realize that the proposal is not *just* verbal, i.e. non-empirical or metaphysical in the disparaging sense earlier noted. Factors of the above kind (symmetry, elegance, etc.) often play an important role in science.[117] The advanced action proposal concerns an objective issue regarding the correlational structure of the world.

> It turns out that there is a possible 'world structure' which has been almost entirely ignored by contemporary physics – partly, I think, because it has been assumed that the concepts on which its visibility depends were not really a matter for physics, being subjective (or 'metaphysical'), in the disparaging sense that physicists sometimes give to this term. In some respects this intuition is correct. But when we prune away its subjective and metaphysical thorns, a physical kernel remains – a kernel which turns out, as we'll see, to be profoundly relevant to some of the deepest problems of contemporary physics.[118]

This 'kernel' is a particular pattern of correlations existing in the world. A world with such a pattern of correlations is best described as a world with advanced action. What would such a world (a world with such a pattern of correlations) look like?

A world containing advanced action would look like a world which exhibited a mysterious, apparently nonlocal correlation between separated particles originating from a common source. More generally, it would simply look like a world which possessed quantum mechanics (and Bell's theorem).

For another way of thinking about our question, replace it by another question expressed in terms of the weaker of the two conventions for analysing counterfactuals (see §4.5.1). The question is

> What kind of world could coherently admit a convention according to which the [inaccessible] past might be counterfactually dependent on the future?[119]

117 After Price 1996, p. 180.
118 Price 1996, pp. 176-7.
119 Price 1996, p. 193.

This is, as Price points out, a question about the world itself, not simply about conventions. It is a 'thoroughly objective matter'.[120]

Well, such a world would look like a world containing advanced action, i.e. a world which, once again, exhibited a mysterious, apparently nonlocal correlation between separated particles originating from a common source, etc.

Furthermore, such a world would be one in which the two conventions for assessing counterfactuals *disagreed*, and the issue of which of the two conventions to choose to use wasn't merely verbal: when we 'divide through by the conventions', as Price puts it, 'there is a real physical remainder'.[121]

What might this physical remainder be? In the case of the spin-correlated twin particles described in Chapter 3 (the EPR/Bell experiment), it is the dependency of a particle's state on the setting of its future detector – the detector it is yet to encounter. This dependency extends right back to its initial state at the origin at $t = 0$. It has the consequence that one particle always 'knows' the spin of the other simply owing to both particles having been together in a singlet state at the origin at $t = 0$. In this regard, we've seen that Bell's theorem depends for its conclusion on the assumption that the statistical properties of systems are independent of future measurements. The theorem is evaded by relaxing the independence assumption in the direction of the future, entailing the adoption of the weaker convention for assessing counterfactuals (according to which the inaccessible past can be counterfactually dependent on the future).

Price writes that when the question of whether or not there is advanced action in the world is formulated in this indirect way, i.e. in terms of the two conventions for assessing counterfactuals, we don't even need that all parties concede the coherence of both conventions. Faced with recalcitrant advocates of the 'hold fixed the entire past' convention, the advanced action theorist, who holds fixed only the accessible past, can concede the others' usage – give them their convention – and simply fall back on the underlying objective issue.[122] And that objective issue is the fact that Bell showed that the statistical properties of systems are *not* independent of future measurements. In other words, the objective issue is the existence of Bell's proof together with the requirement of theoretical adequacy.

In this way of looking at things, Price writes, the 'state of a physical system might be *partially* determined by both its past and future boundary conditions, the two contributions being mutually compatible but individually incomplete'.[123] Certain things then become understandable, such as the claimed dependence of

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120 Price 1996, p. 193.
121 Price 1996, p. 178.
122 Price 1996, p. 178; 1994, p. 328.
123 Price 1991b, p. 974.

the state of a particle (located in Dummett's inaccessible region) on what we regard as a future measurement, as in the EPR case.

Price points out that the kind of backward causation involved here is of a more subtle kind than the simple time-reverse of ordinary forward causation, as usually conceived, e.g. as modelled by running a film clip backward.[124] When we run a film clip backward, there is still a *one-way* direction of causation, that direction simply having been reversed. What's more, it is usually obvious in the case of a film clip that the direction of causation is reversed (that the clip is being run backwards). In contrast, in the advanced action proposal, the causal arrow can lie in *both* temporal directions. And there's no way we can see that that's so because everything still looks normal as regards the direction of time. Instead, we have to rely on abstract reasoning involving mathematics to see that the appearance are deceptive.[125]

But not even an account in terms of both past and future boundary conditions is primary, according to Price. What is primary is the underlying correlational structure of the world. On a more general level, stepping more fully outside the anthropocentric viewpoint, we need not talk of a time direction at all. Instead, we need to distinguish between *two kinds of correlational structures for the world*. From the standpoint of the embedded-in-time observer, 'one "looks as if" it simply contains ordinary forward causation, while the other "looks as if" it contains a mixture of forward and backward causation. The latter structure is the one we need to solve Bell's riddle', according to Price.[126] The latter structure is also the one which can coherently admit a convention according to which the past might be counterfactually dependent on the future. It is an *empirical* matter whether or not the correlational structure of the microworld is of the latter kind. Quantum mechanics together with Bell's theorem suggest that the correlational structure of the world is indeed of the latter kind.

Price's *perspectival* view of causation provides the connection between the two different kinds of explanatory stories, the one in terms of forward and backward causation, and the other in terms of correlational structure. Price believes that the apparent asymmetry and temporal orientation of both time and

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124 Price describes it as 'backward causation, not forward causation backwards'. See Price 1996, pp. 190-1.

125 Somewhat as 2-D flatlanders would need to rely on abstract reasoning (geometry) to ascertain whether they lived in a 2-D world, or on the surface of a sphere. They'd need to add up the angles of a triangle drawn on the 'surface' of their world and see if the angles of the triangle added up to 180°. Alternatively, they could try circumnavigating the hypothetical sphere. This could be difficult if the sphere were very large, or expanding more rapidly than they could travel.

126 Price 1994, p. 322. Note that even though the latter structure 'looks as if' it contains a mixture of forward and backward causation (using Price's terminology), it still *looks* perfectly ordinary to temporal observers, apparently consistent with a one-way arrow of time.

causation is anthropocentric in origin, arising from the asymmetry of our own perspective. We've acquired our notions of causation in virtue of our experience as *agents*, argues Price.[127] When we extend this perspectival view to a microphysics in which the micro-independence assumption has been given up, and the past can be said to depend on the future, we get what is naturally described, from the ordinary asymmetric perspective, as bidirectional causation.[128] 'The temporal perspective of the interpreter imposes a *dominant* but not a universal causal orientation... the correlational structure of the microworld is of the latter (non-classical) kind.'[129] Consequently, as Price writes, '[t]here is an objective distinction between worlds which look as if they contain unidirectional causation and worlds which look as if they contain bidirectional causation' – the objective distinction being located in their correlational structures.[130] This is how Price manages to reconcile two apparently irreconcilable views, namely the anthropocentric origin of the asymmetry and temporal orientation of both time and causation, and the thorough objectivity of the question of whether there is backward as well as forward causation.

When analysed in terms of correlational structure for the world, it turns out that even Bell's 'no free will' or superdeterminism interpretation (described in §4.3), obtained by relaxing the micro-independence assumption in the direction of the past, has the same 'objective core' or correlational structure as Price's own backward causation interpretation (obtained by relaxing the micro-independence assumption in the direction of the future). In Price's words,

> both amount to the suggestion that quantum mechanics shows that what is in the world is simply a particular pattern of correlations (a pattern that classical physics really had no business to exclude a priori). Whether we choose to interpret this pattern in terms of predetermination or backward causation thus turn out to be in an important sense beside the point – the brute physical facts are the same in either case.[131]

We shall return to the subject of the correlational structure of the world. For now, I should simply like to make the observation, not pursued in any detail here, that Price's picture of the correlational structure of the world is analogous in certain respects to the Wheeler & Feynman picture in respect of radiation when there are inadequate absorbers in the universe to cancel out all advanced waves.[132] In Price's picture, we've seen that our temporal perspective imposes an apparent one-way arrow of time and one-way direction of causation on the world. But

127 Price 1996, p. 157.
128 Price 1996, p. 131.
129 Price 1994, pp. 306, 316.
130 Price 1996, pp. 193-4.
131 Price 1994, p. 306, n. 5.
132 For details, see Wheeler & Feynman 1945, pp. 171ff.; Davies, 1974, pp. 149-51.

when we scratch a little deeper theoretically, looking at quantum mechanics, we see that this is better regarded as an illusion. Instead, theoretical adequacy requires us to accept that the world actually contains backward as well as forward causation. In the WF picture, too, if there were inadequate absorbers, there would be explicit advanced effects – but they wouldn't be obvious to us, because the presence of advanced fields would look little or no different from the presence of fully retarded fields (§4.7.1). But that's not what would be actually happening. On deeper investigation, considerations of theoretical adequacy would point to the existence of advanced fields.

Summary

We have now described the core of Price's advanced action strategy at some length. The core is both sophisticated and highly plausible. It appears to provide a breakthrough in the interpretation of quantum mechanics.

Here are just some of the advantages claimed by Price for the revitalized Einsteinian view permitted by the advanced action proposal: Avoidance of the measurement problem, no violation of the spirit of relativity, modest ontological requirements, avoidance of fatalism, compatibility with Einstein's project, and restoration of symmetry on the microlevel.

These are no small claims, yet they all seem to be justified, save for the one concerning the restoration of symmetry on the microlevel. In Chapter 2, I argued that quantum mechanics in the standard interpretation is already symmetric despite the wave function collapse. But even that does nothing to diminish the attractiveness of the advanced action interpretation. If anything, it enhances it, by showing how *compatible* quantum mechanics ('the framework within which any correct theory must fit') is with an advanced action interpretation, when the two are closely examined.

At first sight at least, it would appear that Price's local advanced action strategy provides a natural heuristic for tackling the main counterintuitive aspects of quantum mechanics – the second of the four claims argued in this thesis. It may well be the key that fits the lock. However, even though the key looks as it fits the lock, the proof is in the unlocking. Details need to be scrutinized. That is the task of the next chapter.

We now go to see how Price's proposal fares vis-à-vis the formalism of quantum mechanics.

5

Is Backward Causation the Key that Fits the Lock?

Physicists have long known that the key might fit the lock, but with very few exceptions have thought... that it is too fantastic to be the true solution.

(Huw Price, 1996)

In the last chapter we introduced Price's general advanced action strategy for the interpretation of quantum mechanics. We now turn to a more detailed inspection of the proposal. We want to know how it connects with the formalism of quantum theory – in particular the quantum measurement rules, Planck's constant, the wave function and Heisenberg's indeterminacy principle.

5.1 Price's proposal & the quantum-mechanical formalism

There are two main features of quantum mechanics to be explained in an advanced action interpretation of it. One is EPR and Bell, or the correlation over spacelike distances of the EPR particle pair. In the last chapter we saw how advanced action shows promise of being able to explain the correlation. The other thing to be explained is the origin and peculiar nature of the quantum-mechanical measurement rules themselves, e.g. the two general properties described in §3.4, 'Bell's proof'. Note that this is a more general explanatory requirement than just solving the measurement problem (which, as we've seen, is just an in-house problem of the standard interpretation). Quite generally, as W.K. Wootters remarks, 'the framework of quantum mechanics is a framework for computing probabilities, and nature determines probabilities by squaring complex amplitudes: it is this prescription [too] that we want to explain'.[1] Planck's constant h is inextricably tied in with both features.

We saw in Chapter 1 that both the origin and the numerical value of this constant are mysterious. We also saw in that chapter that both quantization, i.e.

1 Wootters 1994, pp. 259-62.

the presence of h in the equations of quantum mechanics, and complementarity are expressions of one and the same underlying property of the microworld – which property is simply described by the two in different ways. Quantization is an *ad hoc* principle introduced into microphysics to make it accord with the phenomena. It is a mathematical expression of certain relations and quantities. Complementarity is the standard interpretation's attempt at explaining the relations expressed by the principle of quantization; it has come to have the status of a principle in its own right. Yet given a mixed system of the kind Sciama postulates (see §3.6), which seems formally identical to Price's proposal, h is nothing other than a measure of our ignorance of the future boundary conditions. And h is intimately tied in with the quantum measurement rules. So one would expect the details of h to fall out of the advanced action model in terms of quantities more fundamental to the model (as Sciama remarks), thereby submitting the model to a test. This looks promising. So what does Price have to say about the measurement rules and h in the context of his advanced action proposal?

5.1.1 Price's proposal & the QM prescription for computing probabilities

In 1996 Price wrote, 'If the initial hidden state is allowed to vary with the nature of the upcoming measurement, the problem of finding a hidden variable theory is relatively trivial'.[2] And in a later chapter of the same work, '...it is easy to explain Bell's results in quantum mechanics if we allow that particle states can be influenced by their future fate as well as their history'.[3]

> One way to do this would be to allow for a probabilistic 'discoupling factor' which depended on the actual spin measurements to be performed on each particle and which influences the underlying spin properties of the particles concerned. We might say for example that the production of such particle pairs is governed by the following constraint:
>
> > In those directions G and H (if any) in which the spins are going to be measured, the probability that the particles have opposite spins is $\cos^2(\alpha/2)$, where α is the angle between G and H.[4]

At this point the reader is disappointed. The above 'discoupling factor'[5] that Price gives is just the general quantum-mechanical formula for calculating the

[2] Price 1996, pp. 125-6.
[3] Price 1996, p. 246.
[4] Price 1996, pp. 246-7.
[5] Called *discoupling* factor by Price presumably because the more the angle α between G and H goes toward 90°, the greater the loss of spin correlation. At 90°, the correlation is entirely sundered.

probabilities – the same formula that we used in the demonstration of Bell's proof in Chapter 3.[6] It is well known, of course. But nobody knows what lies behind the formula, save that it is an instance of Heisenberg indeterminacy, and therefore an expression of quantum complementarity. (If, say, the value of the y-component of the spin is known, the value of e.g. the x-component remains unknowable even in principle.) Ultimately, the quantum measurement rules are a *postulated formalism* – postulated because they work.[7] Evidently, this part of Price's explanation is more of a *description* than an explanation. Price believes that advanced action is behind the formula, but just when the reader is all set for an explanation of the mathematics of Bell's results in terms of advanced action, Price, in effect, merely reiterates the formula. And that is all we get from Price on the subject, save for his general remarks on the correlation of photons with past and future polarizers, and the fables of 'Ypiaria' and the 'precognitive cat' (see below). But the reader wants to know just how advanced action is connected with the loss of correlation, and indeed, the non-classical correlation that exists even when α is not zero.

The point might be simply made using a photon polarization example. Take a pair of perfectly aligned polarizers. All photons that are passed by one are also passed by the other. Now rotate one polarizer by $\alpha = 22.5°$ relative to the other. At that angle, statistically 14.7% of photons that are passed by one polarizer are blocked by the other, with the remainder (85.3%) being passed. Significantly, though, the polarizations of the photons that are also passed by second polarizer are 'rotated' by 22.5% so as to match that polarizer. At $\alpha = 45°$, the proportion of photons blocked by the second polarizer is increased to 50%, with only 50% being passed by it. However, the polarizations of all that do pass are 'rotated' by 45°. As α increases, fewer and fewer photons are passed by the second polarizer – but the polarizations of such that do are 'rotated' by an even greater angle. At $\alpha = 90°$ no photon is passed by the second polarizer. Two questions arise: (1) Is the 'rotation' of the photon's polarization a consequence of advanced action? (2) If so, why does the advanced action become less and less efficacious as α increases, i.e. as the measurement becomes more and more of an incompatible, or *complementary*, property? Analogous questions arise in the Bell

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6 There, we said that the general rule for the probability of the second spin measurement agreeing with the first spin measurement is given by the formula: $P = \cos^2(\theta/2)$, where θ is the angle of the second measurement relative to the known spin direction of the particle whose spin is to be measured in the second measurement, i.e. relative to the direction along which a spin measurement would be certain to give YES, if made on that particle.

7 Take Planck's constant, which is absolutely central to quantum mechanics. We noted in Ch. 1 that it is introduced empirically, and has the character of *deus ex machina*. Nobody knows what it 'means' (with the possible exception of some advanced action theorists), save that it expresses the natural limit to which conjugate complementary quantities can be measured, and of course that it is a proportionality constant, connecting the wave and particle properties of matter.

case. The quantum measurement rules are the same in both.

It is perhaps surprising that Price has had so little to say about the quantum measurement rules (and h), save what he has said in connection with micro-independence. He simply takes the unexplained rules as givens and stops there, remarking merely, 'In a properly developed theory, something like this [local realist-theoretical explanation of the violation of the micro-independence assumption and the origin of the quantum measurement rules encoded in Bell's results] would no doubt emerge as a consequence of more basic principles'.[8] To find the best form of a local realist theory is a 'technical matter', after all, writes Price.[9]

This does seems to downplay the central importance of the task a little. It is true that, according to Bell at least, in his 1964 paper, there is no difficulty in principle in giving a hidden variable account of spin measurements on a single particle, and in that paper Bell gives an illustration in a simple case on how to obtain the quantum polarization rule for a single particle on that basis. Having done so, Bell says

> So in this simple case there is no difficulty in the view that the results of every measurement is determined by the value of an extra variable, and that the statistical features of quantum mechanics arise because the value of this variable is unknown in individual instances.[10]

Bell goes on to say that there is also no difficulty in reproducing the quantum-mechanical spin correlation between the *twin* particles if, for given values of the hidden variables, the results of measurements with one magnet now depend on the setting of the distant magnet.

Of course, the latter is just what we'd normally wish to avoid in a realist theory. Einstein, for one, wrote:

> But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former.[11]

But now, given the failure of micro-independence and Price's advanced action story, in which the backward influence of future measurements is confined to the past light cones of particles, such dependence is no longer an insuperable mystery. Rather, the dependence is, in a way, just what an Einsteinian realist would expect, because (a) the connection between the twins turns out to be *local* after all

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8 Price 1996, p. 282, n. 8.

9 Price 1994, p. 316, n. 17. See also Price 1996, p. 282, n. 8.

10 Bell 1983b, p. 404.

11 Einstein 1949a, p. 85.

(in the sense that there is no *direct* action at a distance), and (b) it turns out that it is the dependence – the *indirect* action at a distance – that actually helps provide the counterfactual definiteness of the individual twins, i.e. their 'complete' physical description, or the *reality* required in an Einsteinian realist theory. After all, Bell proved that the dependence is a requirement for *any* Einsteinian realist theoretical account. The real difficulty was the apparent nonlocality – which is now shown to be just that – *apparent* only. So there is no impossibility *in principle* in accounting for the correlations and the related probabilities by a local realist theory. The existence of the correlations and of a local realist theory are no longer mutually exclusive.

To my mind, Price's analysis of the independence assumption and its natural connection with the interpretative problem of quantum mechanics is a philosophical breakthrough. But that still leaves the highly non-trivial task of coming up with the properly developed physical theory – a theory which is relativistically invariant, moreover – in which (*inter alia*) the quantum-mechanical prescription for computing probabilities arises from more basic principles. The path is likely to remain 'lengthy and difficult', as Einstein once put it in a related context.

5.1.2 Price's proposal & the wave-behaviour of matter

How does Price's local realist strategy fare in accounting for the wave behaviour of matter? Take the two-slit experiment – that well-charted but nonetheless hitherto fatal reef lying between the realist explorer of quantum mechanics and Einstein's local harbour. How could Price evade the reef and navigate his realist bark into this snug haven without first needing to transform it into a Schrödinger wave in order to diffract around the reef?

The two-slit experiment is usually taken to illustrate wave-particle duality in the Schrödinger representation of the orthodox interpretation, and quantum complementarity more generally. And *nonlocality* is a crucial feature of the experiment in the orthodox interpretation.

This may seem surprising, as EPR/Bell is often taken to illustrate nonlocality, and the two-slit experiment the measurement problem, the background understanding being that the two experiments have little in common, save perhaps that both may be taken as illustrations of complementarity if one subscribes to the Copenhagen version of the orthodox interpretation.

Quite generally, the term 'nonlocality' is used to denote some form of action at a distance, where the nature of the action is counterintuitive owing to the absence of a classically describable form of physical mediation.[12] In the context

12 After Home 1997, p. 191.

of EPR, we saw in §3.1 that the absence of action at a distance was one of the elements of EPR's locality assumption. The other two were separability and counterfactual definiteness.[13] Can we sharpen up the notion of nonlocality some more? For the present purposes, Aerts & Reignier's minimal operational definition may do:

> An entity is "non local" if it is possible to prepare it in a state such that it can be influenced from macroscopically separated regions of space by (macroscopically) local apparatus acting only in one... of these separated regions at one time.[14]

The nonlocality exhibited by quantum-mechanical systems is predicted by orthodox quantum theory. The electron two-slit experiment exhibits nonlocality, owing to its characteristic self-interference effects and wave function collapse. The same goes for the related split-beam type of experiment, in which a wave packet is split by a Stern-Gerlach magnet/half-silvered mirror into two wave packets that go their separate ways. Heisenberg wrote regarding the latter,

> After a sufficient time, the two parts will be separated by any distance desired; now if an experiment yields the result that the photon is, say, in the reflected part of the packet, then the probability of finding the photon in the other part of the packet immediately becomes zero. The experiment at the position of the reflected packet thus exerts a kind of action (reduction of the wave packet) at the distant point occupied by the transmitted packet, and one sees that this action is propagated with a velocity greater than that of light.[15]

Wheeler showed that story is even more strange than that. We can choose whether the photon takes a definite route and so behaves like a particle or propagates simultaneously along two routes and so behaves like a wave, manifesting a 'two-beam interference phenomenon', even *after* the photon has finished

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13 Separability is the assumption that the world can be correctly analysed in terms of distinct and separately existing elements of reality, i.e. the complete physical state of the world is specified once one has specified the intrinsic state of each small spatio-temporal region of the world. It is a key assumption of EPR. Counterfactual definiteness is the assumption that hypothetical measurements would have led to definite outcomes. As we saw in §3.2, EPR 'derive' locality in terms of hypothetical results: *If* a certain measurement, say *q*, had been performed (which could have been performed but wasn't), then there is a certain value of *q* that *would have been obtained*. Not just any value would do. There is a *particular* value that would have come up. Unless counterfactual definiteness of *some* kind is presupposed, the notion of locality cannot even be formulated. It would make no sense, for example, to talk of the speed of light as the upper limit for the transmission of influences between systems, because the notion of a *disturbance* of the existing system by an incoming influence would make no sense, both 'system' and 'disturbance' being ill-defined.

14 Aerts & Reignier 1991, pp. 9-10.

15 Heisenberg 1930, p. 39.

interacting with the magnet/mirror, i.e. even *after the relevant interactions are over*. Evidently, not only is the action propagated with a velocity greater than that of light, but it can propagate backward in time.[16]

A like effect is found with neutrons. Neutron self-interference experiments also show the nonlocality of particles leaving the interferometer, this feature again suggesting the necessity of attributing a *wave* aspect to a quantum entity, at least within the standard interpretation. See below.[17]

The nonlocality in all these experiments arises in two ways.

1. The first is the obvious way, arising from the reduction of the wave packet. Take the split-beam experiment. We have a wave packet that is split by the magnet/half-silvered mirror, so that it is in located in both legs of the apparatus. A measurement at one leg localises the particle in one leg or the other. There is thus an instantaneous reduction of the wave packet throughout the apparatus. The experiment at one leg exerts a kind of action at a distance at the other leg, just as Heisenberg said. This is analogous to the EPR case, where the measurement on particle 1 influences instantaneously the measurement outcome on distant particle 2. In the present case, measurement at leg 1 influences instantaneously the measurement outcome on distant leg 2. If the particle is found in leg 1 it won't be found in leg 2, and if it isn't found in leg 1 it will be found in leg 2. The difference is that in EPR there is a pair of particles, whereas in the split-beam case there is only one particle, but that does not alter the similarity in the relevant respect of the two cases.

2. More generally, though, and this is the second way, the experiment demonstrates nonlocality even *before* the reduction, and quite independently of it. The nonlocality arises from the fact that the wave packet can be located in both legs of the apparatus in the first place. That's consistent with being a wave but inconsistent with being a localised particle. It is *being in both legs at once* before measurement that implies the need for reduction discussed in 1. above – else measurements couldn't have determinate results. By the same token, there can be no reduction of the wave packet unless the particle is described as a wave (or wave packet) to start off with.[18] This is the deeper source of the nonlocality. All the nonlocality can be sourced ultimately to the *wave* picture of matter of orthodox quantum theory. The same goes for the EPR and two-slit cases.

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[16] The lesson for Wheeler, though, an unrepentant (Austin School) Copenhagen theorist, is that 'no phenomenon is a phenomenon unless it is a measured phenomenon' (1978, p. 41). For details of the delayed-action split-beam experiment, see Wheeler 1978, pp. 31-3. This experiment is also well-described in Shimony 1988, pp. 41-2.

[17] See e.g. Aerts & Reignier 1991 for discussion; also Leggett 1987b.

[18] Although the language differs in the Hilbert vector space description of quantum mechanics, the upshot is the same.

If we are *given* the orthodox interpretation's wave picture of matter with its principle of linear superposition, and Born's rule for obtaining probabilities, then the nonlocality of the first kind isn't particularly surprising. It is, as already mentioned, predicted by orthodox quantum theory which explicitly includes the reduction postulate as one of its assumptions. In that case, the entire mystery of quantum mechanics lies in its wave picture and the wave-particle duality. For from the *wave* properties of matter flow the mysterious description of quantum-mechanical systems in terms of a wave function, the use of linear hermitian operators to represent physical quantities, the use of eigenvalue/eigenfunction equations, the expansion postulate, the quantum entanglement and self-interference of states, and the ensuing nonlocality. The measurement and reduction postulates, in turn, are necessitated in the standard interpretation by these.[19]

Bohr certainly thought that the nonlocality of EPR was wholly explicable by the duality. In his 1935 paper; his reply to EPR confined itself to an explication of complementarity – or the duality – in the context of the two-slit experiment (albeit with two particles), it being understood that to explain one was to explain the other.[20] However, we do not need to go that far. *Conceptually*, the nonlocality of EPR and the two-slit experiment appear to be different, in that you can have the nonlocality of EPR even without a real collapse of the wave function, for example with an epistemic interpretation of the wave function, whereas you can't have locality with a real collapse.

Feynman, too, considered that the two-slit experiment has in it the 'heart of quantum mechanics' and 'contains the only mystery'.[21]

Schrödinger also identified the expansion postulate and resulting interference of quantum states, which is of course a key-feature of the two-slit experiment (§1.2[e]), as *the* main difference between quantum mechanics and classical mechanics. He wrote:

> The remarkable theory of measurement, the apparent jumping around of the ψ function, and finally the 'antinomies of entanglement', all derive from the

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[19] This point has been recognized before. Labson & Rohatyn (1989, p. 283) go so far as to write: 'Bell's scenario is... simply a sophisticated variation on the "two slit" experiment described in many textbooks... , there, too, the sum of two single wave amplitudes (one hole open) is not equal to a single dual amplitude (both holes open), thanks to wave interference. Thus, bell's [sic] result is no more mysterious or astonishing than wave-particle duality. Indeed, it is the same thing in another guise. This means that "quantum weirdness" is no different (or more severe) now than it was in the 1920s, when it was first discovered.'

[20] For a careful analysis of Bohr's response to the EPR paper in terms of complementarity, see Bub 2000, pp. 196-204.

[21] Feynman, Leighton & Sands 1965, III-1, p. 1.

> simple manner in which the calculation methods of quantum mechanics allow two separated systems conceptually to be combined together into a single one; for which the methods seem plainly predestined. When two systems interact, their ψ functions, as we have seen, do not come into interaction but rather they immediately cease to exist and a single one, for the combined system, takes their place.[22]

Given that the main puzzle in orthodox quantum mechanics is its wave-particle duality of matter, Price's backward causation theorist's task would seem to be to explain, in terms of a local advanced action theory, not only EPR/Bell, but also the two-slit and split-beam experiments, *without* needing to ascribe mysterious wave-properties to matter. Advanced action – relaxing the micro-independence assumption in the direction of the future – shows promise of being able to do just that. In this connection, it needs to be understood that the Copenhagen explanation of the nonlocality that was so disturbing to Einstein in both the EPR/Bell and the two-slit/split-beam experiments lay in the postulated wave-particle duality of matter. Here is the argument in greater detail.

Take the EPR/Bell experiment. In the orthodox interpretation of EPR/Bell, the spins of the singlet-state particle-pair are correlated only because their wave functions are 'entangled'. That is, the spins of the two spacelike separated singlet-state particles, are described by a *single* wave function which is a linear superposition of the individual wave functions of the two particles, each such individual wave function possessing two eigenstates of zero total angular momentum. It is the definite phase relations, i.e. wave properties, that exist between the linearly combined individual wave functions that allow for the interference of possibilities and resulting correlation of the spins of the particles (see Appendix[d]). The definite phase relations also imply that if the same component of the spin of each particle is measured, the results will be correlated.[23] In this sense, the pair of particles constitutes a single system. Now the point is, the wave functions of the singlet-state particle pair couldn't be phase-correlated or 'entangled' if the particle-pair weren't described as some kind of linearly superposed *waves* to start off with – and the waves interpreted as *more* than mere mathematical fictions. It is difficult to see how ordinary classical probability functions could be entangled in such a fashion, since they are mathematical fictions. The entanglement and possibility of interference is broken by a measurement, resulting in a real collapse of the wave function.

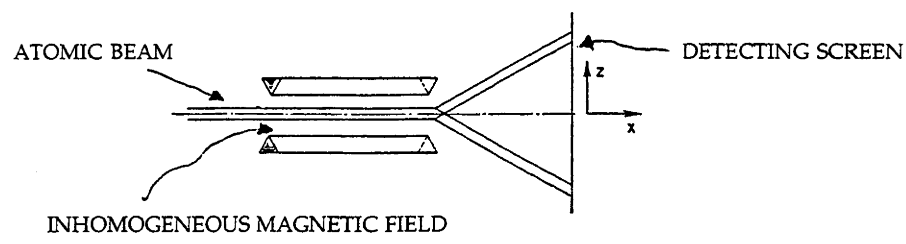
Now take the two-slit experiment. Its analogue of the EPR/Bell-correlation is the interference pattern of 'hits' or detection events that emerges on the detection screen after many runs. This pattern is inconsistent with a local realist particle picture, just as the EPR/Bell correlation that emerges after many runs is

22 Schrödinger 1935a, in Wheeler & Zurek 1983, p. 167.
23 Bohm 1951, p. 616.

inconsistent with a local realist particle picture. The interference pattern over many runs is consistent with the self-interference of *waves,* however, and the waves' subsequent collapse, and that's just how orthodox quantum theory explains it. It predicts such a pattern on the basis of the cross-terms (or interference terms) in the formalism between the superposed possibilities when applying Born's rule. The point is again, the orthodox interpretation relies on the wave aspect of matter to account for the two-hole interference pattern.

Next, take a 'split-beam' type of experiment, involving a single particle or entity. Examples are the interference experiments carried out by the Rauch group,[24] showing self-interference by single neutrons. A detailed account of one of these experiments may be found in Aerts & Reignier 1991; none will be given here. To make a long story short, the observation of interference patterns for low-density beams of neutrons shows that in the interferometer, the wave function of a single neutron is split into two parts, with the consequence that *'the single neutron can be influenced from macroscopically separated regions of space by (macroscopically) local apparatus acting in only one of these separated regions at one time'* [authors' italics].[25] This is just Aerts & Reignier's minimal operational definition of nonlocality, already quoted. The authors conclude that the picture of a 'localized' neutron following a definite path is wrong; such a localized neutron could only explore a narrow neighbourhood of one of the two paths. They also note that the experiment demonstrates the *necessity of attributing a wave aspect* to the neutron.

Another example is the familiar Stern-Gerlach experiment. Consider the standard textbook account of a beam of spin half atoms passing through a Stern-Gerlach magnet.[26] The idea is that in the experiment there is a source-preparation area from which, we have reason to believe, silver atoms or hydrogen atoms (magnetic dipoles) are emitted in a fairly definite state, in this case with total spin $S = \pm 1/2\hbar$.



The apparatus measures the spin direction of the atoms along a designated axis determined by the direction of the magnetic field's inhomogeneity (the z axis above). The apparatus does so by separating the z-up atoms from the z-down

24 See e.g. Rauch 1988.

25 Aerts & Reignier 1991, p. 13.

26 The following account of the textbook statistical interpretation of the Stern-Gerlach experiment follows Bohm 1951, pp. 602-4.

atoms in the initial beam by splitting it into two beams, corresponding to the two permitted spin orientations of the atoms in the magnetic field (i.e. permitted by space quantization). The upper beam consists of atoms in the spin up state, and the lower beam of atoms in the spin down state. The entire measuring apparatus may be regarded as the combination of the inhomogeneous magnetic field, the position coordinate of the atom and the detecting screen.[27]

Now, *before* measurement, when an atom is on its way to the Stern-Gerlach apparatus, it is necessary that it be in a fairly definite state, otherwise no conclusions about the value of the z-spin can be drawn from this experiment. The z-dependence of the atom's wave function will take the form of a *wave packet*, denoted by $f_o(z)$. The initial wave function for the system is then $\Psi_o = f_o(z)(a\psi^{\uparrow} + b\psi^{\downarrow})$, where $\psi^{\uparrow}$ and $\psi^{\downarrow}$ are the spin wave functions belonging, respectively, to S_z = up and S_z = down, and a and b are the coefficients of these spin functions.[28]

On its way out of the Stern-Gerlach apparatus, the atom is represented by the wave function $\Psi = \psi^{\uparrow}(z)\, e^{i\alpha_{\uparrow}} + \psi^{\downarrow}(z)\, e^{i\alpha_{\downarrow}}$. We are interested in the physical meaning of the state represented by Ψ. Now, it is well-known that the purely classical picture of a particle having a quantized rapidly precessing magnetic

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[27] After Bohm 1951, p. 594. The Stern-Gerlach experiment was designed in the 1920s to test for space-quantization, i.e. whether it was true that quantum spin can take on only quantized values as predicted by quantum theory. The essence of the experiment is as follows:

A beam of neutral atoms is formed by evaporating silver from an oven. It contains on average an equal number of spin up and spin down atoms. Let the direction of the beam constitute the x-axis of our system. The beam is collimated by a diaphram and it enters a magnet. The magnet produces a field that increases in intensity in the z direction which is also the direction of the magnetic field. As each atom (travelling along the x-axis) passes through the field it is deflected by a force proportional to the orbital magnetic dipole moment μ of the atom. If the orbital magnetic moment vector of the atom has magnitude μ, then in classical physics the z component μ_z of this quantity can have any value from $-\mu$ to $+\mu$. That's because classically an atom can have any orientation relative to the z-axis, and so this will also be true of its orbital angular momentum and its magnetic dipole moment. Thus the classical prediction is that the deflected beam would be spread into a continuous band, corresponding to a continuous distribution of values of μ_z from one atom to the next. For example, a silver atom with its magnetic moment directed horizontally would experience no force and would go straight past the magnet. An atom whose magnetic moment was exactly vertical would feel a force pulling it up along the z-axis. An atom whose magnetic moment was pointed downward would feel a downward push. The QM prediction is that the deflected beam would be split into several discrete components.

The magnet thus acts as a measuring device which investigates the quantization of the component of the magnetic dipole moment along the z-axis, which it defines by the direction in which its field increases in intensity. Stern and Gerlach found that the beam of silver atoms is split into two discrete components, one component being bent in the positive z direction and the other bent in the negative z direction. They also found that these results were obtained independent of the choice of z direction. (After Eisberg & Resnick 1974, pp. 296ff.)

[28] After Bohm 1951, pp. 594-5.

moment is not a correct model for the spin of a quantum entity. Instead, over the years a popular semi-classical model has developed, according to which the wave function Ψ is implicitly interpreted as follows: When an atom enters the Stern-Gerlach apparatus, it either goes 'spin up' and flies in the upper beam, or goes 'spin down' and flies in the lower beam. More technically, its interaction with the magnet results in the destruction of definite phase relations between eigenfunctions of the measured variable, or decoherence. Consequently, interference terms between the *z*-spin up and *z*-spin down wave functions can no longer contribute to the average of any function of the spin. For this reason, in obtaining averages of functions of the spin alone, the experimenter is able to ignore the apparatus coordinates, and assume that after measurement the spin wave function of any atom is either wholly $\psi^{\uparrow}(z)$ or wholly $\psi^{\downarrow}(z)$, the probabilities of each being given by $\left|a\psi^{\uparrow}\right|^2$ and $\left|b\psi^{\downarrow}\right|^2$. Thus, one has, in effect, located the 'von Neumann cut' (§1.4) at the magnet and replaced the actual wave function for the combined system by a statistical ensemble or mixture of pure states (see §1.3.2 for discussion of these concepts).[29]

No paradox or inconsistency can result since, as von Neumann showed, the measurement chain can be cut and the collapse inserted anywhere in the chain as far as final results are concerned. The correct quantum-mechanical description is always equivalent, at least for all practical purposes, to a classical *mixture* description, the same results for all measurable physical processes being predicted by both:

- the wave function $\Psi = \psi^{\uparrow}(z)\,e^{i\alpha_{\uparrow}} + \psi^{\downarrow}(z)\,e^{i\alpha_{\downarrow}}$ (i.e. the wave function of the system *following* the interaction of the system with the apparatus); and

- by a wave function that is *entirely* $\psi^{\uparrow}(z)\,e^{i\alpha_{\uparrow}}$ or *entirely* $\psi^{\downarrow}(z)\,e^{i\alpha_{\downarrow}}$, but with the respective probabilities $P(z^{\uparrow})$ and $P(z^{\downarrow})$ that each of these is the correct wave function of the system.

According to this standard textbook account, the fact that the final state of the system is represented in quantum mechanics by the composite wave function Ψ is interpreted as signifying nothing more than our lack of knowledge about which of the two possibilities is the case. When the observer finally looks at the apparatus, he or she then finds out in which of the two states, $\psi^{\uparrow}(z)\,e^{i\alpha_{\uparrow}}$ or $\psi^{\downarrow}(z)\,e^{i\alpha_{\downarrow}}$, the system actually is, by finding out in which of the two possible classically distinguishable states the observing apparatus is.

At this point the observer replaces the statistical ensemble of wave func-

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[29] For relevant discussion, see also Whitaker 1996, pp. 284-9; also Wigner 1967, pp. 159-64; also Belinfante 1975, pp. 24-37, 58-66.

tions of spin alone (i.e. the mixture, which replaced the combined spin and apparatus wave function, the latter being a pure wave function) by a single wave function corresponding to the actually observed value of the spin.[30] Because of the earlier destruction of interference (wave function collapse) upon the interaction of the atom with the magnet, this sudden replacement of the statistical ensemble by a single wave function represents no change in the state of spin of the atom, being merely analogous to the sudden change in classical probability functions which accompany an improvement in the observer's knowledge.[31]

However, Aerts & Reignier dismiss this kind of semi-classical interpretation out of hand, as '[of course] not true from first principles of quantum mechanics and also from the results of the Rauch experiments that one can translate *mutatis-mutandis* to the Stern-Gerlach case'.[32] That's right. Leggett, too dismisses it as a conceptual 'non-starter'.[33] Once again, the orthodox theorist needs to fall back on wave-particle duality with all its attendant features and puzzles.

The lesson of all these experiments is that not only EPR/Bell but also *single-particle* experiments demonstrate a quantum nonlocality in the standard interpretation, arising from the wavelike behaviour of unobserved matter, and subsequent collapse. The wave behaviour, in turn, arises from the quantization of matter, as already stated and as we saw in §1.1.

However, we also saw in §1.1 that the wave-like properties of a particle such as the electron are only *inferred* from its ability to exhibit interference-effects over wide regions of space. For this reason, it may be that there is no need after all to place undue emphasis on the wave aspect of matter. It is, as Bohm said, just the simplest workable hypothesis. It is rather the *interference* that is crucial, and the resulting non-classical probabilities of events. But before we can do away with the wave picture of matter, we need some *alternative* picture that can explain in a natural way the 'interference of possibilities' statistics that are characteristic of quantum mechanics – a picture that can *also* account for the new element introduced by EPR, namely the correlation of joint systems over space-like separations.

It now turns out, given Price's account of EPR/Bell, that there is no longer

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30 Bohm 1951, p. 604.

31 It is because definite phase relations between their wave functions have been destroyed that the different members of the statistical ensemble cannot interfere with each other, making possible the statistical ensemble interpretation. Bohm writes, 'If a sudden change of wave function occurred while definite phase relations still existed, then... quantum theory would make no sense at all.' (1951, p. 604) Bohm also cautions that the statistical ensemble of wave functions of spin alone is an idealization, which gives correct averages only when interference between the eigenfunctions of the spin has been destroyed by a measurement sufficiently good to ensure that there is no overlap between wave packets corresponding to positive and negative spin.

32 Aerts & Reignier 1991, p. 15.

33 Leggett 1987, p. 97.

any need to describe the unmeasured twin particles in that experiment as interfering waves. Indeed, the principle of Occam's razor would seem to forbid it. If 'the particles themselves "convey" the relevant influence to its common effect at the point at which they (the particles) separate', as Price claims, via a path that runs backward in time to the space-time locus where the pair originated, then the wave description of spin-half particles is a mere mathematical fiction.

The point of all of the foregoing is that if we can do away with the need for a wave-particle duality in the one type of experiment (EPR/Bell), it seems reasonable to believe we can also do away with it in the other type (two-slit/split-beam) – since in the standard Copenhagen account, wave-particle duality and the attendant superposition of waves and the consequent interference of possibilities is just the one feature that both experiments crucially have in common. That being so, the advanced action account shows promise of being able to show that the wave aspect of matter is ontologically a mathematical fiction. If so, an objective collapse of the wave function is a fiction, too.[34] It would follow that the measurement problem is an artefact of the theory. That, of course, would have implications for all aspects of quantum theory, just as Price claims.[35]

Price gives a nice illustration of how the standard interpretation's interference of quantum waves and the ensuing non-classical probabilities can in principle be explained in a much simpler way by advanced action. He refers to a classic philosophical discussion of the conceptual consequences of quantum superposition by Hilary Putnam in 1965.[36] Putnam notes that in quantum mechanics the probabilities of mutually exclusive possibilities do not add up in a classical way.[37] Price points out that advanced action can provide a very natural explanation of the kind of interference of possibilities statistics which Putnam describes. He illustrates this with a little story entitled 'The Case of the Precognitive Cat'. Owing to a failure of pre-interactive independence, Price's cat has foreknowledge of which of the two doors to his house he (Price) is likely to enter by when he returns home – even though his selection of door is random. As the day progresses and the cat becomes more and more hungry, she has an increasing tendency to place herself in front of the correct door (so as to greet him nicely). In the early afternoon, for example, there might be a 60% chance of finding her in front of *whichever one* of the two doors he happens to select. (The joint probabili-

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34 This seems to be true according to any hidden variable theory. Bell (1987, p. 205) makes an interesting point in connection with the GRW theory, though. Even though the theory contains no HV's, he remarks that we can propose the GRW jumps as the basis of the 'local beables' of the theory. The jumps are centred on a particular space-time point (x,t). They are the mathematical counterparts in the theory to real events at definite places and times in the real world.

35 Price 1996, p. 126.

36 Putnam 1979, pp. 130-58.

37 The mathematics of quantum waves is the same as for water waves, save that their amplitudes are complex rather than real, see Appendix(e).

ties sum to more than 100%.) The increasing probability of puss's choice of correct door as the day wears on corresponds to the inverse of the probabilistic 'discoupling factor' discussed in the preceding section.

Given the independence principle, such a state of affairs is not easy to explain in terms of a local picture. But if the principle is relaxed in the direction of the future (if the cat has pre-interactive knowledge of the correct door) then it is easy to give a classical interpretation of the probabilities. As Price says, they are nothing but conditional probabilities corresponding to our degree of ignorance of the cat's actual position under each of two mutually inconsistent hypotheses (namely that he will enter through door A or door B). Moreover, once the decision is made, then the probabilities are unconditional, and simply reflect our confidence that the cat is actually in front of the correct door.

Of course the story is a fable, and no mechanism for the influence of the future on the present is given save for failure of pre-interactive independence (if such mechanism be needed). But it shows how advanced action could in principle and in a consistent fashion, and *without bringing in waves*, explain the interference of possibilities that is so characteristic of quantum mechanics, and which is closely connected with the measurement problem. It also nicely illustrates the different ontological stances of the Copenhagen and local advanced action theorists. According to the Copenhagen interpretation, there never was any fact of the matter as to where the cat was until she was observed. According to the advanced action hypothesis, the probability is that the cat was already in front of the correct door even before the observation. Nor does the act of observation result in any disturbance or discontinuous change in the cat's position.

Despite the great promise of the advanced action interpretation, Price himself sometimes seems ambivalent about the wave description of matter being a mere mathematical fiction. For example, he has written that he is not interested in the wave/particle metaphors, but only in models which reproduce quantum mechanical predictions, without nonlocality, by invoking advanced action. 'Probably there will be a range of models, some more particle like and some more wave like, but giving the same predictions.'[38] Likewise, he has recently proposed thinking of the 'hidden' reality in terms of Feynman paths,[39] between an initial state (e.g. an electron being emitted by a source) and a final state (e.g.

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[38] Private correspondence, 10/11/99. However, to be fair, six months later he wrote, in reply to my criticism, '10/11/99 was last millennium'. (Private correspondence, 8/5/00.)

[39] The laws of motion can be formulated in terms of a 'least action' principle that makes no explicit reference to the notion of cause and effect, or the direction of time. It is simply one that minimises the difference between the kinetic and potential energy of the system. Famously, Richard Feynman used this principle in his paths-integral account of the interaction of light and matter, or quantum electrodynamics. See Feynman 1985. This book is a transcript of Feynman's lectures on quantum electrodynamics at UCLA around 1983.

detection of the electron at a particular point on the screen in a two-slit experiment). The idea is to 'think of the hidden reality as the instantiation not of one path rather than another but of one entire bundle rather than another, then the quantum mechanical probabilities can be thought of as classical probability distributions over such elements of reality!'[40] But it seems to me that the Feynman path-integral approach crucially depends on the assumption that spin-half particles are 'waves' – or fields, which comes to pretty much the same thing for the present purposes, since a field is something which varies from place to place and time to time – which automatically gives it wave-like properties[41]. As Herbert points out, the mathematics of summing up the paths in that picture depend on the notion of phase and interference (Huyghen's principle) – which implies something like a wave-picture.[42] Bohm & Peat, too, note that Feynman diagrams presuppose the wave picture, representing only the contributions of all different wavelets, which may produce constructive or destructive interference effects, and for that reason cannot represent the actual trajectories (in the classical sense) of particles. 'Since the electron is not only a wave but also has a particle nature, the Feynman diagrams cannot provide an adequate image of the actual movement [of an electron] from whichever standpoint they are regarded'.[43] Instead, each Feynman diagram corresponds to a mathematical formula.

Price partially recognizes this problem when he says that 'the model does not fully restore a classical picture of reality, because it does not assign an individual classical trajectory, but only membership of a bundle of trajectories'. Even so, his reference to the bundle of Feynman paths as a bundle of 'trajectories' remains problematic, because if they are trajectories, they are unlike any classical trajectories of particles of matter because they mutually interfere – behaving instead in that regard like a lot of waves. Better usage is that of Herbert, who writes that the approach 'suggests that the wave function represents the totality of possibilities – *plus mutual phases* – open to a quantum entity' (my emphasis).[44] But if so, it is difficult to see how the paths-integral approach to considering quantum mechanical probabilities represents, ontologically, an advance on the standard quantum mechanical rule for calculating the probability of compound events, save perhaps by being more suggestive of an underlying 'hidden reality'. After all, summing over all paths gives the same wave function as solving Schrödinger's equation. The end-result of either procedure is to be interpreted as referring to an ensemble of measurement results of identically prepared systems. Feynman himself observed, in the context of his own space-time view of quantum electrodynamics ('QED'), that the paths-integral formulation was useful, but,

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40 Price 2001, §7.3.

41 As Polkinghorne points out (1979, p. 74).

42 Herbert 1985, p. 117.

43 Bohm & Peat 1987, p. 178.

44 Herbert 1985, p. 117.

strictly speaking, not necessary.[45] The paths-integral approach also suffers from a serious limitation, in that it does not permit a simple representation of electron spin, even though spin is a 'simple and vital part of real quantum-mechanical systems'.[46]

Price doesn't subscribe to what he regards as the Copenhagen interpretation's unintelligible notion of 'complementarity', or wave-particle duality. Yet, as we have seen, he himself sometimes speaks as if he were advocating models combining both wave-particle duality (in effect, something like the standard interpretation) and advanced action (where advanced action is just what makes possible the 'quantum mechanics is incomplete' interpretation). Moreover, it seems that such a hybrid approach too readily gives up the main advantage of his own advanced action proposal. That advantage is the prospect of having a relatively simple Einsteinian realist theory with locality. We've seen that the only reason 'wave' talk was introduced into quantum mechanics was because of the apparent nonlocality and interference properties exhibited by quantum mechanical systems, e.g. in the two-slit experiment. But it seems that these can be accounted for in principle by advanced action. At least, that is the goal. So why opt for a 'Tycho Brahe' solution, with vague talk of both wave-like & particle-like models? The very terminology, 'wave-like' & 'particle-like' presupposes the standard interpretation of quantum mechanics, and its notion of complementarity. Why implicitly subscribe to the picture to be discarded, when one has the basis of the solution that does away with that picture?

Elsewhere, Price would seem to agree. In his review of L.S. Schulman's 1997 book, *Time's Arrows and Quantum Measurement*, Price writes:

> As Einstein, Schrödinger, and others saw, a big advantage of the idea that the wave function is an incomplete statistical description is that it makes 'collapse' unproblematic – a mere change of information, rather than a physical event. In running together the ensemble view and the Copenhagen ('quantum mechanics is complete') interpretation, it may seem that one has the advantages of both: an easy answer to the measurement problem, and no messy hidden variables to worry about. But of course this advantage is achieved at the cost of a much greater problem – 'inconsistency', as we philosophers call it.[47]

[45] Feynman 1987. One reason for Feynman's remark is that his propagator theory and Dirac's hole theory are mathematically equivalent (though of course different in their interpretation of the maths). The mathematical equivalence is why Feynman said that even the idea of a positron being a backward moving electron wasn't strictly necessary for his theory of QED – though undoubtedly convenient.

[46] Feynman & Hibbs 1965, p. 355.

[47] Price 1998, p. 525.

5.1.3 Price's proposal & the QM wave function

Given Price's proposal, which seems to entail that the wave aspect of particles is a mathematical fiction, it follows that the quantum wave function, too, is a mathematical fiction, no different in principle from the life expectancy function of insurance statistics. That is, given advanced action, the wave function turns out to be, after all, just a 'classical' probability function describing an ensemble of systems in different determinate, albeit hidden states. In both the classical and quantum-mechanical cases, probability enters into the picture only because of our ignorance of the complete state of the physical system. In the case of *quantum* statistics though – in contrast to classical insurance statistics – to know the complete state of the system, we would need to know not only its history but also its *fate*.

In the above regard, it's helpful to bear in mind that even in the standard interpretation of quantum mechanics, the wave function is, as Wigner puts it, 'only a shorthand expression of that part of our information concerning the past of the system which is relevant for predicting (as far as possible) the future behaviour thereof.'[48] But given a local realist theory utilizing advanced action, that's just the root cause of all the difficulties: the entire problem arises just because the mathematical object known as the wave function encodes relevant information *only about the past* of the system. For the wave function to be a complete description, it would need to encode the relevant information about not only the past but also the *future* of the system. This has a bearing on the time-reversal invariance of the laws of quantum mechanics. Price writes, 'I rely on the fact that the asymmetry of the state function is unproblematic, if it is simply an incomplete description.'[49]

Price has recently emphasized that the crucial interpretational issue is that of the *completeness* or otherwise of quantum theory, *not* whether the wave function is real.[50] That is true. However, it needs to be added, I think, that in quantum mechanics the wave function plays the crucial role that it does because of wave-particle duality. If we want to predict anything in quantum mechanics, we need to go to the *wave*-description of matter (§1.1). Do away with the reality of the waves and you thereby do away with the reality of the wave function. We may continue to make *use* of the wave function – but it isn't real.

Another way of making the above point (that a natural consequence of the advanced action interpretation is that the quantum wave function is not only incomplete but also a mathematical fiction describing an ensemble of systems in different hidden but *determinate* states) is as follows. Since the probabilistic

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48 Wigner 1967, p. 166. (Also in Wheeler & Zurek 1983, p. 337.)

49 Price 1996, p. 283, n. 22.

50 Price 2001, §4.

nature of quantum mechanics arises from Heisenberg's indeterminacy principle, which itself arises, according to the standard interpretation, from wave-particle duality or complementarity, and since the advanced action account appears to do away with wave-particle duality as an *irreducible* element of reality, then it is hard to see how the ignorance interpretation of the wave function can be objected to any longer. After all, it is left as the simplest and the most natural interpretation consistent with the predictions of the quantum formalism. Somehow, the details of the interference effects characteristic of quantum systems must fall out of this interpretation. We may expect, then, that in a future local realist theory based on advanced action, the theory's account of both the entanglement and the quantum measurement rules will be found to be closely related, as parts of a single theoretical story, a story relying crucially on advanced action.[51]

Thus the advanced action interpretation of quantum mechanics shows promise of being able to show just in what way the quantum-mechanical description of physical reality is incomplete, and how the world can indeed be analysed in terms of distinct and separately existing elements of reality while retaining special relativity. In short, as Price remarks, it seems to offer the usual advantages of the incompleteness interpretation favoured by Einstein, Schrödinger, de Broglie, Bohm, Bell... These advantages might be expected to be offset by disadvantages appearing elsewhere in the picture, he adds, but that doesn't seem to be the case.[52] On the contrary, writes Price,

> this path to quantum theory [advanced action] removes the main obstacles to a much more classical view of quantum mechanics than is usually thought to be possible. It seems to solve the problem of nonlocality, for example, and to open the door to the kind of interpretation of quantum theory that Einstein always favoured: a view in which there is still an objective world out there, and no mysterious role for observers.[53]

5.1.4 Price's proposal, Planck's constant & the Heisenberg indeterminacy relations

We saw in Chapter 1 that according to the standard interpretation of quantum mechanics, the reason we cannot predict the motions of individual particles is because we cannot have full knowledge of the initial conditions required for such prediction, even in principle. The reason we can't have full knowledge of such conditions, according to the standard story, is because the requisite details do not exist, owing to wave-particle duality. The indeterminacy relations are easy to derive given such duality (see Appendix[h]).

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[51] Or on some other type of theoretical story that is at least translatable into an advanced action one.

[52] Price 1996, p. 250.

[53] Price 1996, pp. 8-9.

The remarkable thing about the advanced action account sketched above is that it enables us to understand in principle, in an intuitively satisfying way, *without recourse to waves* or wave-particle duality, or to the operator formalism of quantum theory, why we cannot have full knowledge of the required initial conditions. It is not that a full set of determinants of the system's future state doesn't exist. It is rather that the quantum theory always operates with an incomplete description state. (How could it be otherwise when the wave function encodes relevant information only about the past of the system, but the initial conditions also depend on the future of the system?) Take probability in classical mechanics. Given that we know the relevant laws, the use of probability in classical mechanics is necessary only to take account of our ignorance of the boundary conditions, the boundary conditions in question being of course those that are conventionally known as the *past* boundary conditions (also known as the initial conditions). Analogously, it appears that Planck's constant h, which plays such a central role in the indeterminacy relations, is, in the context of measurement of the state of a system, a measure of our in-principle ignorance of the *future* boundary conditions determining the state of the system – which can also act as determinants of the 'initial' state of the system. Equally and alternatively, h seems to be a measure of our in-principle ignorance of the *non*-accessible past boundary conditions relevant to determining the state of the system – and so a measure of Dummett's loophole. For greater predictability up to and including full classical predictability we would need to know not only the accessible past boundary conditions – available through an ideal measurement – but also the *inaccessible* past boundary conditions, which depend on the relevant *future* boundary conditions. The difference between *complete* predictability and the level of predictability *actually achieved* in our ideal measurement is just the gap between the past and the accessible past. This difference is given by h.

Consider the spin state of a photon approaching a polarizer. According to the advanced action view, the orientation of the polarizer when the photon reaches the polarizer is a determinant of the photon's spin *before* it reaches the polarizer. Thus, even though it may be hypothesized that there is a sense in which the photon has a determinate state of spin at all times, that state is unknowable even in principle, just as the indeterminacy relations assert, because that future interaction has *yet to take place*. Here is how Price characterizes the ensuing situation in terms of its consequences for the quantum mechanical formalism:

> Roughly speaking, we might say that quantum mechanics represents an idealized codification of all the information about a system available to an observer who is herself embedded in time, in virtue of interactions between the system and the world in that observer's past. Thus it is a complete descrip-

tion from that standpoint, though an incomplete one to God.[54]

Only God could see both the history and the fate of the photon (in the absence of other suitable Archimedean observers).

Analogous considerations apply regarding the state of Schrödinger's cat.

Moreover, given that h, appearing in the indeterminacy relations, is in some not very well understood way a measure of our ignorance of the relevant future boundary conditions pertaining to a measurement of the state of a system, we can reasonably expect the details of h to fall out of a more fully developed advanced action model in terms of quantities more fundamental to the model, as already mentioned.

It is evident from the foregoing that the advanced action interpretation of quantum mechanics does not entail that the indeterminacy relations are *wrong*. Thus, the future development of a local realist theory based on advanced action will not have the consequence that the indeterminacy relations or the quantum-mechanical formalism itself will just wither away. The mathematics of the (non-relativistic) advanced action interpretation remain the same as in the standard interpretation, as Price himself notes,[55] with only the explanatory story behind the mathematics being different.

Indeed, in 1949 Einstein himself remarked that, from his own point of view, the correctness of the Heisenberg indeterminacy relations is 'finally demonstrated'. He also fully recognized 'the very important progress which the statistical quantum theory has brought to theoretical physics', writing:

> This theory is until now the only one which unites the corpuscular and undulatory dual character of matter in a logically satisfactory fashion; and the (testable) relations, which are contained in it, are, within the natural limits fixed by the indeterminacy relations, *complete*. The formal relations which are given in this theory – i.e., its entire mathematical formalism – will probably have to be contained, in the form of logical inferences, in every useful future theory.[56]

5.2 Heuristic adequacy of Price's proposal

In the last chapter and the preceding sections of the present chapter, we have taken a close look at the main elements of Price's proposal for interpreting quantum mechanics. In this regard, Price's various publications have painstakingly peeled back, layer by layer, the obscuring complexities of the interpretative problem to expose its core. So far we have largely concentrated on the strengths

54 Price 1996, p. 259.
55 Price 1994, p. 304.
56 Einstein 1949b, pp. 666-7.

of the proposal, in part because the strengths are so manifest. We've noted that the proposal represents a fundamental advance. On the debit side, we've briefly noted that the proposal so far is in the nature of a general strategy only. Price has little to say, for example, about the derivation of the quantum-mechanical measurement rules, or indeed about most of the main features of quantum mechanics. It is now time to take a critical look at some other aspects of the proposal that give rise to unease.

There are three such aspects that we will mention here. None are objections against advanced action *per se*, or even specifically against Price's proposal. They are rather attempts at clarifying certain aspects of the proposal that remain so far largely unexplored in Price's writings, and highlighting potential problem areas, having regard to the future development of the proposal. The first is Price's claim that the advanced action proposal doesn't conflict with realist intuitions of Einstein's kind, even though the proposal seems to entail, in effect, that the Heisenberg indeterminacy relations remain the last word in physics as far as predictability goes. If so, nothing would seem to change in physics, and that is surely not the outcome Einstein had in mind. The second is to do with the heuristic adequacy of the proposal owing to its being non-relativistic. The proposal has so far very largely concerned itself only with non-relativistic quantum mechanics. To realize its full potential, however, there is little doubt that the proposal will need to be extended to the non-relativistic sphere. And the third aspect giving rise to unease raises the question of whether the proposal entails that only some causation is backward, or whether half of all causation is so. We'll begin with the question of whether Price's advanced action proposal accords with Einstein's realist intuitions.

5.2.1 Does Price's advanced action proposal accord with realist intuitions of Einstein's kind?

We have seen that by 1949 Einstein himself believed that the correctness of the Heisenberg indeterminacy relations was 'finally demonstrated'. The *sole* difference between Einstein and Bohr regarding the interpretation of the quantum mechanics was that Einstein believed, in contradistinction to Bohr, that the essentially statistical character of quantum theory is to be ascribed simply to the fact that it 'operates with an incomplete description of physical systems'.[57] In 1949 he wrote:

> Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly con-

57 Einstein 1949b, p. 666.

vinced that the development of theoretical physics will be of this type.[58]

Moreover,

> It is... not at all surprising that, by using an incomplete description, (in the main) only statistical statements can be obtained out of such a description. If it should be possible to move forward to a complete description, it is likely that the laws would represent relations among all the conceptual elements of this description which, *per se*, have nothing to do with statistics.[59]

A problem now arises. Einstein's above remarks above and elsewhere suggest that attendant on the eventual success of the search for a complete physical description, he expected to recover full predictability in principle, even if perhaps not in practice. For it seems that only in that way could the statistical quantum theory 'take an approximately analogous position to the statistical mechanics within the framework of classical mechanics'. It is true, as we've seen, that Einstein also wrote that the correctness of Heisenberg's indeterminacy relations had been 'finally demonstrated'. But that is not inconsistent with recovering predictability. Even in classical physics, as Bohm points out, there exists an indeterminacy principle, the form of which is the same as that of Heisenberg.[60] Suppose that we observe a moving smoke particle subject to random fluctuations owing to collisions with the atoms that exist at a lower level (Brownian motion), over some short interval of time Δt. We will find random fluctuations of magnitude ΔX in the mean position, and of magnitude ΔP in its mean momentum, which satisfy the relationship $\Delta P \times \Delta X \approx C$, where C is a constant depending on the temperature of the gas and other properties such as viscosity. The form of the relationship between $\Delta P \times \Delta X \approx C$ is just the same as that of Heisenberg, except that Planck's constant h is replaced by the constant C, which depends on the state of the gas (i.e. on 'hidden' variables).[61]

In an analogous way, as Bohm points out, it is possible to postulate a sub-quantum mechanical level containing hidden variables, such that the statistical

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58 Einstein 1949b, p. 672.

59 Einstein 1949b, p. 673.

60 Bohm 1957, pp. 107-9.

61 'Basically', writes Bohm, 'this relationship comes from the formula $(\Delta x)^2 = a\Delta t$ for the mean square of the distance moved by the particle in its random motions during the time, Δt. Thus we have for the root mean square fluctuation in the momentum (assuming zero mean velocity to simplify the argument)

$$\left[\left(\frac{\Delta x^2}{\Delta t}\right)\right]^{\frac{1}{2}} = a^{\frac{1}{2}}(\Delta t)^{\frac{1}{2}}.$$

Then, with $\Delta X = \left[\overline{(\Delta x)^2}\right]^{\frac{1}{2}}$, we get $\Delta X \Delta P = ma = C$.' (Bohm 1957, p. 107.)

character and the indeterminacy of the present quantum theory arises owing to the random fluctuation of new kinds of entities, existing on the lower level. We may hypothesize new kinds of physical processes which depend significantly on the details of what is going on at the sub-quantum level – and in that way hope to go beyond the limits set by Heisenberg's principle. However, Bohm cautions,

> If we consider only those entities which can be defined at the quantum-mechanical level alone, these will be subjected to a genuine indeterminacy in their motions, because the determining factors that are important (i.e. the hidden variables) simply cannot be defined in this level.[62]

Hence the existence of indeterminacy would remain an 'objective necessity' on the quantum-mechanical level, just as quantum theory asserts.

> Thus it is not the existence of indetermination and the need for a statistical theory that distinguishes our point of view from the usual one. For these features are common to both points of view. The key difference is that we regard this particular kind of indeterminacy and the need for this particular kind of statistical treatment as something that exists only within the context of the quantum-mechanical level, so that by broadening the context we may diminish the indeterminacy below the limits set by Heisenberg's principle.[63]

As we saw in §1.3.3, Bell has even coined a name that can be applied to the hypothetical unobservable elements of a realist description: 'beables' (as contrasted with 'observables', and short for 'maybe-ables'). The beables of a theory are those elements which might correspond to elements of reality – to things which exist – quite independently of observation. Observation and observers themselves must be made out of beables. The point is to emphasize the essentially tentative nature of any physical theory, and to give free play to Bell's own realist inclinations.[64]

Einstein's hope was that the laws of quantum mechanics express merely the statistical result of the development of completely determined values of variables presently hidden from us – the quantum-mechanical description being a description of an *ensemble* of systems in different states. Recovering predictability in principle then becomes a possibility.

But the advanced action proposal appears to entail that we can never have full predictability, even in principle, because for that we would first need to know the future – the very thing we are trying to get with complete predictability in principle! (This is why Price can safely assert not only that we cannot hope to

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62 Bohm 1957, p. 106.
63 Bohm 1957, p. 106.
64 Bell 1987, p. 174.

observe advanced action directly [as it were],[65] but also that the mathematics of the advanced action proposal and Bohr's reply to EPR remain the same.[66])

The upshot seems to be that the proponents of the standard interpretation of quantum theory were right when they claimed that Heisenberg's indeterminacy principle represented 'an absolute and final limitation on our ability to define the state of things by means of measurements of any kinds that are now possible or that ever will be possible', to use David Bohm's words.

Such a position would have far-reaching consequences, as Bohm goes on to explain:

> For even if a sub-quantum mechanical level containing 'hidden' variables... should exist, these variables would then never play any real rôle in the prediction of any possible kind of experimental results. In fact, if this hypothesis is true, the future behaviour of a system would, at least as far as *we* are concerned, be predictable to just that degree of accuracy corresponding to the limits set by the indeterminacy principle , and to no higher degree. Thus, it is concluded that the present general form of the quantum theory is able to deal with every kind of measurement that we could possibly carry out. Any theory (such as one involving 'hidden' variables) which claims to deal with more than this would then be just a metaphysical exercise of the imagination, because nothing in physics would be different if these 'hidden' variables did not exist.[67]

When Bohm wrote this, he no longer believed that the indeterminacy relations were the last word. He is only describing what the consequences would be if it turned out that they *were* the last word, as claimed by proponents of the standard interpretation. Now Price has come along and (convincingly) proposed, in effect, that they *are* the last word.

At the same time, Price claims that the advanced action proposal doesn't seem to conflict with realist intuitions of Einstein's kind. On the contrary, according to him it seems to offer the usual advantages of the incompleteness interpretation favoured by Einstein, Schrödinger, de Broglie, Bohm and Bell. These advantages might be expected to be offset by disadvantages appearing elsewhere in the picture, he writes, but that doesn't seem to be the case.[68]

It is true that the advanced action proposal doesn't conflict with realist intuitions of Einstein's kind, as far as the realism *per se* is concerned. There seems no problem, as Price says, in extending to the *past* a form of dependence that realists find unproblematic in the case of the future. But it does seem that the

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65 Price 1996, p. 256.
66 Price 1994, p. 304.
67 Bohm 1957, p. 85.
68 Price 1996, p. 250.

proposal conflicts with realist intuitions of Einstein's kind in another way – in that it appears to rule out once and for all predictability in principle. Although Einstein was motivated by realistic considerations, rather than deterministic ones, surely such a consequence would have been at least *unexpected* by him (even if perhaps ultimately acceptable).

Indeed, Price himself has recently said as much, writing that

> hidden variable theorists may find themselves agreeing with Bohr that QM is complete in the sense of completeness which Einstein shares with Bohr [i.e. in that the QM wave function encodes all the 'potentiality' that could be derived from a complete knowledge of a system's past interactions with the rest of the world]; while at the same time maintaining that it is incomplete in another important sense, which neither Einstein nor Bohr seems to have entertained.[69]

It is incomplete in that it not only fails to tell us whether or not some particular future event will happen (it is incomplete from the Archimedean standpoint – which has always been the recognized issue between Einstein and Bohr), but it is *also* massively Archimedean-redundant, in giving us a lot of information about a wide range of possible futures – information that is irrelevant in the context of the particular piece of information we want (such information being consistent with what will be found to be the case if we wait long enough to see whether the event in question will actually occur).

There also remains the question implicit in the quoted passage from Bohm, namely: of what possible *use* is a physical theory which has the consequence that nothing in physics would be different even if the variables proposed by the theory did not exist?

There is little reason, however, to think that nothing new can come from a fresh point of view just because it is mathematically equivalent to its predecessor. Feynman, for one, was struck by the large number of different physical viewpoints and widely different mathematical formulations that are all equivalent to one another – describing the same physical reality. Yet the theories aren't, as he put it, psychologically identical. 'For different views suggest different kinds of modifications which might be made, and hence are not equivalent in the hypotheses one generates from them in one's attempts to understand what is not yet understood.'[70] In other words, the theories are not identical in their *heuristic potential*.

That is right. I conclude that even though the advanced action proposal contains an unexpected drawback from Einstein's point of view, its realism is as

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[69] Price 2001, §4.
[70] Feynman 1987, pp. 454-5.

good as it gets. We now move on to the second of the three aspects of the proposal giving rise to unease.

5.2.2 Relativistic considerations

The second aspect of Price's proposal giving rise to unease is that it is non-relativistic. Price has so far confined himself to an explication of the puzzles raised by the standard Copenhagen interpretation. As regards EPR/Bell, for example, Price says that the mathematics of the advanced action proposal and Bohr's reply to EPR remain the same. But that maths is the maths of the standard interpretation – which is *wrong* except in the unphysical non-relativistic limit (unphysical because at the limit the speed of light would have to be infinite). For that reason, it would seem, now that the groundwork is done, that Price ought to turn to interpreting some relativistically covariant version of Schrödinger's equation rather than Schrödinger's equation itself – such as Dirac's relativistic wave equation.[71] Dirac's wave equation has *two* classes of solutions to be interpreted: the (+) & (–) energy ones. The two classes of solutions of Dirac's equation (and all relativistic wave equations) would seem to be of importance for Price's project for two reasons:

First, because Price wants to go to more basic considerations in order to reinterpret existing quantum theory. In his case, that means reinterpreting the present quantum-mechanical formalism in terms of advanced action. Now, when one goes to a relativistically *correct* version of the formalism (as opposed to the incorrect non-relativistic formalism), entirely new features are thrown up, which aren't simply additional detail or gloss on the non-relativistic formalism. Instead, they *change the existing picture in radical and unexpected ways*. For example, there is not just a doubling up of solutions in the relativistic formalism – which doubling up itself is central in modern quantum field theory – but it also appears that no quantum wave equation lacking *both* classes of solutions is able to do justice to even the most obvious of the known facts – such as the existence of antiparticles – to say nothing of more esoteric facts such as the existence of quantum spin. Doing justice to the relativistic formalism is not merely a matter of adding backward causation to each of the two classes of solutions.

Even the interpretational problem of nonlocality, which is so central to

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71 Schrödinger's theory of quantum mechanics is an approximation which ignores relativistic effects. It is based on the non-relativistic energy equation, $E = p^2 / 2m + V$. Dirac used the same postulates as the Schrödinger theory but he replaced the energy equation by its relativistic form $E = \left(c^2 p^2 + m_o{}^2 c^4\right)^{\frac{1}{2}} + V$, and derived his relativistically covariant version of Schrödinger's equation possessing negative energy solutions, which predicted electron spin, and showed that the spin is intimately connected with relativity. (After Eisberg & Resnick 1974, p. 302.)

Price's project, is essentially a *relativistic* problem, as Cramer rightly points out.[72] If the speed of light were infinite, it wouldn't exist. That's because there'd be no quantum mechanics to create the problem of nonlocality in the first place. Thus, quantum mechanics, relativity and nonlocality are all inextricably connected.

In short, why reinterpret the formalism that is only correct in the unphysical limit (i.e. if we falsely assume that the speed of light is infinite) when you can reinterpret the physically realistic and more significant relativistically covariant formalism?

Second, the goal is to come up with an advanced action *theory* of quantum mechanics – not just a general strategy. For the purposes of developing a proper theory, going to the relativistic formalism seems essential. After all, the non-relativistic formalism is simply wrong, in just the same kind of way that the Newtonian inverse-square law of gravitation is wrong. To be sure, the Newtonian formula works for most mundane purposes, and even for sending a spacecraft to the moons of Saturn. But the underlying theoretical conceptions of the non-relativistic Newtonian and the relativistic Einsteinian accounts of gravitation are very different.[73] Likewise, some of the theoretical conceptions of non-relativistic and relativistic quantum mechanics (quantum field theory) are very different, the latter changing the picture in radical ways. For example: (a) the momentum and energy operators of non-relativistic quantum mechanics cannot be interpreted as observables with the same degree of generality as the operators of non-relativistic quantum mechanics;[74] (b) there is pair-production and annihilation, with the consequence that the number of particles of any particular kind are not conserved; their number need not even be definite, as linear superpositions of states with different numbers of particles are allowed;[75] (c) there is polarization of the vacuum and virtual particle-pair creation and annihilation; (d) there is spin – now explained as an essentially *relativistic* effect. Spin manifests itself in current fluctuations and is 'tied up with virtual pair fluctuations in regions of the order of m^{-1}'.[76]

Of course, it may be that working on the non-relativistic picture does not preclude the possibility of later relativistic refinement. However, as Cramer warns, the 'Copenhagen interpretation was developed specifically for interpreting the non-relativistic Schrödinger formalism. The structure of Newtonian space-

72 Cramer 1988, p. 234.

73 General relativity doesn't describe gravity in terms of a *force*, but curvature of space-time. Of course, we may still continue using the Newtonian expression 'force', but only so long as we recognize that the expression refers to 'the discrepancy between the natural geometry of a coordinate system and the abstract geometry arbitrarily ascribed to it', as Eddington puts it (1924, p. 38).

74 Dirac 1935, p. 252.

75 Penrose 1989, p. 289.

76 Thirring 1958, 'Fluctuation phenomena', pp. 77-86.

time is deeply embedded in its approach, perhaps inextricably so'.[77] Imagine Einstein deliberately limiting himself to the Newtonian conception of space and time in order to come up with a theory of gravitation. Or, for that matter, imagine a modern cosmologist who chooses to limit herself to the Newtonian theory of gravitation to investigate the large-scale structure of the universe when general relativity is already to hand.[78]

There seems little doubt that for both heuristic adequacy and completeness (to achieve a fully 'Archimedean' or 'God's eye' perspective theory), Price needs to go to the relativistic version of quantum mechanics. Price himself has recently written that he suspects that 'quantum field theory [which is a relativistic theory of quantum electrodynamics] already has everything we need to do it generally [i.e. to achieve a fully Archimedean theory of proper heuristic potential], bar the necessary interpretation'.[79]

Let us take a brief look at how the relativistic picture might open up the interpretative possibilities as regards the correlational structure of the world. Price has argued that recognition of the subjectivity of causal asymmetry does not preclude objective bidirectional causation. That's because we can always step more fully outside the anthropocentric viewpoint, in which case we need not talk of a time direction at all. Instead, we need to distinguish between two kinds of correlational structures for the world. From the anthropocentric standpoint of the observer embedded in time, one looks as if it simply contains unidirectional causation, while the other looks as if it contains bidirectional causation. ('Looks as if' is to be filled out in terms of the perspectival account of causation.[80]) According to Price, it is an empirical matter whether or not the correlational structure of the microworld is of the latter kind. Bell's theorem suggests that the correlational structure of the world is indeed of that kind: '...quantum mechanics shows that what is in the world is simply a particular pattern of correlations (a

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77 Cramer 1988, pp. 233-4.

78 That wouldn't be *quite* as silly as it sounds. Milne tried to do something like that in his theory of kinematic relativity, except that he didn't even presuppose Newtonian gravity. Whereas theories such as general relativity encourage attempts to describe the universe in terms of the known theory (e.g. general relativity), kinematic relativity reverses that procedure, the idea being to deduce as much as possible from basic principles alone, in particular from the cosmological principle, together with the basic properties of space and time and the propagation of light (and to slip into the basic premises as little as possible of what one is trying to deduce). Milne managed to 'derive' the fundamental laws of Newtonian dynamics and other interesting results from the structure of the substratum. But the consensus is that he fudged his premises. For a discussion, see Bondi 1968, Ch. XI; Singh 1970, Ch. 8. For completeness, it should also be mentioned that in 1971 Fred Hoyle and Jayant Narlikar managed to formulate a Machian theory of gravitation which uses only the concepts of special relativity, and which had some notable advantages.

79 Private correspondence, 8/5/00.

80 Price 1996, pp. 193-4; Price 1994, p. 322.

pattern that classical physics really had no business to exclude a priori)'.[81]

Fine. But what are we to make of this 'correlational structure'? It is hard to see how Price can possibly go any further in the non-relativistic formalism than saying, as he does, that the correlational structure of the world *just is* of the kind revealed by quantum mechanics (when quantum mechanics is interpreted in the light of the advanced action proposal) – which amounts to an explanatory dead end.[82] Of course, the advanced action proposal itself, quite independently of any talk of 'correlational structure', has already opened up the 'explanatory dead end' of the Copenhagen interpretation. But it seems to me that one needn't stop there.

There is a noteworthy feature of the relativistic picture of the world which may provide a clue toward a more detailed description of the correlational structure in question. The formalism of both relativistic cosmology and relativistic quantum theory exhibit a mysterious and ubiquitous 'doubling-up' of the world. For example, in the spherical universe model of general relativity, to every event 0 there corresponds an identical event 0' at it's antipodal point, with the consequence that the universe is a mirror universe. (An alternative interpretation of the formalism [of the differential geometry] is that the universe is an 'elliptical' universe, meaning that every pair of antipodal events is the same event, connected by a Möbius twist in the fourth dimension. In this interpretation the universe has the same kind of connectivity as a Klein bottle. Both the spherical and elliptical universes have the same metric or differential geometry but a different topology.[83]) There is also a doubling up of 'worlds' in the Kruskal extension of the Schwarzschild manifold, with each of the two halves of a Kruskal diagram representing all of Schwarzschild space – which in the limit becomes all of Minkowski space. Likewise, the surface of the de Sitter manifold, dividing the world into two 'halves' is the analogue of the Kruskal diagram of a black hole, being related to the de Sitter metric much as Kruskal's diagram is related to the Schwarzschild metric.[84]

In quantum theory, too, there seems to be a genuine duplication of the world lurking behind the paired positive and negative energy solutions of Dirac's relativistic covariant version of Schrödinger's equation.[85] Dirac's equation (like the

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81 Price 1994, p. 306n.

82 One is reminded of d'Espagnat's remark (1979, p. 139): 'One can imagine a physics grounded on positivist principles that would predict all possible correlations of events and still leave the world totally incomprehensible.' I'm not suggesting that there is anything positivist in Price's proposal, though – merely that more needs to be said.

83 Infeld 1949, pp. 141-2. See also Eddington 1924, pp. 157-9.

84 Rindler 1977, p. 186.

85 Though, to be sure, in quantum-mechanical field theory, the negative-energy solutions are associated with antiparticles such as positrons, and there is no explicit duplication. But quantum-mechanical field theory is not without its own serious interpretative problems. The main problems are outlined in Feynman 1987 and Schwinger 1987.

relativistic Klein-Gordon equation and the electromagnetic wave equation) has both retarded and advanced solutions, with the advanced solutions possessing negative-energy eigenvalues.

Schrödinger's equation has the form

$$-\left(\frac{\hbar^2}{2m}\right)\nabla^2 \psi = i\hbar \frac{\partial}{\partial t}\psi$$

where m is the mass of the particle described by the equation, $-\hbar^2\nabla^2 \equiv \hat{p}^2$ is the square of the momentum operator $\left(\frac{\hbar}{i}\right)\nabla$, and $i\hbar\frac{\partial}{\partial t} \equiv \hat{H}$ is the total energy operator. Schrödinger's equation states that $\hat{H}\Psi$ describes how the quantum wave function evolves. Because Schrödinger's equation is first order in the time variables, it doesn't have advanced solutions. But of course Schrödinger's equation isn't physically correct – because it isn't relativistically invariant. It is correct only in the unphysical non-relativistic limit, at which the speed of light would have to be infinite. For this reason it ought to be regarded as only a limiting case of some more physically realistic relativistically invariant wave equation. Cramer points out that when a wave equation that *is* relativistically invariant is taken (such as Dirac's equation or the Klein-Gordon equation) and it's reduced to the Schrödinger equation by taking its non-relativistic limit, the procedure results in *two* equations, the Schrödinger equation and another equation of the form

$$-\left(\frac{\hbar^2}{2m}\right)\nabla^2 \psi = -i\hbar \frac{\partial}{\partial t}\psi,$$

which is the complex conjugate of the Schrödinger equation. It has only advanced solutions and negative-energy eigenvalues.[86] However, this second equation is usually discarded because of its negative-energy eigenvalues. But the fact remains that both equations represent equally valid solutions of the dynamics that underlie Schrödinger's equation, negative-energy eigenfunctions notwithstanding. (Schrödinger's equation itself may be regarded as just the limiting case of a relativistically invariant wave equation when the velocity of light goes to infinity.)

In each of the above cases the 'doubling up' of things arises from going to a *relativistic* picture, as opposed to a non-relativistic picture (necessitated by the fact that the non-relativistic picture is simply wrong, save trivially in the non-relativistic limit). Are we simply to ignore this aspect of the formalism, which arises whenever we go to a picture that takes proper account of relativity, i.e. a picture that is *not wrong*, at least in that respect? Or do we take it seriously and see where it might lead us? Although there is an understandable tendency to

86 Cramer 1986, p. 663.

discard as 'unphysical' any unwanted half of a symmetrical solution, the lesson of Dirac and antiparticles (and of the 'unreasonable effectiveness of mathematics in the natural sciences' more generally)[87] is that the discarded solution may only *look* unphysical to us because of our local, anthropocentric preconceptions, and our familiarity with the earlier, non-symmetrical, non-relativistic picture. It may be that more is really going on than the earlier picture allows, containing as it does only one half of the 'stuff' of the fuller picture (whence the present interpretative impasse). The symmetry of the solution may be indicative of processes occurring on a 'deeper' level of reality. (A suggestion regarding the possible nature of such processes will be made in the next chapter.)

5.2.3 The proportion of backward to forward causation

Now I'd like to raise another possible problem with Price's account of the advanced action proposal, the last of the three aspects giving rise to the unease mentioned at the beginning of the present section. It concerns a tension and possible inconsistency in some of the things Price says about elements of the proposal. The problem is twofold. First, upon analysis, the proposal seems to require that no emission can take place in the absence of a subsequent absorption. But Price himself never says so, writing instead that it is an advantage of the proposal that a particle can be emitted even in the absence of sufficient absorbers. Second, the proposal seems to entail that there is as much backward causation as forward causation in the world. But Price writes that there is backward causation only in exceptional circumstances. Here are the details.

Price states in his 1994 *Mind* paper that quantum mechanics should be interpreted as revealing that some causation is backward rather than forward.[88] In his 1996 book he talks of the possibility of 'exceptional cases, in which the past could properly be said to depend on the future'.[89] The past which he is talking about, and which might coherently be taken to depend on our present actions, is 'the *inaccessible* past – that bit of the past which we cannot simply "observe", before we act to bring it about'. The logical space for the possibility of such an interpretation lies entirely in the gap between the past and the *accessible* past.[90] This is of course Dummett's loophole. Price no doubt has in mind the EPR/Bell's experiment, and the whole class of experiments which show specifically quantum effects. Price is suggesting only a limited retrodependency.

But it would be a mistake to think that the past can depend on the future only in exceptional cases. Take Price's polarizer example, where a photon passes

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87 Wigner 1967, p. 222.
88 Price 1994, p. 306.
89 Price 1994, p. 329; Price 1996, p. 180.
90 Price 1996, p. 180.

through a polarizer. Price argues that symmetry requires that the photon's spin may be correlated with the polarizer it passes through not only *after* the photon has passed through it but also even *before* the photon reaches it – otherwise we could tell the future from the past even in the case of a single photon, in the absence of thermodynamic considerations.

Why does Price want to allow correlations between photons and polarizers in both time directions (thereby rejecting the micro-independence assumption)? Is it just because, for symmetry to exist, he thinks we ought to be able to take either end of the process as the beginning and have the same laws apply in the same way, the two descriptions simply being alternative and equally good descriptions? Is that all there is to it? That is certainly a part of the reason, but there is more to it than that. Price is making a stronger claim. He is saying – or needs to be saying – that there is backward causation involved in the passage of a photon through a polarizer (and quite generally, too, as we shall see). He is, in effect, making an *existence claim*.

To see this, suppose that the photon in question has been emitted from an initial singlet state (the state that is conventionally taken as the initial one), and has its singlet twin that has not yet been discoupled by a measurement. In that case, a future measurement of the photon's spin at $t = 1$ (the measurement being constituted by its successful passage through the polarizer) acts back on the joint initial state at $t = 0$, enabling a correlation of spin-states between the twins without spacelike influences being needed. This is just Price's proposed advanced action explanation of Bell. The future measurement is a boundary condition for the initial state of the system, and there is backward causation at work. Moreover, in Price's advanced account there are no spacelike influences, as the backward causal action is transmitted by the particle itself. In other words, backward causation works in the backward temporal direction in just the same way as ordinary forward causation is supposed to work in the usual forward temporal direction. A particle simply travels between A and B. For this reason, in the case of a singlet-state origin photon, the backward description of the photon starting from the 'future' end (and ending up at the singlet state end) is not just an *alternative* description. It is rather a description of something that, in some sense, *actually occurs*, just as the ordinary forward-in-time process occurs, even though Price provides no details. (If not, backward causation doesn't explain Bell.) In short, it would seem that a photon emitted at A and absorbed at B (where, in the present example, A is the initial singlet state and B is a photon detector placed immediately behind the polarizer) must also, in some sense, be emitted at B and absorbed at A, 'travelling', in effect, both forward and backward in time 'simultaneously'. There is a 'doubling-up' of processes.[91] It needs

91 'Travelling' in time in either temporal direction is of course only convenient shorthand. More technically, one would say that the world line of the backward-in-time

to be kept in mind that we're not talking about the classical system in which our perspectival account of causation is unidirectional (in the sense described in §4.9). In such an account, we could take the direction of causation as being in *either* temporal direction, but not both at once. Here, we're talking about a system that is missing half of the causal determinants required for determinism – which is why the direction needs to be both ways at once. (It is just this that enables the advanced action account to explain Bell.)

But in the advanced action account there is nothing *special* about a photon that has been emitted from a singlet state. It is just like any other photon that has been emitted from some source (any source). The whole point of the advanced action idea is to account for the correlation in terms of ordinary causal processes, with the sole difference that they are also occurring backward in time. Likewise, there is nothing special about a photon passing through a polarizer. It, too, is just like any other photon. The point is, if there is backward causation involved in the passage of a photon which originated in a singlet state, there must be backward causation involved in the passage of *every* photon between α and β, period, quite regardless of whether or not it originated from a singlet state. Why should a singlet-state origin *at one end* induce backward causation in a time-symmetric advanced action theory that is not present in all interactions? What is more, the proportions of backward causation to forward causation on the microlevel must be *equal*, since the particle itself is the bearer of the causal influence in Price's picture. (That backward causation may be involved in equal proportion with forward causation in all photon interactions [and more generally, in all quantum mechanical interactions] isn't really surprising, given that quantum mechanics is missing half of the causal determinants required to make it a deterministic theory.)

If the description of the backward photon is not just an *alternative* description, but rather a description of something that is, in some sense, actually occurring, in parallel with what is occurring in the usual description, it seems to follow that *every emitted photon must also be absorbed*. Else the picture wouldn't make much sense. How could there be a real backward process occurring in the absence of a future emitter? Additionally, an asymmetry would exist that would enable us to tell the future from the past (which is just the problem in the usual way of thinking that Price set out to correct). Consider a photon that is emitted by an atom and is never absorbed (e.g. owing to a shortage of absorbers), propagating instead to future infinity. Given an advanced action account of the sort sketched above, non-absorption would seem to create an intrinsic asymmetry in the world. At one end, the end we conventionally take as the beginning, emissions would always be associated with emitters. But in the backward time description, there

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particle lies along the same four-vector as that of the forward-in-time particle, but with an opposite time direction.

could be emissions even in the absence of emitters. So an unexpected consequence of Price's picture, if it is to be symmetric, seems to be that no photon can be emitted without the photon also being absorbed later. Even if Price's picture doesn't actually *entail* that every emitted particle is also absorbed, that seems to be a natural requirement to make.[92]

This kind of picture, in which both forward and backward causation are equally involved in every microevent, sits uneasily with Price's talk of a 'limited retrodependency', 'in exceptional cases', in the world. For example, the emissions and absorptions of photons by atoms belong to the class of microevents in which both forward and backward causation are equally involved (if I'm right). The emission and absorption of photons must be among the most common events in the known universe. I don't know how many photons there are zapping around in every litre of space outside on a sunny day, but whatever that number is, to it must be added the 500,000 photons or so per litre estimated to exist in the 2.7K relic cosmic background radiation permeating all of space.[93] Every photon in this enormous quantity belongs to the class of object whose present state ϕ_n depends not only on its past but also on its future (that is, if the backward causation story is to be believed). The retrodependency in the world seems rather ubiquitous.

Moreover, it needs to be clearly realized that in modern physical theory, the physics of macrosystems,[94] too, ultimately depend in an essential way on the physics of their constituent microsystems – each of which would exhibit individually the above retrodependency. The cohesion of matter, for example, is inexplicable in classical physics. But it is explained by quantum mechanics.[95]

What is said above also sits uneasily with another of Price's claims. According to Price one of the attractions of his reinterpretation of the Wheeler-Feynman ('WF') absorber theory of radiation, and of his advanced action proposal generally, is that there's nothing in it to prevent us from saying, apropos

[92] Symmetry could be restored by unemitted photons propagating to future infinity in both temporal directions.

[93] Weinberg 1977, p. 73.

[94] By 'macrosystems' I simply mean systems which are so massive that at first sight they do not seem to need quantum mechanical methods for the investigation of their properties: classical methods appear to suffice.

[95] Quantum mechanics makes essential use of the entanglement of states to explain the cohesion. Abner Shimony writes: 'Striking examples [of entanglement of states] are the covalent bond of the hydrogen molecule, the stability of the benzene ring, and the tensile strength of metallic crystals. In all these cases calculations show that no nonentangled state of the electrons of the system can explain the tightness of binding which is found experimentally.' (Shimony 1989, p. 389.)

See also Franco Selleri, who writes, '... quantum mechanics, which originated in atomic physics, can explain the properties of atomic aggregates, as well as of single atoms. Therefore, the properties of matter generally fall within the scope of quantum theory...'. (Selleri 1990, p. 3.)

cases where the absorber is absent, that the retarded wave [photon] from the source simply propagates to future infinity. (The possibility of radiative emissions even in the absence of future absorbers is supposed to have the advantage that it frees time-symmetric electromagnetism from the constraints of cosmology. For example, in the WF model the response of the universe is crucial. It must be 'correct', in the sense that the future universe must be a perfect absorber of radiation.[96]) But I have argued above that every emitted photon must also be absorbed, which seems to be inconsistent with a photon from a source propagating to future infinity.

For the above reasons, as well as for reasons of symmetry generally, Bell's experiment, and more generally, quantum mechanics itself, should be interpreted as revealing that exactly one half of all causation is 'backward' rather than 'forward', at least in so far as we seek to provide a 'classical' hidden variable model of causation on the quantum level, i.e. an account which contains no intrinsic probability.

5.3 Quantum mechanics & bidirectional causation

In the preceding sub-section it was suggested that every photon is a two-way particle, 'travelling' both forward and backward in time all at once. We shall take up this suggestion in some detail in the following chapter. In the meantime, we want to get some feel for it. What does it really mean? How does bidirectional causation differ from unidirectional causation? What might be its consequences? The following discussion assumes the general correctness of Price's perspectival account of causation, and simply seeks to connect it more directly with quantum mechanics. Rather than speaking of just the correlational structure of the world, we shall try to get a handle on what advanced action might actually amount to in physical terms.

Let us begin by reiterating a common refrain in this thesis. Quantum mechanics is a theory of 'insufficient cause', to use William Unruh's phrase. Take commonplace absorptions and emissions of light by atoms. These processes, like all others described by quantum theory, operate on the basis of 'insufficient

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96 Price 1991b, p. 971; Price 1996, pp. 72-3. It seems to me that even if Price were right and there could be emission of radiation in the absence of absorption (or vice versa), this particular 'advantage' would be a disadvantage. Theories with tight constraints are generally preferable to theories lacking such constraints (cf. the Ptolemaic epicycles). Of course, tight constraints on theories are sometimes hard to live with, because they make the theories containing them more readily 'falsifiable', in the sense that they tend to be inconsistent with existing theories and assumptions. When this is the case, something must be jettisoned – either an existing theory or paradigm, or else the new theory with its tight constraints. The natural tendency is to toss out the new theory – or at least relax its tight constraints. One then rationalizes this procedure by describing as a 'disadvantage' the principal theoretical virtue of the new theory, namely its tight constraints.

cause'. Given two absolutely identical situations, the outcomes can differ.[97] Already in 1935, Eddington noted that 'what is lacking to secure a complete and certain prediction of the whole future is always just *half* of the total data that would be needed'.[98] Such lack is built into the operator formalism of quantum mechanics in its alternative position or momentum space representations of the same quantities (as described in Appendix[h]). In position space, both the position and momentum operators are expressed in terms of the coordinates alone, whereas in momentum space, both operators are expressed in terms of the momentum alone. In neither representation is an operator ever expressed in terms of both. The commutation relations between the operators for q and p give the uncertainty relations. As we have seen, in 1958, Sciama pointed out that quantum mechanics is consistent with a theory in which *half* the necessary boundary conditions for arbitrarily accurate predictions must refer to the future of the moment t.[99] The advanced action interpretation seeks to understand this aspect of quantum mechanics within a realist framework by making use of the line of thinking pioneered by Sciama.

5.3.1 Bidirectional causation contrasted with unidirectional causation

Take a Newtonian deterministic system. For such a system it is just as true to say that the future determines the past as to say that the past determines the future. That's because if an isolated physical system is in state A at $t = 0$ it will necessarily be in state B at $t = 1$. Likewise, if it is in state B at $t = 1$, it must necessarily have been in state A at $t = 0$. Unidirectional causation is consistent with causation in either temporal direction. So the entire notion of the direction of the determination seems redundant. Instead, both the past and the future of all the members of a system are determined completely by the equations of motion and their positions and momenta at any moment. Newtonian determinism seems atemporal. Nonetheless, for non-logical, anthropocentric reasons we speak of causation as occurring from the past to the future.

But quantum mechanics is not such a classical deterministic theory. The 'elusive object of desire' of the hidden-variable advanced action project is a model showing that quantum mechanics need not be intrinsically probabilistic, appearances to the contrary notwithstanding. The idea is that complete determinants of

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97 Unruh writes: 'Quantum mechanics arose out of, and encodes within its interpretation, a very uncomfortable feature of the world, that the world seems to operate on the basis of insufficient cause. Things just happen, without our being able to ascribe any sufficient cause to explain the details of what occurred. Given two absolutely identical situations (causes), the outcomes (effects) can differ.' (Unruh 1995, p. 38.)

98 Eddington 1935, p. 98. Schrödinger, too, wrote: '... at most a well-chosen *half* of a complete set of variables can be assigned definite numerical variables... The other half then remains completely indeterminate...'. (Schrödinger 1935a, §2.)

99 Sciama 1958, p. 77.

atomic emission details do exist. Yet it is difficult to introduce determinism into quantum mechanics without falling foul of Bell's inequality, etc. Any 'hidden variable' theory must in general make the same predictions as quantum mechanics. In the standard interpretation, quantum mechanics lacks half of the determinants necessary to be a deterministic theory. They simply don't exist. As for the half of the determinants that do exist, we associate them with the past of the system for the same reason that we usually speak of the direction of time in the macroworld as being from the past to the future.

To maintain that quantum mechanics is a 100% causal theory, it is necessary to show that a quantum mechanical system does not lack the half of the required determinants that appear to be missing. Yet the system must at all times *look* as if it did lack them, needing a theory of 'insufficient cause' to describe it. (For example, it must predict a violation of Bell's inequality, in contradistinction to a 100% causal classical system, i.e. a deterministic system.) So a different type of causal account is required for describing fully causal hidden variable quantum mechanical systems than for describing classical deterministic systems (which are also fully causal). A new idea is needed. We might for instance say that the missing half of the determinants refer to the system's future, just as we say that the existing half refer to its past. There must be backward causation as well as forward causation occurring all at once.

How would one describe such two-way full causation? How is it to be *differentiated* from classical unidirectional full causation? The difficulty is that, just as with classical deterministic systems, the particle itself must be the bearer of the causal influence if locality is to be preserved. The particle itself in its passage between A & B must 'transmit' the influence. But how can the quantum mechanical particle do this without looking, in the causal account we give, just like a classical deterministic particle – which it can't look like even if it is fully causal? (It's no good simply saying that both the past and the future of the particle are determined completely by the equations of motion and their positions and momenta at any moment. That would be merely to reiterate the classical picture.)

One way is to introduce a doubling-up of energy states for the system, perhaps in the way suggested in the next chapter. The positive-energy half takes care of the transmission of the forward causation (retarded action) and the negative-energy half takes care of the transmission of the backward causation (advanced action).[100] The idea is that the one particle exists in a superposition of both states, forward in time in the one state and backward in time in the other. In the negative-energy state, the particle must be conceived of as 'travelling' backward in time. That is because the causal influence is backward in time, and the *particle itself* is the bearer of the causal influence. (Backward causation is

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100 The energy is negative relative to the conventional time direction.

called advanced action just because the action 'travels' backward in time, in the sense that what is conventionally taken as the 'effect' precedes what is conventionally taken as the 'cause'. For example, an advanced particle is already present at a point a distance x from the source at a time $t = x/v$ *before* the instant of its emission, whereas a retarded particle will arrive at the same point at a time $t = x/v$ *after* the instant of emission.) In this picture, it is the one and the same particle that 'transmits' causal influences in *both* temporal directions. (There are no causal influences apart from the particle itself.)

It might be wondered why advanced action is of negative energy in relation to retarded action. The reason is that it turns out that the characteristic energy of a system and its time direction are intimately connected and share the same sign. Absorption of an advanced wave, for example, is equivalent to the emission of a retarded wave. Both decrease the energy of the absorber/emitter. Hence the energies of the waves are of opposite sign (as are their momentum vectors). Similarly, the absorption of a retarded wave is equivalent to the emission of an advanced wave. Both increase the energy of the absorber/emitter, and so the energies (etc.) are of opposite sign.[101] A glance at the energy-momentum four-vector associated with an advanced particle also reveals that it is of negative energy. An energy-momentum four-vector diagram shows the relation between relativistic energy and momentum. The direction of the four-vector is the same as that of the world line.[102] That is one reason why a doubling-up of energy states is required. The other reason is to make 'room' for the backward causation, which needs somehow to be transmitted by the particle itself.

To gain additional insight into the relation between the sign of the energy, the direction of time and the direction of the propagation of the particle (and the associated causal influence), consider the Stückelberg-Feynman picture of pair-production and pair-annihilation (see e.g. Reichenbach, *The Direction of Time* for a description and discussion). Stückelberg-Feynman noted that an electron-positron annihilation event can equally be described in terms of a reversal in time of the electron's path. The forward-in-time electron turns into a backward-in-time negative-energy electron. In the language of Feynman's propagator theory, the electron is scattered back from the annihilation event. The potential, i.e. the boundary condition for such scattering is provided by the annihilation event.[103] So if we ask whether the boundary conditions (the potential) for the particle's trajectory lie in the past or the future, the answer is – in the past if we describe it as a positron, in the future if we describe it as an advanced negative-energy

101 For a thorough discussion, see Cramer 1980, pp. 362-4. See also e.g. Gold 1967, p. 40; Bjorken & Drell 1964, p. 207.

102 'In brief, the momenergy of a particle is a 4-vector of magnitude m pointing along its worldline in spacetime. This description is independent of reference frame.' (Taylor & Wheeler 1992, p. 195.)

103 Bjorken & Drell 1964, p. 207.

electron.

5.3.2 Not a new idea...

I have suggested that bidirectional causation requires that photons and quantum-mechanical systems in general exist in a superposition of positive and negative-energy states, forward in time in the one state and backward in time in the other. I have also argued that such a picture seems to entail that all emitted particles are absorbed. Even if I'm wrong in the latter regard and there is no such entailment, it seems a useful heuristic principle to adopt in an advanced action account. Something like this idea seems to have been first proposed by H. Tetrode in 1922. Tetrode argued that all radiation be considered an interaction between a source and an absorber. No absorber – no radiation! According to Tetrode:

> The sun would not radiate if it were alone in space and no other bodies could absorb the radiation... If for example I observed through my telescope yesterday's evening star... 100 light years away, then not only did I know that the light which it allowed to reach my eye was emitted 100 years ago, but also the star or individual atoms of it knew already 100 years ago that I, who then did not exist, would view it yesterday evening at such and such a time.[104]

The physical chemist G.N. Lewis, too (who coined the word 'photon'), was worried by the apparent failure of physics to take seriously the symmetry of its own equations, writing in 1930:

> I am going to make the... assumption that an atom never emits light except to another atom... it is as absurd to think of light emitted by one atom regardless of the existence of a receiving atom as it would be to think of an atom absorbing light without the existence of light to be absorbed. I propose to eliminate the idea of mere emission of light and substitute the idea of *transmission*, or a process of exchange of energy between two definite atoms... [105]

A variant of the idea was later independently formulated by Wheeler and Feynman in their 'absorber theory of radiation'. It needs to be emphasized that the picture I've suggested above is not the WF one, as there are no half-retarded and half-advanced waves or particles in it (though otherwise the same general principle applies in the WF model [no absorber: no emission][106]). Nor is it directly

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[104] Tetrode, cited in Gleick 1992, p. 120.

[105] Lewis, cited in Gleick 1992, p. 120.

[106] Strictly speaking, in the Wheeler-Feynman case the particle is not prevented from propagating to future infinity, if we are prepared to countenance explicit advanced effects. Wheeler-Feynman do consider a universe in which there aren't enough absorbers to absorb all radiation, and the ensuing advanced effects. (Wheeler & Feynman

motivated in our case (*pace* Lewis above) by considerations of *symmetry,* but rather, considerations of theoretical adequacy and consistency – though ultimately these are connected with symmetry.

5.3.3 Causal loops

This kind of picture entails causal loops. Consider a photon which is emitted by atom A and absorbed by atom B. A consequence of the picture is that the photon emitted by A must be causally antecedent to B, it must have already been at B and travelled to A, and vice versa. A photon's space-time path is 'simultaneously' both forward and backward in time. A photon can only come into being and exist, on this picture, if it is somehow emitted by *both* A and B, in opposite temporal directions. The one photon 'causes' both A to decay (emit a retarded photon to B) and B to be excited (emit an advanced photon to A). In other words, the photon is the 'cause' of its own existence.[107] But that is the case only if we insist on speaking in terms of causation. We may equally say, consistently with the above picture and still speaking from within a temporal perspective, that *neither* event causes the other; and speak, instead, of Price's 'correlational structure' of the world, which merely 'looks as if ...' etc. We may want to adopt this way of speaking because strictly speaking, as we've seen, no *part* of a microlevel 'causal' transaction may be treated in isolation and be said to occur before some other part (given our picture). To that extent, it seems that *any* talk of unidirectional causation is theoretically inappropriate, leading to inconsistency, even within the perspective of a bound-in-time observer. And of course, on going to the atemporal Archimedean view proper, a photon *really* belongs to neither of the two categories, 'advanced' and 'retarded'.[108] On this level, there

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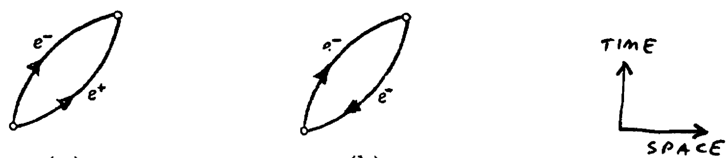
1945, pp. 171ff.) Since the *present* picture contains explicit advanced effects (namely half of all 'causes'), it may be possible in our case, too, to say that there is no need for future absorbers, for reasons analogous to those given by WF in the above paper.

107 This is analogous to virtual particle pair creation from the quantum vacuum – where, say, an electron-positron pair is created out of the vacuum energy. The pair initially move apart, and then come together again to annihilate each other, as in (a).



(a) (b)

A causal loop exists, because the positron member of the pair can be considered a backward-in-time electron, propagating back in time to annihilate itself, as in (b). In the language of propagator theory, the electron created at A is scattered back in time from B to destroy itself at A. (See Bjorken & Drell 1964, p. 92.)

108 Perhaps the ordinary photon is an example of Price's Archimedean 'observer'. From the perspective of its own frame, it spends no time in our universe, since the universe is contracted to infinite thinness in the photon's direction of travel. Consequently, it would seem that the concepts of time and length do not exist for the photon.

is neither causation nor causal loops, and the question of the predictability of future events doesn't even arise, since the perspective is atemporal. Nothing *happens* in it. The world simply *is*.

5.3.4 Price's proposal vis-à-vis the present picture

A consequence of my picture,[109] which is much like Price's, and yet seems to differ from it in an important way, is that there can be no ordinary forward causation without an equal amount of backward causation being involved in the causal transaction (and vice versa, of course), at least on the level of atomic transitions. For example, the ability of an atom to both absorb and emit photons depends crucially on the existence of past and future emitters and absorbers, with 'past' & 'future', and 'emitter' & 'absorber' being arbitrary designations from an atom's points of view. On this view, not only does the ability of an atom to decay and emit a photon depend crucially on the absorption of the emitted photon by a future absorber, but also the ability of an atom to absorb a photon depends crucially on the ability of a past absorber to absorb an advanced photon emitted by the atom (by the future absorber). Another consequence of it is that both absorptions and emissions, examined from either of the two opposed temporal perspectives, are fully causal, even though indeterministic in the sense of being unpredictable. There is no *intrinsic* probability in the proposed picture. Probability enters into it only because of our ignorance, given our temporal perspective, of the complete state of the physical system, which is fully determined by both past and future boundary conditions, much as Price has proposed.

Still, it does seem that the *overall* picture Price gives tends to obscure the fact that quantum mechanics operates on the basis of 'insufficient cause', in the specific sense that one *half* of the classically required 'causes' appear to be missing in any quantum event, when such event is considered from an anthropocentric perspective. In any self-respecting advanced action hidden variable theory, a natural way of providing these missing causes (hidden variables) would be by reference to future boundary conditions. Given that Price wants to provide an advanced action interpretation of quantum mechanics, and also restore symmetry in microphysics, I think it's fair to say that to date his analysis is inadequate just to the extent that it obscures or glosses over the fact that there is a sense in which quantum mechanics ought to be interpreted (from the anthropocentric temporal perspective of a macroscopic observer) as revealing (a) that exactly one half of all causation is 'backward' rather than 'forward', and (b) that the two directions of causation complement each other in the transactional way described above, enabling events to occur. To use a biological analogy, we might say that they work in *symbiosis*.

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109 Or perhaps not so much a consequence, but rather a principle in its own right.

If I'm right, Price is faced with a mini-'basic dilemma' of his own, the two horns of which are these:

(a) He can admit that quantum mechanics should be interpreted as revealing that no quantum event can occur without both forward and backward causation being involved in an essential way; e.g. if backward causation is invoked to explain the Bell correlations, there must be backward causation involved in all quantum-mechanical processes, quite regardless of whether or not the systems concerned happen to be in singlet states; or

(b) he can admit that his advanced action strategy can't explain Bell.

5.4 Required elements of an advanced action theory

What might be some of the more important elements of an advanced action *theory*, as opposed to a *general strategy*? It seems to me that such a theory – one taking proper account of relativity – ought to be able to achieve the following (in addition to showing how a local realist theory can reproduce the predictions of quantum mechanics in the EPR/Bell type of experiment):

(a) Retain the notion that the apparent asymmetry and temporal orientation of both time and causation are anthropocentric in origin, arising from the asymmetry of our perspective, as argued by Price.

(b) Answer the question of whether the wave function describes a single particle or an ensemble (this is possibly the single most important question for any interpretation of quantum mechanics).

(c) Account for why things happen in quantum mechanics on the basis of 'insufficient cause'; this requirement is obviously closely connected with the preceding one.

(d) Derive the indeterminacy relations from more basic elements of the model (presumably advanced action); this requirement is closely connected with the two preceding ones.

(e) Explain the complementarity manifested by quantum-mechanical systems; why is it that different *kinds* of measurements (e.g. of position or of momentum) produce nonlocal collapses of the wave function in the standard interpretation? Clearly, this requirement is closely connected with the three preceding ones.

(f) Account for the interference properties of quantum mechanical systems, e.g. as exhibited in the two-slit experiment. Since the standard interpretation in the Schrödinger version manages this by treating the particles as waves when unobserved (i.e. by invoking wave-particle duality), this request, too, amounts to a request for an explanation of quantum mechanical complementarity in terms of advanced action. Presumably, this would be achieved using the same basic elements of the model as in (d) and (e) above.

(g) Derive Planck's constant from more basic elements of the model. This question, too, is closely connected with the above requirements, since h is a measure of our ignorance of the future, which, in turn, is responsible for the in-principle indeterminacy in quantum mechanics.

(h) Account for the quantum measurement rules: in particular why the wave function is multiplied by its complex conjugate to obtain the quantum-mechanical probabilities; this requirement, too, is closely connected with the above ones, since the quantum measurement rules in their present form reflect the above-mentioned indeterminacy.

(i) Account for the 'correlational structure' of the world that Price mentions, or the particular 'mix' of forward and backward causation existing in the world from the anthropocentric standpoint of the embedded-in-time observer in various states of motion and gravitational potentials.

(j) Explain why there was a low-entropy beginning. Why wasn't the gravitational part (i.e. the tidal effect of the curvature of space-time) of the entropy of the world at its maximum value at the big bang, but was 'set', essentially, at zero?

Additionally, if one is going to look to *quantum field theory*, as Price has recently suggested,[110] for all the elements necessary to achieve a fully Archimedean theory of proper heuristic potential – bar the required interpretation, which is to come from advanced action – it would seem that the following minimum requirements will need to be added to the above list:

(k) Account for the phenomena of quantum vacuum fluctuations in terms of more basic elements of the model – presumably, advanced action.

(l) Clear up the related cosmological constant problem. Why doesn't the universe collapse into a black hole as a consequence of the gravitational effect of the average energy density of the quantum field-theoretical vacuum arising from these vacuum fluctuations (the 'zero-point energy' of the

110 See §5.2, 'Heuristic adequacy of Price's proposal'.

vacuum)?[111]

(m) And last but not least, derive the fine-structure constant from more basic elements of the model – something which Julian Schwinger, for one, has declared impossible within the framework of quantum electrodynamics.[112]

For the reasons I've tried to state above, it seems to me that all these requirements ought to be within the compass of a proper advanced action *theory* of quantum mechanics, as distinct from a general strategy.

Summary

In this chapter, the focus has moved on from Price's general strategy for interpreting quantum mechanics. We have been concerned to see how well the proposal fares in the light of certain theoretical/interpretational issues of quantum mechanics. One concerned the derivation of the measurement rules from advanced action, a second the question of whether the Heisenberg indeterminacy relations would remain the last word even in a realist Einsteinian model utilizing advanced action. A third noted that Price's interpretational efforts had so far extended only non-relativistic quantum mechanics.

The present chapter also raised the important question of the proportion of forward to backward causation in the world, and the related question of whether the two types of causation are *alternatives*, or whether they *coexist* in every micro-transaction. If the latter, how do they manage to coexist? The notions of unidirectional and bidirectional causation were compared. It was argued that Price's local advanced action proposal to interpret quantum mechanics is misleading in one important respect, and an interpretative dilemma for Price was revealed. This is the fourth of the four claims argued in this thesis.

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111 The 'cosmological constant', as the expression is today used, is a constant proportional to the energy density of a vacuum. According to quantum theory, the quantum vacuum (i.e. the 'ground state' of relativistic quantum field theory) is a seething 'soup' of virtual particle creation and annihilation, possessing a non-zero average energy density. That gives rise to a problem, because in general relativity, energy density is the source of gravitational fields (space-time curvature). Since the energy density of vacuum is non-zero, the existence of the bare vacuum ought to produce an appreciable curvature of space-time – arising not from the presence of matter located in the vacuum but from the energy of the vacuum itself. General relativity thus provides a means of determining the energy density of the vacuum by simply measuring the space-time curvature produced by it. The energy density turns out to be of catastrophic magnitude. This suggests that there is something wrong in the present quantum field-theoretical picture of the vacuum.

112 Schwinger 1987, p. 466. Feynman appears to hold a similar belief, judging by some remarks in his 1965 Nobel Prize Award Address.

To open up the way, it is proposed making fuller use than is customary of the doubling-up of energy states for quantum-mechanical systems revealed by the relativistic wave equation. The positive-energy solutions take care of the transmission of the forward causation (retarded action) and the negative-energy solutions take care of the transmission of the backward causation (advanced action).[113] The idea is that the one particle exists in a superposition of both kinds of states, forward in time in the one state and backward in time in the other. In the negative-energy state, the particle is to be conceived of as 'travelling' backward in time.

The above idea sets up things for the heuristic proposal of Chapter 6.

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113 The energy is negative relative to the conventional time direction.

6

A Heuristic Proposal

Rather against my better judgment I will try to give a rough impression of the theory. It would probably be wiser to nail up over the door of the new quantum theory a notice, 'Structural alterations in progress – No admittance except on business', and particularly to warn the doorkeeper to keep out prying philosophers.

(A.S. Eddington, 1928)

In the last chapter we concluded our investigation of Price's advanced action proposal. It was argued that in his advanced action proposal, Price is or ought to be making an existence claim, according to which both forward and backward causation are equally involved in every microevent (§5.2.3 & §5.3). It was argued that a consistent advanced-action picture requires that a quantum-mechanical particle exists in a superposition of positive- and negative-energy states, forward-in-time in the one state and backward-in-time in the other. A photon, for example, ought to be thought of as a two-way particle, 'travelling' both forward and backward in time at once. In this chapter, I try to extend this idea. The following account is a speculative attempt at interpreting quantum mechanics using advanced action, and Price's perspectival view of causation.

6.1 Some unfinished business

In 1924 de Broglie speculated that material particles were somehow associated with hitherto undetected oscillatory phenomena. In this way a unification of matter and light was to be achieved. Both would be different forms of some new kind of system that could act sometimes like a wave and sometimes like a particle.[1] Although the proposal was startlingly successful, neither de Broglie nor anyone else since has been able to explain the exact nature of the connection between the system's two aspects, at least within a local theory or without retreating behind the positivism of the Copenhagen interpretation. Even Price's local advanced action picture, although qualitatively able to explain quantum entanglement and the Bell inequality seems unable to make headway with quan-

[1] After Bohm 1951, p. 59.

tum interference generally without itself relying on the wave picture – the very thing it is supposed to explain away.

In the present chapter I put forward a heuristic proposal as regards the connection. It is an advanced action proposal, and it seems to shed light on wave-particle duality, and quantum-mechanical interference generally. It answers the question of what it is that 'waves' in wave mechanics. It shows how advanced action explains the mysterious quantization of energy, and how Planck's constant, Bohr's complementarity and Heisenberg's indeterminacy relations fall out of advanced action in a natural way. It gives an answer, in terms of 'hidden variables', to the question of why things happen in quantum mechanics on the basis of 'insufficient cause', and the related question of whether the wave function describes a single particle or an ensemble. It shows how matter gets its marching orders from both past and future boundary conditions, and is thereby enabled to 'know' exactly how to behave – even though its behaviour *looks* intrinsically probabilistic. It amounts to a 'hidden variable' interpretation of quantum mechanics.

I emphasize that the proposal is not a fully worked out theory. It is rather an interpretation of the existing formalism of quantum mechanics. Its attraction is in the connections it makes between seemingly unconnected ideas and issues. Some of these are: quantization and wave-particle duality (which permit the characteristic self-interference and superposition of states of quantum-mechanical systems), the negative energy solutions of Dirac's relativistic wave equation, Price's advanced action proposal and perspectival view of temporal asymmetry, and the cosmological constant problem.[2]

6.2 Introducing the idea

We begin by once more considering Planck's constant h. We are interested in the relation between energy and frequency ($E = h\nu$) revealed by h, and in trying to connect this relation with an Einsteinian realist theory utilizing advanced action.[3] We saw in Chapter 1 that it was the relation between energy and frequency that was novel in Planck's discovery, and not the concept of action itself, which was well known in classical mechanics. Before one can understand the quantization of energy represented by h, it seems at the very least that one must know what the frequency refers to.[4] (It is no good saying that it refers to the

[2] A difficulty is of course that while such claims are easy to make, they are generally very hard to prove. For that reason, what is attractive to the author may be just what is unattractive to the reader.

[3] By the same token, we are of course interested in the relation between momentum and spatial frequency or wave number $p = hk$ (or $p = h/\lambda$) revealed by h.

[4] A relation between energy and frequency of course requires a proportionality constant, and h is that constant. It is a conversion factor between the classical units of energy, e.g.

oscillation of a de Broglie [matter] wave, because in a realist account that is just what we want to interpret. The wave packet – which is what *collapses* in the standard interpretation – is just a linear superposition of de Broglie waves in that interpretation.)

So then, what of the frequency associated with *h*? In the case of the classical electromagnetic equations, we find another constant associated with a frequency, that being *c*, the speed of light. That frequency describes a real and measurable physical process, namely the fluctuation of electric and magnetic fields in a light wave. Might not the frequency associated with *h* also describe some analogous process which matter undergoes, which process might account for the characteristic interference of quantum systems, and so for the wave aspect of matter in the standard account?[5]

I propose that there is such an underlying process, and that all that is strange and non-classical in quantum mechanics arises because of it. The process has a frequency, a period and a 'wavelength' corresponding to the de Broglie wavelength even though it is not really a wave, as we shall see. It does, however, permit the characteristic self-interference of quantum-mechanical systems, and yield the usual indeterminacy relations. An element of the present proposal is that the familiar dynamical properties of objects such as energy and momentum are determined by this lower-level process, and are not intrinsic properties of matter. 'Fix' the lower-level detail and you thereby fix the higher-level detail. In particular, the frequency of the underlying process determines the higher-level or macroscopic (theoretical) observables, energy and momentum.[6] We could perhaps say, using Price's terminology, that the latter are *perspectival* and *anthropocentric*. Our customary talk of the energy or momentum of a system is a 'projection' from the kind of perspective we have as scientifically sophisticated human agents in the world. The underlying process itself, of which energy and momentum are projections, is unobservable even in principle, even though it is amenable to theoretical description. Thus, of necessity, our talk of 'energy' and 'momentum' has a conventional component. However, our de facto 'orientation' as (scientifically sophisticated) human agents requires us to choose something like the convention that we do, for the underlying process is objectively real, and imposes constraints on us. Thus our talk of the macroscopic properties energy and momentum has an objective component. The inter-joining of the conventional and objective elements in this picture is much like that in Price's perspectival account

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joules or electron volts, and the natural quantum-mechanical units of energy, namely cycles per second. But that doesn't tell us what the 'cycles' are.

5 In this connection, recall that according to the standard interpretation, the frequency refers to the frequency of oscillation of the wave function of a system in a state of definite energy. That frequency is given by $v = E/h$. (Weinberg 1993, p. 110n.)

6 The same goes for the relativistic equivalent of energy and momentum, the energy-momentum four-vector.

of causation. Some such inter-joining seems to be the case in all scientific theories.

The quantization of energy, the Heisenberg indeterminacy relations and a reinterpretation of de Broglie waves fall out of the proposed underlying process in a natural way, as we shall see. The process also connects in a natural way both past and future boundary conditions as determinants of the fates of systems.

What is this hypothetical process? Stated briefly, it is the negative energy catastrophe, which first made its appearance in Dirac's hole theory, or theory of the positron. We do not need to subscribe to the original hole theory to accept the proposed picture, however. Here is a summary of Dirac's theory, and its known limitations. Readers familiar with the theory may want to go straight to §6.4.

6.3 Dirac's introduction of negative mass into physics: the hole theory

It is well-known that for isolated systems, the laws of conservation of energy and momentum are not two different principles, but the same principle viewed from two different points of view: in any frame of reference, energy E and momentum p are related by

$$E^2 = c^2 p^2 + m^2 c^4 .$$ [7]

Because the above energy-momentum mass relation has two roots,

$$E = \pm c\sqrt{p^2 + m^2 c^2}\,,$$

Dirac's 1928 linear relativistic wave equation of an electron in an electromagnetic field[8] (which preserved the symmetries of both quantum mechanics and special relativity) is compatible with two kinds of solutions – those in which the kinetic energy of the electron is positive and those in which it is negative. The latter solutions are called the negative energy solutions and mean physically the intro-

[7] Regarding this equation, recall that Schrödinger's theory of quantum mechanics is an approximation which ignores relativistic effects. The Schrödinger time-dependent equation is an operator equation version of the non-relativistic energy equation, $E = p^2 / 2m + V$. The relativistic operator equation version of the Schrödinger equation is the Klein-Gordon equation, which also has negative-energy solutions. However, it does not include spin, and therefore applies only to particles of zero spin. Dirac used the same postulates as the Schrödinger theory but he replaced the energy equation by its relativistic form $E = \left(c^2 p^2 + m_o{}^2 c^4\right)^{\frac{1}{2}} + V$, and derived his relativistically covariant version of Schrödinger's equation possessing negative energy solutions, which predicted electron spin, and showed that the spin is intimately connected with relativity. (After Eisberg & Resnick 1974, p. 302.)

[8] Dirac 1928a.

duction of *negative mass*.[9] Thus relativistic mechanics permits in principle two sets of energy level distributions of matter: those with rest energy $+ m_0c^2$ and higher, and those with rest energy $- m_0c^2$ and lower. A 'forbidden' zone of width $2m_0c^2$, energies for which the Dirac equation has no solutions, separates the positive energy states from the negative energy states. More specifically, the Dirac equation has four solutions corresponding to an electron at rest, predicting four different kinds of electron – spin up and spin down of positive energy and spin up and spin down of negative energy.

The negative energy solutions led to a major difficulty. It is a well-established principle that physical systems tend to seek states of lowest energy. Consequently, ordinary positive energy electrons would be unstable in a vacuum because there would be nothing preventing them from emitting all their energy in the form of photons and dropping from positive mass states to negative mass states. This would result in the almost instantaneous disappearance of all matter of positive mass (in around 10^{-10}s). The world ought not to exist.

In 1929 Dirac came up with a physical explanation of why the world exists despite the negative energy solutions.[10] He assumed that nearly all the states of negative energy were already occupied by electrons, one electron per each state in accordance with the Pauli exclusion principle, and that nearly all the positive energy states were unoccupied. In the hole theory, a perfect vacuum is simply a state in which all the negative-energy states are filled and all positive-energy states empty. Such an infinite distribution of negative-energy electrons does not contribute to the electric field, only departures from the distribution doing so.[11] Since nearly all the negative energy states were already occupied, positive energy electrons were thereby generally prevented by the exclusion principle from dropping from positive mass states to negative mass states and emitting all their energy in the form of photons.

Now, imagine that one negative energy/negative mass electron is removed from the sea by raising it to a positive energy state by the injection of sufficient energy into the vacuum (at least $2m_0c^2$, where m_0 is the rest-mass of an electron). The negative energy electron has made a transition from the hidden 'world' of negative energy states into the familiar positive energy world of our experience, to emerge as an ordinary positive energy/positive mass electron. In leaving the negative energy sea it has left a *hole* in the initial distribution of negative energy and negative charge. This hole, or the absence of negative charge and negative mass, is indistinguishable from the presence of positive charge and positive mass. It acts just like an ordinary positron. Consequently, when a negative-

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9 Gamow 1985, p. 128. The rest of this section closely follows Gamow 1985, and also Herbert 1988.

10 Dirac 1930.

11 Dirac 1935, p. 271.

energy electron manages the transition from the negative- to the positive-energy state, a *pair* of particles always emerges, an electron and a positron.

In the converse process, when there exist an unoccupied negative energy state and ordinary positive energy electrons, one of the electrons may drop into the unoccupied negative energy state. This will betray itself in the disappearance of the electron. The filling of the negative energy state will in turn betray itself in the disappearance of a positron. Thus both an electron and a positron will disappear simultaneously, their charges cancelling each other, and their energies appearing in a chargeless form, as photons of gamma radiation.

Neither an electron nor a positron is destroyed or created in the Dirac process; the electron merely moves down and up between positive and negative energy levels, emitting energy when it goes down and absorbing energy when it goes up. (Stückelberg's and Feynman's comparable models picture the electron moving not between positive and negative energy levels but backward and forward in time.) There is thus one-to-one correspondence between the negative-energy solutions of the Dirac equation and the positron eigenfunctions.[12]

Two years after Dirac's prediction, the positron was experimentally discovered. Since then, a whole family of antiparticles has been discovered, each conventional particle having its opposite number.

6.3.1 Limitations of Dirac's hole theory

A major limitation to the generality of Dirac's 'hole' theory seems to be that it works only for fermions. For bosons, which also obey relativistic equations that (like Dirac's) have both positive and negative energy solutions but which do not obey the exclusion principle, there is nothing in Dirac's theory to prevent them from making radiative transitions into the negative energy states even if these states are already filled with other negative energy bosons.

Another limitation is that in Dirac's theory, electrons are never created or destroyed singly but always together with positrons, that is to say, in electron-positron *pairs*. (The reason is, of course, that the outward processes of pair-creation and annihilation are according to the theory mere epiphenomena; what 'really' happens is that an electron simply transits every now and then between positive and negative energy states – an electron being eternal in this sense.) But in nuclear beta decay electrons are created without positrons out of the energy and the electric charge in the electron field.[13] A related difficulty is that in Dirac's theory, there ought to be as many positrons as electrons at any one time in the positive-energy world. (The same difficulty exists in modern cosmological theory.)

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12 Bjorken & Drell 1964, p. 67.

13 Weinberg 1993, pp. 234-5.

Yet another limitation is that the negative-energy 'sea' would need to be of infinite depth, with an infinite number of electrons per each cubic centimetre of vacuum (or at least a very great number).[14] The mass of these electrons (even though negative) would be infinite/(very great), which would result, according to the general theory, in the radius of curvature of empty space being equal to zero/observable over even short distances. An analogous difficulty arises in modern quantum field theory when we measure the background energy density of empty space. According to quantum field-theory this energy is not zero, because even 'empty' space is filled with virtual particle pairs. The mass-energy of these particle-pairs is very great. Therefore their gravitational effects ought to show up in the curvature of space-time even over short distances.[15] I shall say more about both difficulties in §6.10.5, 'The cosmological constant problem'.

Even putting the above difficulties to one side, physicists have always been uneasy about the fact that to explain the motion of just one visible electron, Dirac had to assume the existence of an infinite number of invisible electrons filling up the Dirac sea. In the early forties, Carl Stückelberg and Richard Feynman independently discovered a mathematically equivalent but ontologically less fulsome way to solve the negative-energy problem.[16]

6.3.2 The Stückelberg-Feynman interpretation

Stückelberg and Feynman proposed that the world is such that the two kinds of solutions of Dirac's equation exist in nature in particular combinations: All positive-energy solutions run forward in time, while the troublesome negative-energy solutions run backward in time. Solutions of the other kind – positive-energy/backward in time, negative-energy/forward in time are forbidden – are options nature has chosen not to use.

This Stückelberg-Feynman (SF) scheme works to prevent the world from collapsing by forbidding positive-energy electrons from turning into negative-energy electrons. Such an event is impossible because it would involve a (+) energy electron travelling forward in time turning into a (–) energy electron travelling forward in time, but the SF scheme does not contain any (–) energy electrons travelling forward in time.[17] A possible history for a (+) energy electron is to

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14 Gamow 1985, p. 130. The negative-energy sea would also need to consist, simultaneously, of all the different kinds of spin-half particles, such as electrons, protons, neutrons, etc. This is paralleled in quantum field theory, in which all the *fields* exist simultaneously.

15 Hawking 1988, p. 157.

16 This paragraph and the following four paragraphs closely follow Herbert 1988, pp. 144-8.

17 Nor, for that matter, is there any solution of the electromagnetic wave equation which has negative energy and also moves in the future light cone. This is because the time direction and the characteristic energy are intimately connected and share the

change into a (–) energy electron travelling backward in time. Viewed from our forward-time perspective, this process looks like an electron and a positron travelling forward in time which meet and vanish in a flash of energy – a matter-antimatter annihilation event.

In Dirac's scheme, before he added the filled sea, (+) energy electrons were unstable in vacuum because nothing prevented them from turning into (–) energy electrons. In the SF scheme, (+) energy electrons are stable in a vacuum but unstable against positron collision, but since positrons are relatively rare in our part of the universe, electrons will last virtually forever here.

Feynman showed that the SF scheme is mathematically equivalent to the filled Dirac sea, both schemes giving the same answers to all calculations. Despite the equivalence, it is thought that the SF scheme is a more realistic way of looking at the electron than Dirac's way. That's because the Dirac scheme will only work for fermions. For bosons, which do not obey the Pauli exclusion principle, filling up the (–) energy states with invisible particles will not prevent the (+) energy particles from dropping into the (–) energy states.

Following the interpretative efforts of Stückelberg and Feynman, the world was finally perceived as completely safe from the negative-energy catastrophe. Not even the recalcitrant bosons could now prevent the world from existing in the comfortable manner to which we have become accustomed (though of course the backward-in-time world lines *were* a bit disconcerting).

However, there is another way to secure stability – a way that opens up new possibilities for the interpretation of quantum mechanics. Simply allow the negative energy catastrophe to happen.

6.4 The negative-energy catastrophe is alive & well

Progress in physics is usually made by dropping assumptions.

(David Bohm, in Davies 1995)

Dirac postulated the existence of a negative-energy sea of electrons because it seemed obvious to him that *something* must prevent positive-energy electrons from falling into the negative-energy states. But why is such prevention necessary? Assume instead that the negative-energy catastrophe is taking place at every instant – that both fermions and bosons are continuously falling into negative energy states. Once a particle of positive energy, say an electron, has fallen into a negative energy state, there is a still lower energy state for it to fall into.

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same sign. (After Cramer 1980, p. 364.) Thomas Gold, too, pointed out that negative energy implies 'backward in time', and vice versa, which is why we never find a process going 'forward in time' which is associated with negative energy. For a discussion, see Gold 1967, pp. 35-41.

That is its previous *positive* energy state, which, from the frame of the particle in question, is now a negative energy state – the particle always perceiving its own state as a positive energy one. It 'falls' back into its original positive energy state, and so on, the process always repeating, the electron spending equal amounts of time in each state.

There are, in this picture, two simultaneous radiative transitions occurring. In the one, there is loss of all the particle's energy. In the other, a corresponding gain. It is the ceaseless 'falling' of the electron into ever 'lower' energy states in search of elusive equilibrium (always around the next 'corner' but never attained) that gives it its 'spin' and renders the electron a tiny circulating current.

To help fix the idea, consider the following rough picture. (Additional details are provided throughout the rest of the chapter.) Take a free electron. In addition to its 3-velocity, the electron has an additional degree of freedom, in that it also undergoes a rapid oscillation between negative and positive energy states, of frequency $\approx 10^{20}\ s^{-1}$. The two degrees of freedom, the former in 3-space and the latter at 'right-angles' to it, together give something like a wavelike property to the electron's world line, though the resulting (abstract) 'wave' is not only complex but also discontinuous (and thus not really a wave) owing to Dirac's equation having no solutions for the region between the positive and negative energy states.

The main feature of interest of this picture for our purposes is not so much that the electron's path can perhaps, in a certain crude sense, be pictured as a 'wave', but rather the reality of negative-energy states, and what may be a novel interpretation of them.[18] In fact, the idea is better thought of as a way of bringing in interference *without* bringing in waves, as we shall see in §6.9.

There is more to be said, owing to the one-to-one correspondence between the positive and negative-energy solutions of Dirac's equation. The present proposal takes this correspondence to mean that every electron in one of the two opposite spin states represented by the two positive-energy solutions of Dirac's equation for an electron at rest has its negative-energy doppelgänger. Consistency with Dirac's theory and Feynman's propagator approach would seem to require that the doppelgänger have its world line oppositely directed to that of the electron.[19] Accordingly, every electron[20] is associated with a *pair* of world lines,

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[18] There is nothing objectionable about the introduction of negative energy *per se* into our world picture. For example, the energy of a gravitational field is negative. See e.g. Guth 1997, pp. 289-92.

[19] Dirac was led to infer that the negative-energy solutions of his wave equation referred to the motion of the positron. However, positrons do not have negative kinetic energy, so the referral was rather to the motion of a hole (the unoccupied negative-energy state) in the Dirac sea of negative-energy electrons. (Dirac 1935, pp. 270-71.) Although we perceive the hole as a positron, the hole is also equivalent to a negative-energy electron travelling backward in time (the Stückelberg-Feynman physical inter-

oppositely directed in time, the two being separated by Dirac's 'forbidden' energy zone of width $2m_0c^2$ for which the Dirac equation has no solutions.[21]

In effect, we have replaced Dirac's negative-energy sea of electrons by individual advanced negative-energy electrons, one per each positive-energy electron.[22]

As the positive-energy electron falls into the negative-energy state, symmetry requires that its negative-energy counterpart simultaneously falls into the positive-energy state, and so the negative-energy catastrophe described above is mirrored by the converse process. The negative energy catastrophe is occurring ceaselessly in *both* the two 'worlds' revealed by Dirac's equation. This is what does the 'waving' in wave mechanics. This is how the present proposal answers Bell.[23] Described in terms of the Stückelberg-Feynman scheme, a forward-in-time (+) energy electron changes into a backward-in-time (–) energy electron, while at the same time its backward-in-time negative-energy counterpart undergoes the converse process.

Consider a crude picture of an electron's space-time path between A and B. It is a consequence of this picture that corresponding with the path, there is also another path, namely that of a backward-in-time negative-energy electron.[24]

There is a refinement necessary to the above somewhat simplistic picture. When the negative energy electron falls 'up' into the positive energy state, consistency requires that it ought to leave a *hole* in the 'Dirac' negative energy sea of electrons from whence it emerged. (In the present picture there is actually no need to postulate Dirac's filled negative energy sea of electrons since in it every particle simply has its negative energy counterpart [the negative-energy 'sea' is only

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pretation of the positron – the basis of the propagator theory). Hence the backward-in-time world line of the negative-energy counterpart of the electron, which I've postulated.

20 And every other particle, too, including the positron and other antiparticles.

21 I leave open for a moment the question of whether the doppelgänger is somehow to be identified with the original electron.

22 If we want to go to a description in terms of 'seas' of particles, both the positive and negative-energy electrons may be regarded as holes, respectively, in positive and negative-energy seas of positrons – even though in the present picture the 'seas' are more like very thin *carpets*, being only one particle deep; see §6.10.4.

23 Bell (1987, p. 187) asked: 'What is it that "waves" in wave mechanics?... In the case of the waves of wave mechanics we have no idea what is waving... and do not ask the question. What we do have is a mathematical recipe for the propagation of the waves...'

24 The analogy of wave talk is useful. In terms of that talk (but bearing in mind the above caveats), there are at any instant a *pair* of 'waves' associated with the electron, of opposite energies, oppositely directed in time, their crests and troughs coinciding. The pair constitute a four-vector *standing wave* – a superposition of advanced and retarded waves – the two ends of the wave coinciding with the creation and future destruction of the electron. No problems of 'colliding causality' arise just as none arise in the case of ordinary standing waves.

one particle deep, so the hole in the Dirac sea is more like a hole in a carpet], and in any case the negative-energy catastrophe is an essential part of the picture. However, let us stay with the language of the original hole theory in this paragraph because that language is likely to be familiar to the reader. [It is readily translatable into the language of the present picture.]) It might then be thought that we ought to see not only the electron but also this hole, or the *absence* of negative energy, negative charge, and 'negative' spin, i.e. we ought to see a positive energy positron of opposite spin to the electron, just as we do during ordinary pair-creation, so that we never see a free electron without a positron companion.[25] The reason we *don't* see the positron in this case is because there is also the other process occurring 'simultaneously' – the free electron in our frame dropping into the negative energy state – into the hole. When it does so, the created positron disappears. That is, *as soon as* the positron is created, it is *un*-created. The upshot is that, whereas there is always an ordinary electron to be seen, its companion positron is never to be seen.

The continual disappearance and reappearance of the positive-energy free electron (its dropping into the negative-energy state and its replacement by its negative-energy counterpart) may be thought of as a *virtual process* – the continual annihilation of a real electron, accompanied by the radiative loss of its rest-energy into the vacuum (actually into the duplicate world) and the continual re-creation of a real electron through the re-absorption of the radiated energy from the vacuum (from the duplicate world). Another way of describing the process is as an exchange of virtual particles: the electron is continuously exchanging energy with *itself*. This may account for the existence of a *virtual photon cloud* about every electron and other charged particle. The process is a kind of *complementary* of the familiar process of virtual pair creation and annihilation in the vacuum in which a virtual particle pair spontaneously appear out of the vacuum and mutually annihilate (which may be regarded as a closed causal loop if we think of the positron as an electron which goes backward in time [§6.6]). In contrast to the familiar process, in the present process it isn't the duration of the *existence* of the (real) particle that is limited by Heisenberg's uncertainty relation but rather the *duration of its virtual annihilation*. In the present picture, then, there are *two* equally important virtual processes taking place in the world: a virtual creation and un-creation of 'new' matter from the vacuum (just as in the standard picture – needing, however, interpretation in terms of the new concepts of the present picture), and also a virtual un-creation and re-creation of 'existing' matter (the hypothetical new process – another closed causal loop) – the two processes being the two sides of a single coin, as it were. The two appear to represent a new *symmetry* in the world.

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25 This would be so irrespective of whether the hole is in the Dirac 'sea' or the electron 'carpet' of the present picture.

There appears to be no reason why this picture cannot be extended to include quanta of other fields. According to this picture, not only every free electron but every other real particle and antiparticle in the universe is ceaselessly undergoing a like process of oscillation or interference between negative and positive energy states. Unlike Dirac's original hole theory, the picture also accommodates the creation in some processes (such as nuclear beta decay), of electrons without positrons. From an Archimedean perspective, the total energy of the world is zero.

6.5 Evidence for the negative-energy catastrophe?

There is no question that there is an unseen world. The problem is how far is it from Midtown and how late is it open?

(Woody Allen, Examining Psychic Phenomena)

Is there any evidence that such a process may be occurring? There seems to be evidence for it in the formalism of quantum mechanics. When Dirac sought a relativistically covariant equation (of the form of the non-relativistic Schrödinger equation $i\hbar\frac{\partial\psi}{\partial} = H\psi$) with a positive definite probability density for an isolated electron, he was led to his celebrated equation possessing the extra negative-energy solutions. Now, when we superpose the plane-wave solutions at our disposal to construct localized wave packets from the complete set of free-particle equations, a result emerges that suggests the above process: in addition to the time-independent group velocity of the electron, there now appear *cross-terms* (interference terms) between the positive and negative energy solutions that oscillate rapidly in time with angular frequency

$$\omega_o = \frac{2mc^2}{\hbar} = 1.6\times10^{21}\,\text{sec}^{-1}.$$

Note that this frequency is twice the de Broglie wave frequency of an electron at rest, $\omega_0 = 7.8 \times 10^{20}\text{sec}^{-1}$. This rapid oscillation or interference between positive and negative energy states (*zitterbewegung*), apparently first noticed by Schrödinger, is proportional to the amplitude of the negative energy solutions in the packet.[26] The mysterious *wave* aspect of matter, which we are trying to interpret, appears to be closely connected with the negative energy solutions.

In the standard interpretation, the zitterbewegung is associated with virtual electron-positron pair creation and annihilation, as is electron spin.[27] I propose that the zitterbewegung be reinterpreted as the 'negative-energy catastrophe' process described above. Likewise, virtual electron-positron pair creation and

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[26] Bjorken & Drell 1964, p. 38. For some details, see Dirac 1935, pp. 260-2.

[27] Thirring 1958, pp. 79 and 82, respectively.

annihilation is also to be reinterpreted in terms of that process (§6.10.4).

6.6 Bringing in advanced action

Advanced action enters the picture in the following way. The present proposal entails a duplication of the world. Owing to its oscillation between the negative and positive energy states, a particle such as an electron is present both in our macroscopic world – 'our' world being defined as the one in which matter is conventionally taken as being of (+) energy – and a duplicate macroscopic world in which matter is conventionally taken to be of (–) energy. We could say that the electron spends equal times in each world (would have equal probability of being found in either state if that were an observable). This seems to be a natural interpretation of the one-to-one correspondence between the positive and negative energy solutions of Dirac's equation.

Now, as we saw in §6.4, the negative-energy solutions of the Dirac equation propagate backward in time, thus reversing the roles of emission and absorption (and associating physical observables with the negatives of the parameters of the negative-energy solutions).[28] Consequently, the duplicate world is a backward-in-time world in relation to our world. Together, the two worlds constitute the *whole* world (the Archimedean world). This enables us to bring into our picture advanced action and future boundary conditions as partial (half) determinants of the fates of particles in a natural way, together, of course, with past boundary conditions and retarded action, which are the remaining (other-half) determinants. The oscillation connects both the forward-in-time and backward-in-time worlds. Each electron, and more generally, each particle of matter, in both worlds, loops incessantly between the two, its successive positions in each constituting its world line.

Above, I've said that the oscillation connects the two worlds. But it should not be thought that there simply exist two independently existing world lines of the particle, one in each world, oppositely directed in time, and that there is an additional, passive process, namely the oscillation of the particle connecting the two. It is rather that the world line of the particle in the one world *generates* the world line in the other world, and vice versa, through the oscillation. The process is atemporal. *Each world line is necessary for the other's existence.*

As a consequence of the oscillation, the world line of each particle is both forward in time and backward in time 'simultaneously', the two separated by the forbidden energy zone $\geq 2mc^2$. The backward particle's past boundary conditions are the forward particle's future boundary conditions, and vice versa. According to the present proposal, an ordinary electron is such a particle-'pair'

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28 Bjorken & Drell 1964, p. 207.

undergoing the above process.

That is how, on the quantum level, matter manages to get its marching orders from both past and future boundary conditions, and is enabled to know how to behave – even though that behaviour looks intrinsically probabilistic to us (appears to be missing half the requisite causes[29]). The two duplicate worlds provide the necessary boundary conditions on the quantum level for each other's existence. There is perfect symmetry between backward and forward causation, just as there is between matter of positive and negative energy. This is how the present proposal connects the negative-energy catastrophe and the hypothetical *wave* aspect of matter with advanced action. (A specific illustration is given in §6.10.1.) In §6.9, I shall attempt to reinterpret de Broglie waves and the quantum mechanical wave function more directly in terms of the present process.

6.7 Obtaining Planck's constant and the quantization of energy

I've argued in §1.1 that to explain Planck's constant h is to interpret quantum mechanics, and vice versa. There are at least five things that a realist advanced action theory must explain about h. (1) Its magnitude. (2) The mysterious relation between energy and frequency entailed by h. Just what does the frequency refer to?[30] (3) The quantization of energy in multiples of h. I've noted in §1.1 that even though the quantization of energy is central to quantum mechanics, it is not well understood. (4) The Lorentz invariance of h, i.e. why h is a *constant*. (5) The relation between h, advanced action, and the missing 'hidden variables' sought by Einstein. If advanced action is to play the main part in a local hidden variable interpretation of quantum mechanics, h must be a measure of our principled ignorance of the future boundary conditions determining the fates of systems. (Regarding this point, we saw in §3.6 that Sciama noted that the world can be fully deterministic if half the necessary boundary conditions for arbitrarily accurate predictions refer to the past and half to the future of an arbitrary moment t. As for Planck's constant, Sciama noted that 'h is a "measure" of the amount of deviation from classical mechanics'. Consequently, in a theory in which half the boundary conditions must refer to the past and half to the future of the moment t, h is 'a measure of our ignorance of the future'.[31])

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29 As Schrödinger put it, '... models with determining parts that uniquely determine each other, as do the classical ones, cannot do justice to nature... The classical concept of *state* becomes lost, in that at most a well-chosen *half* of a complete set of variables can be assigned definite numerical variables... The other half then remains completely indeterminate...' (Schrödinger 1935a, §2.)

30 Recall that it is the relation between energy and frequency that is mysterious, and not the concept of action itself. Planck's constant arises because such a relation requires a proportionality constant, and h is that constant.

31 Sciama 1958, p. 78.

The trick then, as noted in §1.1), is to come up with a physical theory which uses both past and future boundary conditions to determine the fates of quantum mechanical systems and to account for h, relating the two. In that connection, Sciama also noted that if quantum mechanics is deducible from a more basic theory, then presumably h will be expressed in such a theory in terms of quantities fundamental to the basic theory. With an advanced action theory, the quantities fundamental to the basic theory are advanced action and future boundary conditions. If these could be brought in so that they act in 'symbiosis' with retarded action and past boundary conditions, they could play the role of the hidden variables of an Einsteinian realist theory.

It seems that the present proposal goes a considerable way toward doing that. It does not explain the magnitude of h. So there is more to be said.[32] But it does appear to provide an explanation of (2), (3) (4) and (5) in terms of a single physical 'mechanism', namely the negative-energy catastrophe. As regards (2), the frequency ν in $E = h\nu$ is the frequency of the oscillation between the negative and positive energy levels. This is a crucial part of the present explanatory story as it is only the existence of ν that necessitates the presence of the proportionality constant h in the formalism of quantum mechanics. As for (3) and (4), see below. As regards (5), we've seen that the oscillation entails that each particle has associated with it a *pair* of world lines, separated by Dirac's forbidden zone, one forward in time and the other backward in time, in this way connecting past and future boundary conditions as determinants of the behaviour of quantum systems in a natural way – the backward in time world line being the long-sought hidden variable in each case.

Regarding (3), the quantization of energy in increments of $h\nu$ seems to fall out of the negative energy catastrophe in a natural way. Here is how.

We have seen in Chapters 1, 4 and 5 that h reflects our ignorance of that half of the boundary conditions necessary for arbitrarily accurate predictions which refer to the *future* of the moment t. However, I have postulated that quantum mechanical systems can and do access both the past and future boundary conditions (which jointly determine the complete dynamic variables of the system).

I have also postulated that the energy of a system (e.g. an electron) is determined by the frequency of the oscillation between the negative and positive energy levels associated with the system. Fix the lower-level detail (the frequency

[32] In the opinion of the author this 'more to be said' is to do with cosmology. In natural units, the magnitude of h is of course 1. A large part of the issue concerning the magnitude of h is to explain the fine structure constant, which connects e, c and h (in the form $\hbar$) in a single relation. Its magnitude is 137.03597. This number is a complete mystery. It is known, however, that its inverse square root , [–] 0.08542455, is the experimentally determined probability amplitude (or coupling constant) for an electron to emit or absorb a real photon. (Feynman 1985, p. 129.)

of the postulated process) and you thereby fix the higher-level details, such as the energy and the momentum. (The significance of this is, as we've seen, that the higher-level or macroscopic properties, the energy and momentum [relativistically, energy-momentum four-vector], are not intrinsic to the system, even though they are all we can access. They are rather 'epiphenomena' – anthropocentric *projections* of the hidden element [the frequency, or frequency vector] of the more fundamental underlying process.[33])

So far so good. But we need to be more specific about how h enters into this picture. h is a constant of energy multiplied by a time. How does the *time* come in – and exactly how does the present picture explain the fact that the product of energy and time is a constant? Well, in addition to a frequency, the postulated underlying process has a *period* τ, such that $\tau = 1/\nu$ [= t, in e.g. seconds]. The period is simply the *time* taken for completion of one oscillation. Since $\nu \Rightarrow E$, we have $1/E = \tau$ (divided by a proportionality constant). The two are inversely proportional. In natural units they are reciprocals. The greater the frequency of the oscillation (and so the energy of the associated system), the briefer the period (or time) of the oscillation, and vice versa. If we multiply the two, we obtain a constant of action. That constant is h, i.e. $E \times \tau = h$.[34] Recall (§1.1) that a common use of the term 'action' (energy × time) by physicists is in measuring simple oscillations.

In the above connection, it's important to remember that a central premise of the present proposal is that *nothing at all* can occur in the absence of equal amounts of forward and backward causation, i.e. in the absence of the postulated *pair* of world lines of the present picture, along which causality is propagated. (See §5.3 regarding the bidirectional propagation of causation.) For example, it would be impossible for a photon to be emitted in the absence of equal amounts of forward and backward causation. We've also seen that the pair of world lines are *generated* by the oscillation of a microphysical system between the negative and positive energy levels. *One* completed oscillation is the smallest unit, or quantum, of such generation. Hence a single oscillation is also a single

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[33] This seems consistent with the way energy is defined in today's quantum mechanics as 'the change in phase (in cycles or parts of cycles) of the wave function of the system at a given *clock* time when we shift the way our clocks are set by one second' (Weinberg 1993, p. 110n). As for the mass and momentum, they are another way of talking about the energy. 'In much the same way', continues Weinberg, 'the component of the momentum of any system in any particular direction is defined as the change of phase of the wave function when we shift the point from which positions are measured by one centimeter [metre in SI units] in that direction, again times Planck's constant. The amount of spin of a system around any axis is defined as the change of the phase of the wave function when we rotate the frame of reference we use for measuring directions around that axis by one full turn, times Planck's constant.' (Weinberg 1993, pp. 110-11n.)

[34] I conjecture that the required period is the 'inner time' of the system. For a discussion of the distinction between 'inner time' and 'outer time', see Jammer 1974, pp. 150-4. See also the brief discussion of the same in the notes to §1.3.2.

quantum of bidirectional causation. (I shall call such a quantum a *bicausa.*) Now, consider (a) the fact that the oscillation is of some frequency ν and period τ, together with (b) the postulate that the oscillation manifests itself macrophysically as an energy proportional to the frequency of the oscillation ν. It follows that in one completed oscillation (the minimum 'bicausa' for anything to happen), we have an energy E existing for a minimum duration τ, yielding a quantity that has the dimensions of action. Moreover, increase ν and you reduce τ, and vice versa. The unit of action remains constant. This constant of action is h.[35]

Here is an illustration. Take the emission of a photon by some atom (more specifically, by the atomic electron of some atom). The photon carries with it a unit of time, namely the period of its vibration. The product of the amount of energy emitted by the photon and the period of its vibration is always a constant, given by h. That is, $E \times \tau = h$. The same relations apply to every emission of a photon, by any atom. The photon's energy is likely to be a different number of joules and its period of vibration a different number of seconds, but their product will always be h. There are two things to be explained by any red-blooded realist theory here. One is the atomic emission details. What determines the exact moment of emission and the velocity and energy of the emitted photon? That explanation is given in §6.10.1. The other is: why is the product of the emitted photon's energy and period always a constant, i.e. quantized, regardless of the details of the emission? According to the present proposal, the product is a constant because the photon (and every other emission product, and indeed, everything else in the universe) is undergoing the same oscillation between the negative and positive energy levels. Consequently the above relation between ν and τ applies to all photons and emitting/absorbing atoms. Of course, not all photons are of the same frequencies or periods, nor all atoms. The frequency of any particular photon (or atom) is proportional to the photon's (atom's) energy, and the photon's (atom's) period is inversely proportional to the energy. However, by appropriately rotating our own world line or that of our measuring instrument to allow for the different energies of the photons (or atoms), we can make even the numbers of joules and seconds to come out the same – showing (as Eddington put it), that even though there are many different material atoms, there is only *one* quantum of action (or as I've put it, there is only *one* quantum of bicausa). This quantum is one oscillation between the negative and positive energy levels.

The proposed relation between energy and frequency, and so between energy and time, requires a proportionality constant owing to the different units involved, and h is that constant, so that $h = E/\nu$.[36] Just as in the standard inter-

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35 There is more to be said, owing to the distinction between bosons and fermions, but I shall not attempt to say it here.

36 The relation is normally written as $\hbar = E / \omega$, where $\omega = 2\pi\nu$ and $\hbar = h/2\pi$, repre-

pretation, h is a conversion factor between the classical units of energy, e.g. joules or electron volts, and the natural quantum-mechanical units of energy, namely cycles per second (which in the present model refers to the frequency of the oscillation of the system between the negative and positive energy levels). In natural units, of course, the magnitude of h is 1. The important point for our purposes, however, is that even in natural units, h is a constant, and its dimensions are those of energy multiplied by time. That's what the present proposal explains, together with the mysterious quantization of energy in increments of $h\nu$.[37]

The Lorentz invariance of h (point [4] above) follows. The individual elements of h, energy and time (or momentum and position), are expected to vary relativistically from observer to observer depending on the state of motion because the more basic elements, frequency, period, etc. of the underlying process do so. The upshot, however, is that h itself is invariant in every frame.[38]

6.8 Obtaining the indeterminacy relations and Bohr's complementarity

A classical particle has a *trajectory*, the concept of which implies that a particle has both a well-defined position and momentum. These are incompatible concepts in quantum mechanics. This follows from the postulates of the standard interpretation. The inherent indeterminacy revealed by the Heisenberg relation $\Delta p \Delta q \geq h$ is a consequence of the postulates. A sharp measurement of one of the pair of conjugate variables not only precludes any possibility of knowing the value of the other, but entails that such value does not even exist. Hence we can never say how the particle got from A to B. Indeed, there is a sense in which the particle takes all possible paths.[39]

How does the present proposal account for the indeterminacy? What does it say about the claim that a microphysical particle does not have a trajectory?

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senting the quantization of angular momentum.

[37] Recall (§1.1) Feynman's 1956 remark: 'We do not understand energy as a certain number of blobs. You may have heard that photons come out in blobs and that the energy of a photon is Planck's constant times the frequency. That is true, but since the frequency of light can be anything, there is no law that says that energy has to be a certain definite amount... there can be any amount of energy, at least as presently understood. So we do not understand this energy as counting something at the moment, but just as a mathematical quantity, which is an abstract and rather peculiar circumstance.' (Feynman, Leighton & Sands 1965, I- 4, p. 7.)

[38] This would be the case even in frames that are faster-than-light relative to the selected frame, although it would seem that the roles of the individual conjugate elements would be interchanged, just as, when $v > c$, timelike quantities and spacelike quantities are interchanged along the axis of motion.

[39] Davies 1984, pp. 8-9.

Well, we have seen that any measurement of the state of a microphysical particle can give knowledge at best of only one *half* of the information necessary to determine its hypothetical trajectory, the remaining half being left to be determined in the system's future. That's because a measurement is necessarily macroscopic, in that it relies on macroscopic apparatus. (This also applies to Renninger-style measurements, in which it is the absence of a result that gives us information about the system's state.) To make a measurement of a microphysical particle such as an electron is *ipso facto* to measure only (at best) one half of the details of its complete state, namely the half conventionally associated with the relevant past boundary conditions. Always, the other half remains unmeasured and unknowable, owing to the determinants of that half lying in the future of the macroscopic system (of apparatus, electron and observer). Only the microphysical electron itself has access to both sets of determinants, its *complete* state being determined by a loop-back process involving the future boundary conditions. The negative-energy catastrophe creating the duplicate and oppositely directed world lines of the electron in each of the two worlds enables the future determinants of the electron's state to act back, via advanced action, on its presently knowable state, providing the missing determinants of the complete state, and hence the requisites for a 'trajectory', subject to clarification below.

It is evident that in the case of a sharp measurement, one half of the determinants of the state are unknowable, even though they exist (albeit in the duplicate world). The same goes for any *un*sharp measurement, too, e.g. a measurement of an electron's position at one of the two slits of the two-slit experiment, in which the electron is not precisely located within the slit region itself. Such a measurement amounts to a mix of indeterminacy in the position and momentum. However, regardless of the details of the mix, one half of the electron's complete state again remains unknowable, that half being represented by the product of the indeterminacies in $\Delta p \Delta q \geq h$. That is to say, h is a measure of our ignorance of the system's future determinants. This is the first part of how the present proposal explains the inherent indeterminacy in the microsystem – why the *present* cannot be known in all its details, as Heisenberg put it – and why probabilities are needed to make predictions in quantum mechanics. As for the second part – an explanation of the *complementarity* inherent in the indeterminacy relations – we'll come to that in a moment.

Analogous considerations apply to the indeterminacy relation $\Delta E \Delta t \geq h$.[40] As is noted in Appendix(h), the members of an ensemble of identically prepared unstable systems (described by the same wave function) will not all radiate

[40] We saw in §1.1 that the indeterminacy relation $\Delta E \Delta t \geq h$ is an alternative way of writing $\Delta p_x \Delta x \geq h$, as is suggested by setting $c = 1$, so that energy (mc^2) = mass (m). When we do so, time (t), or better, period (τ) = a length (x, or better, λ). Recall that the period of a light wave is the inverse of its frequency (ν), so that $\tau = 1/\nu$. The latter, in turn, is given by the wavelength of light divided by its speed, so that $\lambda/c = \lambda/1 = \lambda$.

precisely the same energy, nor will they all radiate at the same time. Instead, the spread of the energies ΔE of the decay-product (e.g. a photon) which will be observed and the spread of the times of emission Δt of the decay-product (the lifetime of the state of the system) will be related by $\Delta E \Delta t \geq h$.

Such a spread arises in the present picture because both the energy of the decay product and the lifetime of the excited state of the system producing it are determined by the loop-back process mentioned above involving the future boundary conditions. Again, the negative-energy catastrophe generating the duplicate and oppositely directed world lines of the system in each of the two worlds enables the future boundary conditions to 'act back', via advanced action, on the emitting system at the time of emission, providing the missing determinants of the (apparently probabilistic) decay and emission phenomena. (For a specific illustration, see §6.10.1 below.) In this way, the Heisenberg indeterminacy relations fall out of the present proposal in a natural way – as does an ignorance (statistical ensemble) interpretation of the wave function (see §6.9 below).

In the standard interpretation, the requirement that the indeterminacy relations apply to an ensemble of measurements of identically prepared systems leads to all kinds of difficulties to do with the collapse of the wave function, as we saw in §1.3, the difficulties being known collectively as the measurement problem. But that requirement seems intuitively almost self-evident in the present picture. One is tempted to say: how could it be otherwise? After all, even though the particle's behaviour is fully determined by the relevant boundary conditions if we go to an explanatory account which includes *both* the two worlds of the present picture, yet in the world to which we are confined we can know at best only one half of those determinants – the remaining half relating to the unknowable future. We are therefore forced to rely on statistics in our predictions. But, as we have seen, as soon as we introduce statistics, we are necessarily talking of an ensemble – because, as Rosenfeld points out,[41] that is what statistics is for – comparison of many similar (but not identical) cases with different outcomes.

Even so, there is more to be said. Why is it, on the present picture, that it is the *position* that is unknowable, if we have measured a sharp value of the *momentum*, and vice versa? How does the present proposal explain the *conjugate relation* between the properties of momentum and position (known as 'complementarity' in the standard interpretation)? In other words, why is it that *certain* pairs of classical properties are incompatible (represented by non-commuting operators), while other pairs are not? For example, why is there an indeterminacy relation between momentum and position but none between momentum and

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41 Rosenfeld 1979, p. 28.

electric charge?

The short answer is that the conjugate relation simply reflects the perspectival nature of the macroscopic properties of 'momentum' and 'position' when applied to microphysical systems, coupled with our inability to know the relevant future boundary conditions.

I have proposed that neither momentum nor position is an intrinsic property of the lower-level microscopic system in either of the duplicate worlds, the two being rather epiphenomena – macrolevel projections or 'shadows' arising out of the elements of the hypothetical more fundamental lower-level process that has been proposed, namely the negative energy catastrophe. There is a sense in which each exists only as a *construct* of the experimental set-up, which is necessarily *macroscopic*. An indeterminacy of the momentum or position, or any mix of the two, simply reflects our ignorance of the one half of the boundary conditions (those conventionally taken as relating to the future) necessary to make arbitrarily accurate predictions of the microphysical system's fate. However, owing to the macrophysical level at which we operate, and the macrophysical concepts we have – such as that of the *trajectory* of a physical object (presupposing the concepts of determinate position and momentum) – we find it natural to think in terms of a deterministic causal picture. But such an intuitive causal picture breaks down when it is applied to microsystems owing to (in the present proposal) our inability to take into account the effect of future causal determinants. The breakdown is reflected in the indeterminacy relations, which are expressed in causal language, in terms of the relevant macrophysical properties (momentum and position in the case of physical trajectory[42]). This is the case even in an advanced action picture. However, even though the concept of trajectory needs both the concepts of momentum and position, it does not need both the concepts of, say, momentum and electric charge. The latter is not necessary for the concept of trajectory. Hence there is no indeterminacy relation between momentum and electric charge. Quite generally, we find it natural to conceive of the total indeterminacy in terms of some *mix* of indeterminacy of momentum and position, or energy and time, or some other *natural pair*, depending on the experimental set-up) even though these natural pairs are macrophysical projections of the paired conjugate elements of the underlying oscillation, namely frequency and period.

Take the present advanced action proposal. According to the proposal, if we have measured the sharp value of either one member of any pair of conjugate complementary properties, the value of the other member of the conjugate pair is always determined by the backward history of the particle in the duplicate

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[42] Note that momentum, rather than velocity, is the conjugate of position in the case of the trajectory of a physical object. That's because a physical object has mass-energy, which varies relativistically depending on its speed. Even a photon has mass-energy, though no rest-mass-energy. All of its energy is due to its speed.

world, and remains unknowable to us. Even though it remains unknowable, we may say that it *exists*, nonetheless, just because its present value is ultimately determined by the system's boundary conditions – even if these are *future* boundary conditions. (Why do we require *both* properties to have sharp values even if one is unknowable? Because that's what 'fully causal' *means*. A system is fully causal only if sharp values of both conjugate pairs of properties exist, whether we can know them or not.) Analogous considerations apply in the case of a measurement of any mix of the pair. However, it needs to be borne in mind that in the present proposal the elements of the mix are not primary, as they only reflect the elements of the more basic underlying process, namely the oscillation between the positive and negative energy levels.

The hypothesis that momentum and position are macrophysical constructs inapplicable to microphysical systems is consistent with the fact that knowledge of either member of the pair of conjugate non-commuting properties (such as momentum and position), or any mix of the two, can be made to emerge at will, even retrospectively as in the Wheeler delayed-choice type of experiment (§§1.2[e], 3.5, 5.1.2), depending on our experiment, with the consequent 'destruction' of our knowledge of the other (or of the previous mix of the two).[43] A sharp measurement of one *destroys* previously obtained knowledge of the conjugate member of the pair, as we have seen.[44] Even when the measurement isn't a sharp one, e.g. when there is an indeterminacy in the *mix* of momentum and position, the minimum *total quantity* of what is unknowable, determined by h, remains identical in an ideal measurement to what it would have been had the measurement been a sharp one.

The fact that the particular indeterminacy relation we have chosen above is that of momentum-position isn't particularly important, as there exist any number of such conjugate relations, one for each classical dynamic variable (Appendix[h]). Again, this is just what one would expect on the view that the classical dynamic quantities are projections or 'shadows' of the proposed lower-level process.

A consequence of the above view would seem to be that even the traditional primary qualities of matter have much of the nature of secondary qualities when applied to matter on the microscopic level. That isn't to say that specifically human observers are necessary for their existence – or Schrödinger cats either; rather, their nature and distinctive character presupposes the existence of suit-

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43 One is reminded of Bohm's 1951 *potentiality* interpretation of the classical dynamic properties in his orthodox period.

44 A *sharp* measurement means an accurate measurement of some *specified* higher-level quantity, be that momentum or position. Here the specified quantity was the momentum. But of course, the sharp measurement could equally have been of the position, in which case the unknowable higher-level quantity would have been the momentum.

able macroscopic observing equipment. The traditional classical dynamic qualities of matter are qualities existing *only* on the macroscopic level, and even there they are not quite objective, as shown for example by the delayed-choice type of experiment, at least in the sense in which primary qualities are traditionally held to be objective. However, the elucidation of the distinction between 'objective' and 'subjective' is, as Price observes,[45] a thorny and as yet unresolved issue, and constraints of space prevent me from entering that debate. I merely note that it seems that Bohr was right, up to a point, with his doctrine of complementarity. Let me enlarge on that briefly.

According to Bohr, the classical dynamic properties of momentum and position do not belong to the electron itself, but to 'the entire measurement situation', being really relations between the quantum entity and the measuring device (Chapter 3). Upon measurement, one or the other or a mix of both emerges, depending on the experimental set-up.[46] According to the present proposal, too, there is a sense in which, from the perspective of an observer in either one of the duplicate macroscopic 'worlds', the properties of momentum and position, or some mix of the two, are relations between the quantum entity and the macroscopic measuring device – just because the (higher-level) properties are not intrinsic to the quantum entity, and the mix of the indeterminacy in the momentum and position reflects the existence of unknowable future boundary conditions. But the present proposal begins from where Bohr left off, giving a 'hidden variable' account of this peculiar state of affairs in terms of a more basic underlying process, the negative-energy catastrophe, whereby the electron is enabled to have its state determined by both past and future boundary conditions.

There is still more to be said. What about the fact that a *retrospective* trajectory for a microphysical particle can always (in principle) be worked out. *Did* the particle possess such a trajectory? We noted in §2.5.2.2 that for Heisenberg it was a matter of 'personal belief' whether such a calculation concerning the past history can be ascribed physical reality.[47] This question may be rephrased as follows: As viewed by an 'Archimedean' observer, *does* the particle possess such a trajectory in either one of the two *non*-Archimedean worlds? From our anthropocentric (non-Archimedean) point of view, it is tempting to say that once both the past and future determinants have finished doing their work, i.e. when the past is well and truly fixed, the particle can be said to *have possessed* a trajectory. Of course, the trajectory must be worked out in such a way that it takes into account all the interactions and interference processes the particle undergoes in-

45 Price 1991a, pp. 111-44.

46 As Heisenberg later explained, 'the decisive step', for both Bohr and himself, 'was to see that all those words we used in classical physics – position, velocity, energy, temperature, etc. – have only a limited range of applicability... [they] lose their meaning when we get down to the smallest particles'. (Buckley & Peats 1979, pp. 6-7.)

47 Heisenberg 1930, p. 20.

between its start and finish, including its 'zitterbewegung' arising from the existence of the zero-point energy of the vacuum (reinterpreted in terms of the present model). The ensuing retrospective trajectory can be very complicated and counterintuitive indeed.

However, it is by no mean clear that there ever is a single instant when, in the frame of any non-Archimedean observer, the future determinants *have* finished doing their work (when the relevant accessible past and the past have no overlap). Consequently, it is by no means clear that the retrospective trajectory calculated at any one instant is *the* trajectory, rather than a construct with no objective reality. It likely that one would need to *be* a truly Archimedean observer (i.e. to be able to see *all* of the past and *all* of the future) in order to be able to able to determine even the *past* trajectory of a microphysical particle, let alone the future one, in either of the two non-Archimedean worlds. If so, our knowledge of the past and future trajectories are on an equal footing.

6.9 The relation of de Broglie waves to the proposed process; reinterpreting the QM wave function

What is the relation of the de Broglie or matter waves of the standard interpretation to the process proposed in the present model? The question is important not only in its own right, as one the theory ought to be able to answer, but also because we want to know if the collapse of the wave function is an artefact of the standard interpretation. One question is likely to give a handle on the other. We saw in Chapter 1 that de Broglie's relation $\lambda = h/p$ connects in a single equation two apparently incompatible states of matter, namely being a *wave* (having the wavelength λ) and being a *particle* (having the momentum p). The corresponding equation for the frequency of the matter wave is $\nu = E/h$, and the wave velocity of matter is given by $V = c^2/v$. In Chapter 1, I also described the relation of de Broglie waves to the wave function in the standard interpretation, noting that Schrödinger's equation simply specifies the laws of de Broglie wave motion which the particle of any microscopic systems obey. Furthermore, when the wave function collapses in the standard interpretation, it is the superposition of de Broglie waves (the wave packet) which collapses. An obvious way, then, to shed light on the question of whether the collapse of the wave function is an artefact is to determine if the de Broglie waves themselves are an artefact.[48] A way to do this is to see if the work that they do is done by some other process – in the present case by advanced action.

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[48] Even though there are other, more abstract mathematical formalisms of quantum mechanics than that of Schrödinger, they, too, contain the equivalent of the wave function, namely the state vector. If we can show that the basis of the Schrödinger formalism is an artefact, that would be strongly suggestive that the state vector of the other formalisms, too, is an artefact.

Historically, de Broglie postulated matter waves for reasons of symmetry. (If light waves can exhibit particle-like properties, then why cannot particles exhibit wave-like properties?) But his hypothetical waves quickly became the basis of the new mechanics (initially called *wave mechanics*) not so much because of the symmetry but because of their explanatory power – the work they do. This work they do (through Schrödinger's equation and Born's interpretation of de Broglie waves) is, in Wigner's words, 'to furnish probability connections between results of subsequent observations carried out on a system'.[49] Now, this is just what a classical probability function does. But we have seen that there are significant differences between the classical probability function and the wave function of quantum mechanics, namely the closely related nonlocality and interference properties of the latter. Additionally, de Broglie waves permit a linear superposition of states, and are complex. For a discussion of all these, see Appendix(d)(e)(k).

Now, all these properties seem to be entailed by our proposed process. The probability connections of course arise owing to one half of the determinants of a microphysical system's fate being in its future, from the point of view of a macrophysical observer. Or take the nonlocality. A special property of de Broglie waves is to connect over spacelike distances two or more distant systems which have once been connected. Additionally, de Broglie waves define an instantaneous frame, and can be interpreted as 'waves of simultaneity'.[50] Neither is in principle problematic for de Broglie waves (given the collapse of the wave function) since they always travel faster than light (in the stationary limit with infinite velocity and wavelength).[51] But we have seen that in the present model, the duplicate, backward-in-time world line associated with every particle (advanced action) does all that: advanced action readily provides an instantaneous frame. So there is no need for de Broglie waves on that score.[52] As for the characteristic quantum-mechanical interference, that is ubiquitous in the present model, owing to the existence of the interacting and interfering negative and positive-energy states associated with each particle. No need for de Broglie waves on that score either.

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49 Wigner 1967, p. 166.

50 For details, see Rindler 1977, pp. 90-1.

51 We're talking about the phase velocity, of course; the group velocity is always associated with the particle. As regards the stationary limit, that is an idealization: strictly speaking, there are no stationary particles owing to the zero-point energy of the vacuum.

52 In the case of de Broglie waves, there is, of course, a dropping off of the speed of connection with increasing velocity (as v goes to c, V drops to c), so that the connection is no longer instantaneous. But that simply follows from the relativity of simultaneity, which has its mathematical formulation in the Lorentz transformations. In the present process, too, the same relation applies for the same reason, i.e. the distant systems are connected by Lorentz transformations.

Or take the fact that de Broglie waves permit quantum-mechanical systems to exist in a coherent linear superposition of all possible states. But the present proposal explains the state of linear superposition as an artefact of the standard interpretation, in that it simply reflects the system's future boundary conditions (e.g. the angle of a future polarizer, or a Stern-Gerlach magnet) which act back on the system to determine its state when its is measured. The *ensemble* of possible results of a single experiment arises because unknowable future boundary conditions (the proposed hidden variables) contribute to the result. An example is given in §6.10.1. We may talk of a 'state of linear superposition', provided we realize that it is just *façon de parler*, a convenient shorthand for the more complex and richer picture.

To give an illustration, according to the standard interpretation, the only way a particle can be sharply localized (in the Schrödinger representation) is by associating it with a wave consisting of an infinitely large number of superposed sine waves, differing infinitesimally in λ and ν, the combination of which gives a group wave having an infinitesimal spread in space (see Appendix[h]). The group constitutes a single sharp pulse, the time of arrival of which can be known with certainty ($\Delta t = 0$). Hence we would thereby also know with certainty the *position* of the particle associated with the wave pulse ($\Delta x = 0$). But in this case, the superposed waves would have wavelengths and frequencies ranging from zero to infinity. It follows that we could know nothing about the frequency of the pulse itself ($\Delta \nu = \infty$).[53] That being the case, de Broglie's relation $\lambda = h/p$ tells us that we could know nothing about the *momentum* of the particle associated with the pulse ($\Delta p_x = \infty$). Its state involves a superposition of all different momenta. But the same conclusion, the unknowability of the momentum, follows directly from the present proposal owing to the perspectival nature of 'position' and 'momentum', and the fact that one half of the determinants of a system's state belong to its future. Hence, if one half is known (i.e. the position in this case), then the momentum is necessarily unknowable.

Or take the curious property of de Broglie waves that their wave magnitudes – wavelength, wave number and phase velocity, which depend on the momentum p of the associated particle – are defined only at the place where this momentum has a meaning at the instant considered, i.e. at the location of the particle (as presumably revealed by a measurement). Elsewhere in space, these magnitudes, and so the wave itself, are left undetermined.[54] This, too, is readily understandable in present picture, since according to that picture, the 'wave' is nothing more than the 'particle' itself oscillating between the negative and positive energy levels, connecting past and future boundary conditions, with the de Broglie wave properties of matter emerging out of this process, just *as if* there were such

53 Resnick 1972, p. 187.
54 D'Abro 1951, p. 609.

waves.[55] This has the consequence that the connection between distant entangled systems is always 'local', within a light cone along the world line of the particle, just as in Price's proposal.

This also explains why de Broglie waves transmit no energy, being in that regard unlike most other waves. De Broglie waves are an artefact of the standard interpretation. As Price tells us, it is the particle itself that connects two distant regions, and is the only transmitter of energy between them.

Why, then, retain de Broglie waves as ontological elements of reality? I propose that they be reinterpreted (away) in terms of the present proposed process. But to reinterpret de Broglie waves in this way is to reinterpret the quantum-mechanical wave function and its collapse (in the Schrödinger formalism), and to go to an 'ignorance' interpretation of it. In such an interpretation there are no overt spacelike influences nor collapse of the wave function. A 'measurement problem' of sorts remains, in that it remains impossible to specify the state of an unmeasured quantum object in classical terms. However, that is exactly what one would expect given the nature of the proposed solution, and it poses no serious conceptual problems of the kind that are notorious in the standard interpretation.

6.10 Why things seem to happen in QM on the basis of 'insufficient cause': applying the present model

6.10.1 Resonance radiation: the emission & absorption of light

Consider the decay of an excited atom A to a lower energy state. The atom emits a photon, say at time $t = 0$. According to quantum mechanics, the instant of emission, the direction of emission, and the energy of the emitted photon are intrinsically probabilistic. The decay (once regarded as spontaneous) is usually taken to arise from the zero-point fluctuations of the quantized electromagnetic field. As for the origin of the vacuum fluctuations, they are a quantum field-theoretical consequence of Heisenberg's indeterminacy principle, which is itself a consequence of wave-particle duality. How does the present model account for the emission details in terms of hidden variables? Here is how.

First, suppose that the emitted photon is absorbed by some atom B somewhere. Say the absorption occurs at time $t = 1$.[56]

Since the sign of energy is conventional, the duplicate (negative energy) world arising from the negative-energy catastrophe is a world just like ours – a

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55 Alternatively and, it would seem, equivalently, we could say that the particle is a standing wave (of electromagnetic radiation) between the two worlds of the negative and positive energy solutions. See §6.4.

56 It is a consequence of the present model that the universe is 100% absorbing. There can be no emissions in the absence of absorptions, and vice versa.

'mirror' image (*CPT* conjugate) of the positive-energy world, differing from a mirror-image in being real, unlike a mirror image. The same processes occur there as in our world. In that world, too, atom A (i.e. the negative-energy counterpart of the positive-energy atom A) emits a photon which is absorbed by atom B (the negative-energy counterpart of the positive-energy atom B). Both the emission and absorption are intrinsically probabilistic within that world.

Even though the sign of energy is conventional, there is nothing conventional about the *difference* in the signs of the energies between the two worlds. Each is the opposite of the other. Each world is also a backward-in-time world in relation to the other. Overall, all the statistical tendencies of the one world would be reversed in the other. Disordered systems would tend to become more ordered, radiation would tend to converge onto objects and heat them, in stars the absorbed heat would go into reversing their characteristic nuclear transformations, heat would in general flow from cold bodies to hot and entropy would decrease.[57] In respect of individual processes, every process in the one world would have its exact time-reversed counterpart in the other. Consequently, in the frame of an observer in our positive energy world, such as a theoretical physicist investigating the apparently probabilistic decays and excitations of atoms, that absorption by B at $t = 1$ is also consistent with an emission by B at $t = 1$ of an advanced photon of negative energy. In that picture, the advanced photon is absorbed by atom A at $t = 0$, which is raised into a higher (–) energy state. Our physicist perceives its extra (–) energy as the loss of (+) energy. In other words, since the absorption of (–) energy is equivalent to the emission of (+) energy [the presence of (–) energy is equivalent to the absence of (+) energy], the physicist perceives this raising of the negative-energy atom A into an excited state as the decay from an excited state of atom A – which accordingly emits a retarded photon of (+) energy, which travels to atom B. (The sequence is atemporal: neither leg of the transaction may be said to objectively occur before the other.)

In the frame of observers in either world, both the emission and absorption details of the photon (including the time, direction and energy of emission) remain intrinsically probabilistic when having regard only to that world. Yet the chance absorption of a photon by atom B in the one world acts back via the duplicate world on the emitter atom A to determine the latter's complete emission details. And vice versa. The *same* atom, in its two guises, both absorbs a negative energy photon from the 'future' and emits a positive energy photon toward the 'future'. This is the causal 'bootstrap' mechanism of atomic decay I propose. It seems to mesh with the quantum-mechanical rule $\langle B|A\rangle = \langle A|B\rangle^*$, which says that the probability amplitude for a normalised system of finding it in the state B (e.g. an eigenfunction of energy) given that it starts in the state A is the same as the complex conjugate (or time-reflected) probability amplitude of

57 See e.g. Gold 1965, pp. 143-65.

finding the system in state A given that it starts in the state B.[58]

The other-world emission and absorption is the hidden variable necessary for a fully causal account of the decay and emission by atom A in our world. An identical account, with absorptions and emissions interchanged and signs of energy reversed is given for the decay of atom A in the duplicate world.

That is how atoms get their marching orders as regards decays and absorptions from both past and future boundary conditions, and are thereby enabled to 'know' exactly how to behave – even though their behaviour *looks* intrinsically probabilistic to us non-Archimedean observers, consistent with an interpretation in terms of a statistical ensemble of identically prepared systems. We have seen how this picture naturally leads to an ensemble interpretation.

It may be asked: why postulate the above real duplication of the world? Why not just say that atom A decays at $t = 0$ because the emission by it of the photon of positive energy, which is absorbed by atom B, is equivalent to the absorption by A at $t = 0$ of an advanced photon of negative energy from B? The reply is that this explanation seems merely verbal. Owing to the logical equivalence of 'emission of positive energy' and absorption of negative energy', it seems no more than just another way of saying the original thing in different words. (The same kind of thing could correctly be said of every classical system. For discussion, see §5.3.) In contrast, the present proposal goes well beyond mere logical equivalence, postulating a real duplication of the world and a real process of interaction between the duplicate worlds with measurable consequences (e.g. the virtual photon cloud about each electron), and bringing together hitherto seemingly disparate phenomena within a single explanatory framework.

Recall that it was Dirac's *interpretation* of his equation that made possible his bold prediction of the new particle now known as the positron. Dirac's interpretation turned on taking an *unobservable* – the negative energy sea – to be real. Furthermore, Dirac reasoned that the *absence* of a bit of this unobservable amounted to the *presence* of a *something* that was observable! Now, if the expressions 'absence of negative energy' and 'presence of positive energy' were just two ways of saying the same thing, there would have been little point in Dirac's postulation of his negative energy sea and *holes* in the sea which we could actually see as real particles. He would have been uttering little more than trivial tautologies. The fact is, Dirac's postulation of an electron moving up and down between positive and negative energy levels was something over and above the bare mathematical bones of the formalism of his equation, giving significance to the existence of a difference in meaning of two (apparently) equivalent expres-

58 The probability of B following A is given by $\langle A|B\rangle\langle B|A\rangle = \langle B|A\rangle^{*}\langle B|A\rangle$. In this connection, see for example some remarks by Eddington (1928, pp. 216-17n); see also Cramer 1988, p. 229.

sions, expressions whose only difference *seemed* to be verbal. It is the *context* that ultimately determines the meanings of expressions in our language. And it is our *physical theory* that provides the required context – and thus tells us what is 'real'. So also in the present case. As Einstein once told the young Heisenberg, what is observable or not [and therefore real] is not for us to decide but for our theory.[59] (Lord Dunsany said as much more informally when he remarked that 'Logic, like whiskey, loses its beneficial effect when taken in too large quantities.')

Another possible way of describing the above bootstrap mechanism is as follows. The absorption of a negative-energy photon by atom A causes A to decay to a lower energy level (the details of the transition depending in part on the energy of the absorbed photon), and emit a photon. (So far this is just as above; the new part starts here.) That emitted photon *is* the negative-energy photon, which is now of positive energy and which has tunnelled permanently into our world from the negative-energy world through the mediation of the atom. We may also (less generally) regard the above process as a reversal in time of the negative-energy world photon's world line.

Yet another way of thinking about the above process is in terms of vacuum fluctuations and the negative-energy sea of Dirac's hole theory. Every quantum particle is enveloped in a cloud of virtual photon pairs and other quanta. The same applies of course to the absorbing/emitting atom described above. Using the language of Dirac's hole theory, we might say that a vacuum fluctuation is the 'momentary' presence of a virtual photon *pair* in our frame. The pair consists of a negative-energy sea photon that has tunnelled into our frame by virtue of the uncertainty principle (which 'smears out' over small scales the metric of space-time including the delineation of light cones), and the *hole* in the negative energy sea left by it – which we'd 'see' as the presence of a second photon, effectively of negative energy as considered by us. In the absence of a mediating atom, the virtual photon pair could exist only for the time permitted by the uncertainty principle before mutually annihilating (i.e. before the photon which tunnelled into existence drops back into the negative-energy sea). In the present case, where there *is* a mediating atom, the negative energy virtual photon (the hole) is absorbed by the atom, causing the atom to decay (a process strikingly analogous to the process resulting in the reduction in the surface area of a black hole as it emits Hawking radiation), and the positive energy virtual photon flies off, no longer virtual, as the real resonance radiation emitted by the atom. The dropping of the atomic electron to a lower energy level is just another way of describing the absorption of the negative energy photon. And we can think of *that* process as the time-reverse of the emission of the (positive-energy) photon by the atom as it decays to a lower energy level. (The converse of the process occurs when an atom absorbs a photon in our frame.)

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59 Rosenfeld 1979, p. 23.

The consequence of the above picture is that atoms in our world receive their 'instructions' on when and how to decay from processes occurring in the negative-energy world. Likewise, atoms in the negative-energy world receive *their* instructions on when and how to decay from processes occurring in our world. Seeing that macroscopic matter is made up of microscopic matter, the existence of both worlds appears to be necessary for anything to happen at all in our world – the two worlds being inextricably bound, in the sense that the one world provides one half of the boundary conditions for the existence of the other, and vice versa. This proposal is tantamount to showing how *future boundary conditions* are as important as past boundary conditions for determining the fates of particles, in line with the project of Sciama and Price. Additionally, it shows something of the *nature of the physical connection* between future and past boundary conditions.

A consequence of the proposal is that it restores symmetry in the boundary conditions. The low entropy extremity at the beginning of the world is a high entropy extremity 'on the other side', in the duplicate world, and the high entropy extremity at the end of the world is a low entropy one on the other side. The state we call the 'big bang' is both extremely smooth and orderly and extremely unsmooth and disorderly all at once, as is the state we call the 'big crunch'.

The proposal also resolves the vexed question of whether the quantum wave function is a complete description of a quantum system (such as an electron), or whether quantum systems represented by identical wave functions can differ. If we limit our description of the world to either one of the duplicate 'worlds' alone, the wave function is indeed a complete description of the system. But as soon as we bring in the other (negative energy) 'world', we see that the description is incomplete, and consequently it must describe an ensemble in the sense of a classical distribution of probabilities. In particular, the quantum-mechanical conception of a 'pure state' is always a mixture of states, and quantum mechanics simply determines the probabilities with which the different eigenfunctions occur in the mixture, and are picked out by measurement.[60] As Einstein once

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60 As we have seen, according to the standard interpretation, the wave function contains a complete description of the quantum system associated with the wave. If so, all quantum systems represented by an identical wave function are physically identical, and their different measured behaviour must arise from an intrinsic quantum indeterminacy. A collapse of the wave function is required.

Other theorists reject the orthodox interpretation. They maintain that the different measured behaviour of quantum systems represented by an identical wave function arises from the fact that the quantum systems were physically different before measurement even though described by identical wave functions. The wave function fails to represent (save in the average) certain parameters or 'hidden variables' which also need to be taken into account in representing the complete state of the system – but which are omitted in the formalism. This omission is offered as the explanation of the statistical character of quantum mechanics, i.e. why the wave function accurately put it, '...the whole 'egg-walking' performed in order to avoid the 'physically real' becomes superfluous'.[61]

6.10.2 Quantum entanglement & EPR; no violation of the spirit of SR

In the standard picture, distant systems can be instantaneously connected by de Broglie waves – which may be regarded as 'waves of simultaneity', as we have seen.[62] In the present picture, advanced action does the connecting. More specifically, it is the proposed oscillation between the negative and positive energy levels that instantaneously connects distant systems, doing the same work in this regard as the de Broglie waves, without, however, the spacelike influences of the latter. Spacelike influences are avoided because (as we have seen) the locus of the connection always travels with the particle. In this respect the present proposal is much like Price's proposal, and not unlike de Broglie's initial hypothesis.

For the above reason, neither the entanglement of distant, previously connected systems, nor the delayed-choice aspects of the EPR or other quantum experiments appear to present *in-principle* difficulties for the present account. By the same token, there is no violation of the 'spirit of SR' in its account of EPR. Such a violation is implicit in the standard interpretation's account of EPR, as has been pointed out by e.g. John Cramer, Paul Dirac, Roger Penrose, Huw Price, Mendel Sachs, Abner Shimony. For details, see §4.6.[63]

In brief, the 'violation' refers to the fact that, in a collapse interpretation, the properties of momentum and position are frame-dependent in the case of a pair of spacelike separated measurement events. (Quite generally of course, in any relativistic theory, when particles are spacelike separated, the order of events is frame-dependent, simultaneity being a concept that needs relativity for its elucidation). That being so, the standard theorist is likely to run into consistency

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predicts only the behaviour of an *ensemble* of quantum entities and not that of individual quantum entities. In this view, the collapse is an artefact of the interpretation.

The present proposal resolves the dispute by showing that the question as posed is ambiguous. It fails to specify whether by 'complete description' we mean the description of the system just in our own 'world', or a description that includes both worlds. If we restrict the scope of the term 'complete' to a theoretical description of the system just in our own world, then all quantum systems represented by the same wave function are indeed identical and the wave function contains a complete description of the systems within the limitations that we've imposed. But if we also bring in the duplicate world into our theoretical picture, we see that quantum systems prepared in identical states are in fact physically different before measurement even though described by identical wave functions. That's because the wave function fails to represent (save in the average) the *complete* relevant system – namely the paths and momenta of the duplicate-world photons which are absorbed by duplicate-world atoms and which result in the observed phenomena in our own world.

61 Einstein 1949b, p. 672.

62 Rindler 1977, p. 91.

63 The problem has been carefully described by Price (1996, pp. 204-6).

problems unless (a) there is a privileged reference frame, or else (b) a separate wave function applies in the case of each observer. However, the existence of a privileged frame is inconsistent with the Hamiltonian eigenvalue form of quantum mechanical equations in the other Lorentz frames. As for (b), it seems to bear little resemblance to the usual standard-interpretation understanding of the wave function.[64]

No such problem arises in the present proposal, since according to it neither momentum nor position are intrinsic properties of microphysical systems, and the collapse is an artefact of the standard interpretation. Microphysically, there is just the underlying oscillation. We saw how the quantization of energy arises (§6.7), and the macrophysical quantum complementarity along with the Heisenberg indeterminacy relations (§6.8). We noted the Lorentz invariance of h pursuant to the present proposal. However, its individual elements, energy and time (or momentum and position), are expected to vary relativistically from observer to observer depending on the state of motion because the more basic elements, frequency, period, etc. of the underlying process do so.

The situation is reminiscent of that in the Feynman propagator theory, in which, owing to the frame-dependence of a sequence of events, 'one man's... particle is another man's... antiparticle'.[65] In the present proposal one man's *momentum variable* p is another man's *position variable* q. Neither is an intrinsic property of quantum systems, but rather perspectival in Price's sense of the term, involving both conventionalism and objectivity in an essential way (see §4.8).

6.10.3 Quantum-mechanical self-interference

The present proposal gives promise of being able to explain the characteristic self-interference of quantum-mechanical systems in a way that is consistent with the state of a system being determined by both past and future boundary conditions. Take an electron. According to the present proposal it exists in both positive and negative-energy states, these states being associated with past and future boundary conditions, respectively. And since its 'path' may also be pictured as a pair of interacting world lines propagating in opposite directions, with opposite signs of energy, interference possibilities arise. I shall not try to give the full details here, owing to considerations of space.

6.10.4 Pair-production; the zero-point energy of the vacuum; zitterbewegung

Pair-creation occurs in the present picture much the same way as in Dirac's hole theory. In the latter, it is possible for an electron in the negative-energy sea to

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64 Cramer 1986, p. 657.

65 Feynman & Weinberg 1987, p. 10.

absorb radiation and be excited into a positive-energy state, emerging as an ordinary electron. The resulting hole in the negative-energy sea is interpreted by an observer relative to the vacuum as the presence of a particle of positive energy and positive charge and opposite spin, i.e. a positron. This is the creation of a particle-pair.

In the present picture, Dirac's negative-energy sea of electrons is replaced by individual negative-energy electrons, one per each positive-energy electron. There is thus an absence of the Dirac negative-energy sea. But sometimes it is useful for heuristic purposes to go to an account in terms of something like Dirac 'seas' of particles, bearing in mind, however, the elements of the present picture. The electron we see in our world is equivalent, in terms of Dirac's picture, to a hole in a Dirac negative-energy sea of positrons.[66] Likewise, its negative-energy counterpart is equivalent to a hole in a positive-energy sea of positrons. There are two seas of positrons, of opposite energies, with a pair of corresponding holes in them (the holes representing the electron of our experience).[67]

It is an accepted principle that a state of uniform charge or energy or pressure everywhere is undetectable. Only variations in it are detectable.[68] So the existence of the two seas does not conflict with our usual experience of the world. Nonetheless, because a description in terms of the two seas is equivalent to, and therefore no less real, than the alternative description in terms of a pair of electrons of opposite energy, pair production can occur in either world in the manner envisaged by Dirac. A negative-energy positron in the Dirac sea of positrons may absorb radiation and be excited into a positive-energy state, emerging as a non-virtual ordinary positron. The resulting hole in the negative-energy sea is interpreted by an observer relative to the vacuum as the presence of a particle of positive energy and negative charge and opposite spin, i.e. an electron. We have the production of a negatron-positron pair. (Despite the existence of the almost filled seas in each world, there is always room for a particle to make the transition from one to the other, as the energy to raise it to the requisite energy state comes from the transformation into energy of existing members of the constituents of the sea.)

What is new in this picture, besides the trivial fact that the present discussion focuses on a negative-energy positron sea instead of Dirac's negatron one, is that there is one-to-one correspondence between members of the positive and negative-energy seas, each of which is undergoing the negative-energy catastrophe – the oscillation and the interference between the positive and negative-energy

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[66] Dirac 1935, p. 272.

[67] Instead of two Dirac 'seas', we may prefer to talk instead of two 'carpets' of positrons, of opposite signs of energy, with matching holes in each. This would be better usage as in the present picture each 'sea' is only one particle deep. However, the details will not be given here.

[68] Dirac 1935, p. 271; Gamow 1985, p. 130; Polkinghorne 1979, p. 73.

states earlier described in respect of the *single* pair of positive and negative-energy electrons (the holes in the present variation to the original picture). The process entails virtual-pair production, in just the same way as the process involving the single pair of electrons. (Regarding the latter, see §6.4 above.) Since this process is going on everywhere in what we regard as the *vacuum*, it may account for vacuum fluctuations.

One more thing. Take a pair of holes in the two seas. Consider first the hole in the positive-energy positron sea (which hole we see as an ordinary electron). A negative-energy positron from the negative-energy sea 'falls' up into the positive-energy sea and into the hole, filling it, but in the process leaving a hole in a slightly different location in the negative-energy sea. A member of the positive-energy positron sea falls into ~~this (new) hole~~, leaving a hole in the positive-energy sea – which hole we see as an ordinary electron. Again, there is a pair of holes in the two seas. These holes again correspond to the electron of our experience. But the electron is now in a slightly different location as a result of this virtual process. It has shifted position without actually moving in space. Zitterbewegung![69]

6.10.5 The cosmological constant problem

Recently James Woodward wrote, 'Even a casual reading of a smattering of popularisations of physics published in the past several years should convince one that a state of Kuhnian "crisis" now obtains.'[70] Oddly, though (he continues), the crisis hasn't come about as a consequence of the accumulation of experimental anomalies, as Kuhn suggested ought to be the case. By and large experimental results have borne out the predictions of general relativity and quantum mechanics. Rather, the crisis stems from our inability to reconcile the two theories in a single theory of quantum gravity. A consequence of this theoretical inability is the 'cosmological constant' problem.

Let us try to apply the preceding picture of the duplication of the world and the negative energy catastrophe to the 'cosmological constant' problem. That problem arises when we measure the background energy density of empty space. According to quantum field-theory this energy is not zero. Now, we know that gravity interacts with all forms of energy. Therefore a non-zero vacuum energy density ought to produce a gravitational field (space-time curvature). General relativity thus provides a means of determining the energy density of the vacuum by simply measuring the space-time curvature produced by it. To do so is to determine the magnitude of the cosmological constant, since one is proportional to the other. In other words, we can indirectly measure the vacuum energy density by simply observing by how much space is curved.

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69 In the standard account, the lack of a classical path for a charged particle is ascribed to virtual pair-creation. See Thirring 1958, e.g. pp. 6, 13, 79-86.
70 Woodward 1996, p. 956.

The theoretical value of the cosmological constant (as predicted by the standard model) is wildly at variance with the actual measured value of the vacuum energy density. It seems that a conservative theoretical estimate of the magnitude of the cosmological constant is such that if it were correct, the curvature of space-time ought to be readily apparent over distances of even a kilometre – with a concomitant breakdown of Euclidean geometry over such distances – which is obviously false.[71] The actual curvature is found to be zero – zero to an accuracy of one part in 10^{120} in units of Planck mass, the natural mass/energy scale of gravity – 'the closest experimental determination of a zero quantity we have ever come up with'.[72] Likewise, Larry Abbot writes that the theoretical estimate based on what our best theories tell us is incorrect by (at least) a factor of 10^{46}.

> Few theoretical estimates in the history of physics made on the basis of what seemed to be reasonable assumptions have ever been so inaccurate.[73]

Abbot speculates that maybe the problem arises simply because we've put too much into our quantum field-theoretical picture of the vacuum.

The duplication of the world we have proposed may offer a clue. In §6.10.1 it was suggested that one way of explaining the emission of photons by atoms in terms of the present model is that photons tunnel between the two 'worlds', the process manifesting itself in the creation of virtual particle pairs, and when properly interpreted, providing the missing half of the 'causes' of quantum events. It is also suggested that the origin of the virtual photon cloud about every electron may be traced to the negative-energy catastrophe, and that an analogous process occurs in the case of all other particles (all quantum fields), bringing about the existence of the virtual quanta appropriate to the field in question. In §6.10.4 I extended the present picture to the vacuum itself, with like consequences. Now, it is just the existence of such virtual quanta that is responsible for the cosmological constant problem, each pair increasing the mass-energy of the universe by $2m_oc^2$, where m_o is the mass of the particle in question. The solution suggested by the present picture is as follows. There are *two* such negative-energy catastrophes going on, one in each of the two 'worlds' or duplicate universes of the present model, *each* giving rise (in terms of the standard picture) to an appreciable zero-point energy of empty space and so to a non-zero curva-

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[71] The cosmological constant = $8\pi G/c^4$ × vacuum energy density. 'Defined in this way, the cosmological constant can be assigned units of 1 over distance squared. In other words, the square root of the cosmological constant is a distance. This distance has a direct physical meaning. It is the length scale over which the gravitational effects of a nonzero vacuum energy density would have an obvious and highly visible effect on the geometry of space and time.' Abbott 1991, p. 74.

[72] Gross 1988, p. 145.

[73] Abbott 1991, p. 78.

ture of empty space. But the mass-energies of the two worlds are of opposite sign. If so, there are *two* curvatures of opposite sign. The two curvatures are 'back-to-back', as it were. They cancel out, yielding a zero overall curvature.

In brief, then, the tentative solution to the cosmological constant problem that I propose is that the underlying process responsible for the zero-point energy of the vacuum is the momentary tunnelling of a particle from the duplicate (negative energy) universe into our universe. We 'see' this particle and the hole left by it in the negative energy universe as a virtual particle pair. The corresponding process also occurs in the other direction: a particle from our universe tunnels into the duplicate universe, being 'seen' there as a virtual particle pair. There is perfect symmetry between the two processes, the vacuum energy generated by the one being exactly offset by the other.

6.10.6 Restoring symmetry in the boundary conditions

Modern cosmology traces the temporal asymmetry of the world (including the temporal asymmetry of the second law) to cosmological origins; it is a consequence of the fact that entropy was very low soon after the big bang. According to Price, though, cosmologists haven't really taken on board the lessons of Boltzmann, in that they haven't yet learned to think in an *atemporal* way; yet it's *only* by thinking in an atemporal way that we can ever hope to understand time's arrow.

Price has examined recent attempts by cosmologists to understand why entropy was so low in the early universe – that's to say, why the matter in the early universe was arranged in an extraordinarily smooth way given that it is a system dominated by gravity. He notes that the usual current objection to the Gold universe (which is smooth at both ends) is that it would require incredibly special and unlikely initial conditions to bring about its smooth, low entropy *final* state – such conditions needing to have been especially 'orchestrated' or programmed to bring about that special state – which programming would then need to be explained. But as he rightly points out, exactly the same can be said about the special conditions obtaining in the early stages of the expanding Friedmann universe of the standard model. The *early* universe is incredibly special and 'unlikely', obviously needing to somehow have been 'programmed' to bring it about... Yet *it's there*... So why can't the smooth second half of the Gold universe also be there?

This is a debate I'm unable to enter into here owing to limitations of space.[74] I simply point out that the present proposal has the consequence that not only the laws but also the initial and final boundary conditions of the universe are

[74] All kinds of issues arise, including the question of whether the boundary conditions *need* explanation (Callender 1998, p. 149).

symmetric.[75] The low entropy extremity at the 'beginning' of the world (in either of its two guises) is a high entropy extremity 'on the other side', in the duplicate world, and vice versa. Likewise, the high entropy extremity at the 'end' of the world is a low entropy one on the other side, and vice versa. The state we call the 'big bang' is both extremely smooth and orderly and extremely unsmooth and disorderly all at once – as is the state we call the 'big crunch'. In effect, there is a smooth second half of a 'Gold' universe hidden behind the unsmooth second half of the standard-model universe in either of the two worlds.

Moreover, if the initial singularity is a white hole, and a recollapsing universe collapses into a black hole in the big crunch, then the black hole is also a white hole, and vice versa. There is much more I could say on this subject, but space prevents.

Summary

The question I have set about answering in this chapter is, can a physical 'mechanism' be found that would enable backward causal influences to be transmitted along the world lines of particles in the same way that they appear to be transmitted in the case of forward causation? There appears to be such a mechanism. It is the 'negative-energy catastrophe' that Dirac's hole theory was designed to combat.

The account given seems to have notable advantages, in that it not only restores symmetry to the initial and final boundary conditions of the world, but also allows the interpretation of Planck's constant in terms of advanced action. I have argued that to explain Planck's constant is to interpret quantum mechanics, and vice versa. I have also suggested that there are at least five things that a realist advanced action theory must explain about to h. (1) Its magnitude. (2) The mysterious relation between energy and frequency entailed by h. Just what does the frequency refer to?[76] (3) The quantization of energy in multiples of h. I've noted in §1.1 that even though the quantization of energy is central to quantum mechanics, it is not well understood. (4) The Lorentz invariance of h, i.e. why h is a *constant*. (5) The relation between h, advanced action, and the missing 'hidden variables' sought by Einstein. If advanced action is to play the main part in a local hidden variable interpretation of quantum mechanics, h must be a measure of our principled ignorance of the future boundary conditions determining the fates of systems. (Regarding this point, we have seen that Sciama noted that the world can be fully deterministic if half the necessary boundary condi-

75 The laws are *CPT* symmetric, not *T* symmetric.

76 Recall that it is the relation between energy and frequency that is mysterious, and not the concept of action itself. Planck's constant arises because such a relation requires a proportionality constant, and h is that constant.

tions for arbitrarily accurate predictions refer to the past and half to the future of an arbitrary moment t.)

The trick, then, is to come up with a physical theory which uses both past and future boundary conditions to determine the fates of quantum mechanical systems and to account for h, relating the two. As Sciama put it, if quantum mechanics is deducible from a more basic theory, then presumably h will be expressed in such a theory in terms of quantities fundamental to the basic theory. With an advanced action theory, the quantities fundamental to the basic theory are advanced action and future boundary conditions. If these could be brought in so that they act in 'symbiosis' with retarded action and past boundary conditions, they could play the role of the hidden variables of an Einsteinian realist theory. The analysis of bidirectional causation in Chapter 5 suggested the present model as a possible way of achieving this.

It seems that the present proposal goes some way toward an advanced action theory. It does not explain the magnitude of h. So there is more to be said. But it does appear to provide an explanation of (2), (3), (4) and (5) in terms of a single physical 'mechanism', namely the negative-energy catastrophe.

As for the catastrophe itself – all matter disappearing from the world into the negative-energy state – that is not a concern. We simply sit back and allow it to happen. The particles will soon be back as there are still lower energy levels for them to fall into. Those are the positive-energy levels, which, in their frame, are negative-energy ones. The oscillation of a particle generates the pair of temporally oppositely directed 'paths' that allow advanced action to do its work (in conjunction with retarded action).

Eddington once remarked that 'Philosophy seems to me full of half-finished sentences; and I do not know what to make of it.'[77] No doubt much that is in this chapter will strike the reader as fitting Eddington's description. The contents are far from being a fully worked out theory. Nonetheless, the proposal put forward seems to me to have heuristic potential. This potential lies in its ability to connect several very different and seemingly unconnected ideas and issues, sometimes in a quite specific way. The most notable of these are quantization and wave-particle duality, the negative energy solutions of Dirac's relativistic wave equation, Price's advanced action proposal and perspectival view of temporal asymmetry.

The proposal answers the question of what is it that 'waves' in wave mechanics. It shows how advanced action explains the mysterious quantization of energy, and how Planck's constant, Bohr's complementarity and Heisenberg's indeterminacy relations fall out of advanced action in a natural way. It gives an answer, in terms of 'hidden variables', to the question of why things happen in

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[77] Eddington 1935, p. 156.

quantum mechanics on the basis of 'insufficient cause', and to the related question of whether the wave function describes a single particle or an ensemble. It shows how matter gets its marching orders from both past and future boundary conditions, and is thereby enabled to 'know' exactly how to behave – even though its behaviour *looks* intrinsically probabilistic. It amounts to a 'hidden variable' interpretation of quantum mechanics. A consequence of the proposal is that not only the laws but also the boundary conditions of the world are symmetric.

Concluding Remarks

This is a short conclusioun.
(Chaucer, The Knight's Tale)

We now take brief stock of what has been argued in this thesis. We began by looking at the problem of the interpretation of quantum mechanics. That was partly because of Price's recent claim that the puzzling consequences of quantum superposition are just what we might have expected if we weren't so fully in the grip of a classical, anthropocentric world view. Might it be that the natural explanation of those aspects of quantum mechanics giving rise to the interpretative problem is that time is two-directional? If so, that would have ramifications for the more general problem of the direction of time. We also wanted to see if there is some feature of quantum mechanics that might be said to constitute the essence of the interpretational problem, having in mind Price's claim that advanced action may be the key. It turns out that there is indeed such a feature. That feature is Planck's constant, which plays a key role in the subsequent discussion.

An important part of the project of the thesis has been to provide a critical assessment of Price's claim that time is symmetric. In doing so, four main claims have been argued:

(1) The standard interpretation of quantum mechanics is philosophically unsatisfactory.

(2) Price's local advanced action strategy provides a natural heuristic for tackling the main counterintuitive aspects of quantum mechanics, namely its quantization of energy, complementarity, non-locality, and stochasticity.

(3) The collapse of the wave function does not render the standard interpretation of quantum mechanics time-asymmetric in a lawlike way (contrary to Price).

(4) Price's local advanced action proposal to interpret quantum mechanics is misleading in one important respect. The necessary adjustment to the required local advanced action proposal is made clear.

The first claim was shown to be true in Chapters 1 and 3, and the second in Chapters 4 and 5.

Chapter 2 contained a lengthy analysis of Price's assumption that the collapse of the wave function rendered the standard interpretation time-asymmetric in an intrinsic way. The analysis showed that the assumption is misguided. Hence the third claim.

Chapter 5 raised the important question of the proportion of forward to backward causation in the world, and the related question of whether the two types of causation are *alternatives*, or do they *coexist* in every microtransaction. If the latter, how do they manage to coexist? The notions of unidirectional and bidirectional causation were compared. A consequence of my picture, which is much like Price's, and yet seems to differ from it in an important way, is that there can be no ordinary forward causation without an equal amount of backward causation being involved in the causal transaction, and vice versa, at least on the level of atomic transitions. For example, the ability of an atom to both absorb and emit photons depends crucially on the existence of past and future emitters and absorbers. Additionally, Price's advanced action proposal seems to entail that all emitted particles are absorbed. As a consequence, Price's local advanced action proposal to interpret quantum mechanics is misleading in one important respect; an interpretative dilemma for Price was revealed – hence claim 4.

In Chapter 6 an advanced action heuristic proposal was put forward. It answered the question of what is it that 'waves' in wave mechanics. It also gave an answer, in terms of 'hidden variables', to the question of why things happen in quantum mechanics on the basis of 'insufficient cause'. It showed how matter gets its marching orders from both past and future boundary conditions, and is thereby enabled to 'know' exactly how to behave – even though its behaviour *looks* intrinsically probabilistic. It is a 'hidden variable' interpretation of quantum mechanics. A consequence of it seems to be that not only the laws but also the boundary conditions of the world are symmetric. We started out by looking at the microworld, and we seem to have ended up with a general conclusion applicable to the macroworld.

FINIS

Appendix

The main features of the formalism of quantum mechanics

In §1.2, we listed the main features of the standard interpretation of quantum mechanics:

(a) Description of the system in terms of a wave function.

(b) Use of linear hermitian operators to represent physical quantities.

(c) Use of eigenvalue/eigenfunction equations.

(d) Expansion postulate.

(e) Measurement postulate.

(f) Reduction postulate (also known as projection postulate).

(g) Use of macroscopic measuring apparatus.

(h) Heisenberg's indeterminacy principle.

(i) Spin.

(j) Pauli's exclusion principle.

(k) Essential complexity of the quantum-mechanical description of state.

Here are the details of each.[1]

(a) ***Description of the system in terms of a wave function***

For every physical system there exists a wave function determined by the physical situation, which contains all possible information about the system. (The wave function is symbolized by Ψ.[2]) Quantum mechanics gives rules for finding

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[1] I shall follow mostly Bjorken & Drell 1964, Bohm 1951, Davies 1984, Dirac 1935, Eisberg & Resnick 1974, Feynman, Leighton & Sands 1965, Goswami 1997, Longini 1970, Penrose 1989 & 1994, Shimony 1989, Unruh 1995 and Zimmerman 1966.

[2] The symbol denoting the quantum wave function in the coordinate realization, written in one dimension, i.e. for a particle moving in the x-direction only, is $\Psi(x,t)$. In

the wave function for different situations and extracting information from it. The wave function contains all the information about the observables associated with the system it represents. Whenever physicists work out a problem in quantum mechanics, e.g. calculating the probabilities of the future behaviour of a quantum system, it is to the wave function that they must turn. The wave function itself, though, is not an observable quantity, nor does it have a direct physical interpretation. Although it is sometimes described as a wave of probability, it is better described as a 'wave from which many related probabilities can be calculated'.[3]

Since the wave function contains all the information about the physical system, it may be said to describe the *state* of the system at any instant (which is why the wave function is also known as the 'state function').

Schrödinger's 1926 equation describes the time-development of the wave function, and thereby of the state of the associated physical system. It plays the same role in quantum mechanics as the equations of motion in classical mechanics. In three dimensions, for a single particle of mass *m*, it is written as follows:

$$-\frac{\hbar^2\nabla^2}{2m}\Psi(q,t)+V(q,t)\Psi(q,t) = i\hbar\frac{\partial\Psi(q,t)}{\partial t},$$

where $V(q,t)$ is the potential energy describing the forces acting on the particle, and $\nabla^2 = \partial^2/\partial x^2 + \partial^2/\partial y^2 + \partial^2/\partial z^2$.[4]

In one dimension, the equation reduces to

$$-\frac{\hbar^2}{2m}\frac{\partial^2\Psi(x,t)}{\partial x^2}+V(x,t)\Psi(x,t) = i\hbar\frac{\partial\Psi(x,t)}{\partial t}.$$

Throughout the Appendix, I shall usually, for reasons of convenience, give the one-dimensional form of equations.

If the system's initial state Ψ at time t is assumed to be known, Schrödinger's equation describes its subsequent time-evolution. The evolution is continuous and deterministic, and proceeds as if the wave function were a classical field described by some classical field equation such as that of Maxwell.

In the Schrödinger formalism, the wave function must be everywhere well behaved, meaning that it is continuous, has a single value at every point in space and at every time, and a continuous first derivative. Furthermore, it must conserve total probabilities (discussed in [e] below), such that for a single particle

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three dimensions it's written $\Psi(x,y,z,t)$ [or $\Psi(q,t)$ or $\Psi(\mathrm{r},t)$]. If spin is included, it may be written as $\Psi(q,s,t)$.

[3] Bohm 1951, p. 96.

[4] The symbol ∇^2 ('del squared') is known as the Laplacian operator. It is not only convenient to write but also indicates here that the equation changes its form for various coordinate systems just as vectors do.

described by the normalized wave function,[5] for example, the total probability of finding it somewhere is given by

$$\int_{-\infty}^{\infty} P dq = \int_{-\infty}^{\infty} \Psi^* \Psi dq = 1.$$

That is, the integral must have a finite value, meaning that the total amount of the observable associated with the wave function is conserved, regardless of its particular distribution.

The final thing that needs to be mentioned here in connection with the wave function Ψ is the *principle of linear superposition.* As we've seen, Schrödinger's equation presupposes that a microscopic system may be represented as a wave. Now a basic principle of classical wave theory, applying for example to electromagnetic waves, is that if Ψ_1 and Ψ_2 are possible wave functions representing states of the system, then any linear combination of them, $a\Psi_1 + b\Psi_2$, where a and b are arbitrary real-number weighting constants (also known as *amplitudes*), is also a possible wave function Ψ, and therefore the representation of a possible state of the system.[6] It is necessary to assume some such hypothesis to account for the interference of the waves, and the production of wave packets.[7] This basic principle of wave theory also applies to the wave functions of quantum theory. Given that all permissible wave functions must be solutions of Schrödinger's wave equation, the sum of the two solutions must also be a solution. This must be so since the wave equation is linear.[8] In other words, the new

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5 'Normalized' means that the probabilities obtained on the basis of the expansion coefficients must sum to one. The wave function is assumed to be normalized, because the total probability that the particle is somewhere in space must be unity. If Ψ is not already normalized, it can be normalized by multiplying it by a suitable constant such as A, such that $|A|^2 \int \Psi^* \Psi dq = 1$. (Bohm 1951, p. 177n.) Before the procedure of normalization is carried out, the amplitude of the wave function is arbitrary, because the linearity of the Schrödinger equation allows the wave function to be multiplied by a constant of arbitrary magnitude and still remain a solution to the equation. Normalization fixes the amplitude by fixing the value of the multiplicative constant. (Eisberg & Resnick 1974, p. 153.)

6 Though this is not true for electromagnetic waves of very high intensity passing through matter. In that case the principle of linear superposition is not precisely obeyed, and the resulting phenomena are described by non-linear optics. (Sears, Zemansky & Young 1987, p. 914.)

7 Bohm 1951, p. 174. Bohm says that it is not known whether this is the only hypothesis that will explain interference. It is, however, the simplest one that will do so, and it has been successful in explaining electromagnetic and acoustic interference phenomena.

8 A linear relation between two variables is one that can be represented graphically as a straight line. For example, a linear equation, such as Schrödinger's, is one in which the sum of any two of the solutions of the equation is also a solution of the equation. The quantum-mechanical superposition of wave functions is linear because it is postulated that the operator $\hat{O}_n$ does not depend on the function ψ_n itself. In a quantum-mechanical

state, too, is completely defined by the two original states, provided that the relative weights of a and b are known, and also the phase difference of the two systems. Since that is so, we must assume that between these states there exists a relationship such that whenever the system is definitely in one state, say Ψ, it can also be correctly regarded as being partly in each of two or more other states, say Ψ_1 and Ψ_2, as in the above illustration.[9]

(b) *Use of linear hermitian operators to represent physical quantities*

Every physical observable or classical dynamic quantity (e.g. potential energy V, total energy E, momentum p, coordinates q, spin s), is modelled by a linear hermitian operator that operates on the wave function. Operators are mathematical functions that operate on other functions.[10] They take as their input any quantum-mechanical wave function, and give as output either a different wave function, or possibly the same wave function times a real constant which is the measured value of the observable. An operator is linear if the result of the operation on the sum of two input wave functions is the same as the sum of the operations on each of the input wave functions. All change in observables such as p_x or x or E is determined by the change in the wave function. Like the wave functions on which they operate, operators themselves have no direct physical significance, being rather mathematical auxiliaries, used in calculating average values of physically observable quantities.[11]

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measurement (in contrast to the non-linearity of electromagnetic waves of very high intensity passing through matter – see note above), it is assumed that the measured system does not act back on the macroscopic measuring apparatus, which 'recoil' would alter the state of the measured system owing to its mutual coupling with the measuring apparatus. (Sachs 1988, pp. 126, 128, 247.)

9 It follows that no new state is formed by superposing a state with itself, but only the original state over again. Say the original state is represented by Ψ. When it is superposed with itself, the new state is

$$a_1\Psi + a_2\Psi = (a_1 + a_2)\Psi,$$

where a_1 and a_2 are numbers. Since $a_1 + a_2$ is an arbitrary number, it is quite generally true that when a wave function is multiplied by any number, not zero, the resulting wave function will represent the same state. Only if $a_1 + a_2 = 0$ would the state change. In that case, the two components would have cancelled each other through destructive interference and the resulting 'state' is no state at all. (After Dirac 1935, pp. 15-16.)

10 The operator concept in the sense of the transformation of a function to another function can be traced back to Leibniz, and Lagrange's generalization of Leibniz's approach. For a potted history of the concept, with particular reference to quantum mechanics, see Jammer 1966, pp. 224ff.

11 This is why Zimmerman, for one, complains that quantum theory doesn't provide a theory of microscopic nature at all. 'It starts to do so, with its talk of operators belonging to a microphysical system, and so on. But then when the theory begins to make contact with reality, one finds that the operators lead to representations not of one microsystem, but of an ensemble of similar microsystems – strictly, an infinite number of systems.' (Zimmerman 1966, p. 489.)

To each operator there corresponds an ensemble of numerical values (its 'spectrum'), which may be discrete or continuous. The numerical values that may be taken by the physical observable are the measurable characteristic values (eigenvalues) of the operator, enabling the quantization of quantum-mechanical systems to be modelled by the operator formalism. An operator is called 'hermitian' if its expectation value (defined below) is real. Similarly, the eigenvalues of hermitian operators are real, sharp, and physically realizable. Prigogine & Stengers identify the use of operators and the separation of the physical quantity (represented by its operator) from its numerical values (represented by eigenvalues of the operator) as *the* essential feature of quantum mechanics.[12]

For the momentum p_x, the corresponding momentum operator is $\hat{p}_x = \frac{\hbar}{i}\frac{\partial}{\partial x}$.[13] For the position x, the corresponding position operator is simply $\hat{x} = x$ (operating by x is the same as simply multiplying by x). Notice that the operators for both the momentum and position variables p_x and x are expressed in terms of the *position x alone*. This prevents coming into conflict with the Heisenberg indeterminacy principle (see [h] below), according to which there is a difficulty in expressing p_x as a function of x, since p_x and x cannot be simultaneously known with complete precision. Now, whenever we want to find the expectation value of p_x, we can do so with the wave function expressed as a function of the position x alone, by replacing the number p_x, by the differential operator $\frac{\hbar}{i}\frac{\partial}{\partial x}$.[14] For the total energy E, the corresponding operator is $\hat{E} = i\hbar\frac{\partial}{\partial t}$. Finally, there is the potential energy operator, which is simply $\hat{V} = V$.

The operators for all other measurements are found by writing the quantities to be measured classically in terms of energy, momentum, position and potential, and substituting the above four operators for the classical quantities.[15] There is no differential operator for the variable time, and in this sense the time t appearing in Schrödinger's equation, and elsewhere in the quantum-mechanical formalism is not a physical observable represented by a differential operator. It is rather a parameter external to the system of interest, e.g. a number measured

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12 Prigogine & Stengers 1984, p. 221.

13 Which may also be written as $\hat{p}_x = -i\hbar\frac{\partial}{\partial x}$.

14 That is because we are working in a position representation (position space), i.e. assuming that Ψ is a function of space and time. In momentum space, the position and momentum variables would be expressed in terms of the momentum alone, e.g. for the position x, the corresponding position operator is $\hat{x} = i\hbar\frac{\partial}{\partial p_x}$, and the momentum operator is simply $\hat{p}_x = p_x$. Even though they look different, both the position and momentum representations of a classical dynamical variable such as x are representations of the same thing.

15 Longini 1970, p. 13.

by a laboratory clock.[16] The absence of a differential time operator is connected with the fact that we cannot define the expectation value of a function of time in the way we can for a function of position.[17] There is, though, a 'time-reversal' operator $\hat{T}$, known as 'Wigner's time-inversion operator', such that $\hat{T}\psi(x,t) = \psi^*(x,-t)$. (This operator is discussed at greater length in §2.3.)

Consider the equation

$$\overline{f(x,p_x,t)} = \int_{-\infty}^{\infty} \Psi^*(x,t) f_{op}\left(x, -i\hbar\frac{\partial}{\partial x}, t\right)\Psi(x,t)dx,$$

where $\overline{f(x,p_x,t)}$ is the average of many measurements of the observable $f(x,p_x,t)$ made on identically prepared systems, known as the *expectation value*, and the operator $f_{op}(x,-i\hbar\,\partial/\partial x,t)$ is obtained from the function $f(x,p_x,t)$ by everywhere replacing p_x by $-i\hbar\,\partial/\partial x$. Doing so makes the equation integrable if we know $\Psi(x,t)$.

The wave function contains, through the above equation, all the information that Heisenberg's indeterminacy principle allows us to learn about the observables associated with the wave function – information such as the expectation value of the coordinate x, the potential energy V, the momentum p_x, the total energy E, and, in general, the expectation value of any dynamical quantity $f(x,p_x,t)$.[18]

Schrödinger's equation may also be written as

$$\hat{H}\Psi = \hat{E}\Psi,$$

where (in one dimension) $\hat{H} = \frac{\hat{p}_x{}^2}{2m} + \hat{V}$ is a linear hermitian operator representing the total energy of the system.[19] (It is customary to call the operator representing the total energy of a system 'the hamiltonian', designated by $\hat{H}$, after the classical hamiltonian function H – an expression for the total energy of a system in terms of all the position and momentum variables for all the physical objects

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16 It may, however, also be regarded as an operator with the particularly simple property that it multiplies the wave function by a number, just as in the position representation, $\hat{x}$ is represented as a number. A difference is that in the alternative momentum representation, the position x *is* a differential operator, whereas t remains a number in both representations (i.e. there is no differential operator $\hat{t} = i\hbar\frac{\partial}{\partial E}$).

17 See e.g. Goswami 1997, pp. 31-2, 36, 57.

18 After Eisberg & Resnick 1974, pp. 159-60. Note that the fact that an atom is found only in discrete energy levels entails that energy cannot be just a function of position and momentum, otherwise it could be made to vary continuously by giving the position and momentum slightly different values. (Prigogine & Stengers 1984, p. 220.)

19 In three dimensions, it is written $\hat{H} = \frac{1}{2m}\left(\hat{p}_x{}^2 + \hat{p}_y{}^2 + \hat{p}_z{}^2\right) + \hat{V}$.

belonging to the system.[20] $\hat{H}$ is also known as the 'time-displacement operator'.)

The above way of writing Schrödinger's equation follows in a natural way from the operator formalism postulated above. Take the classical equation relating the total energy E to the momentum p_x and to the potential energy $V(x,t)$:

$$\frac{p_x^{\,2}}{2m} + V(x,t) = E,$$

where m is the particle's mass, and $V(x,t)$ describes the forces acting on the moving particle, i.e. the *electrical* potential energy. ($p_x^2/2m$ is simply another way of writing the non-relativistic expression for the kinetic energy of the particle $1/2\ mv_x^2$.) The left-hand side of the equation is the classical hamiltonian.

We want to tie in quantum mechanics to classical mechanics. We proceed as follows:

Replace the dynamical quantities p_x and E by their respective differential operators $\hat{p}_x = -i\hbar\frac{\partial}{\partial x}$ and $\hat{E} = i\hbar\frac{\partial}{\partial t}$. That gives

$$\frac{1}{2m}\left(-i\hbar\frac{\partial}{\partial x}\right)^2 + V(x,t) = i\hbar\frac{\partial}{\partial t}.$$

Since $(-i\hbar)^2 = -\hbar^2$, and $(\partial/\partial)^2 = (\partial/\partial x)(\partial/\partial x) = \partial^2/\partial x^2$, we get

$$-\frac{\hbar^2}{2m}\frac{\partial^2}{\partial x^2} + V(x,t) = i\hbar\frac{\partial}{\partial t}.$$

Now apply this to any wave function $\Psi(x,t)$. We get

$$-\frac{\hbar^2}{2m}\frac{\partial^2\Psi(x,t)}{\partial x^2} + V(x,t)\Psi(x,t) = i\hbar\frac{\partial\Psi(x,t)}{\partial t}.$$

But this is just Schrödinger's equation, which describes the time-development of the wave function, and thereby of the physical system. So it seems that postulating the operator relations given above is equivalent to postulating Schrödinger's equation. In fact, that is essentially the procedure originally followed by Schrödinger in obtaining his equation.[21]

$\hat{H}$ is the most important operator in quantum mechanics. That is because it

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20 The classical hamiltonian represents a principle of 'stationary action', a variant of the principle of 'least action'. For some discussion of the similarities and differences between the classical hamiltonian function and Schrödinger's equation, see Penrose 1989, pp. 288, 174ff.

21 Eisberg & Resnick 1974, p. 158.

contains the potential energy V. Longini, for example, writes:

> The potential is the *only* connection between the quantum mechanical formulation and the 'real world'. The way a ball rolls downhill, the reason a pendulum swings, or the difficulty of leaving the earth can all be stated in terms of the potential energy being a function of position. In like manner, electrons behave in ways determined by the spatial (meaning function of position in space) dependence of the potential energy.[22]

This is why the problem of obtaining the eigenfunctions of $\hat{H}$ ('finding the hamiltonian' for a system) is a basic problem of quantum theory. Once we have obtained all of them, we have obtained a general solution of the wave equation as a function of time.[23]

(c) *Use of eigenvalue/eigenfunction equations*

If an operator acting on a function gives back the same function, multiplied by some constant (number), the function is said to be an *eigenfunction* of the operator, and the constant its *eigenvalue*. The eigenvalue-eigenfunction equation for a hermitian operator $\hat{O}$ is

$$\hat{O}\psi_n = o_n \psi_n,$$

where ψ_n is the operator's eigenfunction belonging to the eigenvalue o_n.[24] For a hermitian operator, o_n is always real.

Where a function is an eigenfunction of the operator, a measurement of the observable represented by $\hat{O}$ is certain to lead to the result o_n. Take $\hat{p}_x \psi_a = a\psi_a$, where a is an eigenvalue of the momentum operator $\hat{p}_x$, and ψ_a is an eigenfunction belonging to the eigenvalue a. A measurement of p_x is certain to give a. (In this case, $\hat{p}_x \psi_a = a\psi_a$, where a is the actual measurable value of the system in the state ψ_a.[25] Clearly, it must be a real number.[26]) Every possible result of the

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22 Longini 1970, p. 15.

23 Bohm 1951, p. 225.

24 The complication that there may be more than one eigenvalue will be dealt with later.

25 To be an eigenfunction of momentum, the wave function ψ must be of the form $\psi = ae^{ip_x x/\hbar}$. The eigenfunctions of x in momentum space are plane waves, just like the eigenfunctions of p_x in coordinate space. (Bohm 1951, p. 214.)

26 Or take $\hat{x}\psi = b\psi_b$, where b is an eigenvalue of the position operator $\hat{x}$. Since $\hat{x} = x$, we have simply $x\psi = b\psi$, where b is the exact position of the particle such as an electron along the x axis. A measurement of the position is certain to give b. But it can do that only if ψ is zero for $x \neq b$. Thus, the magnitude of ψ is related to position. The special eigenfunctions for the position operator are called 'delta functions' or 'Dirac delta functions'. Where ψ is not a delta function, we cannot state an exact position for the particle. Longini, whom I've followed here, points out (1970, p. 12) that there is no ordinary function which satisfies the equation $x\psi = a\psi$ for *one* value of the constant a

measurement of an observable, with the system in any state whatever, is one of the eigenvalues of the observable. The converse is also true: every eigenvalue of an observable is a possible result of the measurement of that observable.[27] The set of eigenvalues of an observable are just the possible results of measurements of that observable.[28]

Even though measurements always yield eigenvalues, in the general case, when a system is in a given quantum state Ψ, and provided that Ψ is not an eigenfunction of the operator $\hat{O}$, the observed value of any observable O cannot be predicted. Instead, we can speak of it having an average value for the state, and also a probability for having any specified value for the state, meaning the probability of obtaining such specified value upon measurement of the observable.[29]

Consider again Schrödinger's equation. There are many situations in which the potential energy of a particle does not depend on the time explicitly, the forces that act on it (and so the potential) varying with the particle's *position* only. In such cases the time-dependent equation may be simplified by removing all reference to t by using a standard mathematical technique called 'separation of variables'. The technique consists in searching for a solution in which the wave function can be written as the product of a position-dependent function $\psi(x)$ and a time-dependent function $\varphi(t)$:

$$\Psi(x,t) = \psi(x)\varphi(t),$$

where ψ and φ are functions, respectively, of x and t alone. Solutions of this form exist provided that the potential energy does not explicitly depend on the time, so that the function for the potential can be written $V(x)$. In that case, the wave function is an eigenfunction of $\hat{H}$.

In such a case, the function $\varphi(t)$ is the function that specifies the time rate of change of the wave function $\Psi(x,t)$, once the initial value is known. It is an oscillatory function of frequency $\nu = E/h$, given by the expression

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and *every* value of the variable x. Hence the use of Dirac's delta functions.

27 It is worth pointing out that it is a fiction, albeit a convenient one for expository purposes, that to every Hermitian operator there corresponds an experiment to observe the corresponding quantity. Hartle, for example, writes (1968, p. 705n): 'In fact, experimental arrangements are known for only a few of these quantities. This remarkable circumstance seems to arise from the fact that we are able to conceive of experimental arrangements only in classical terms. The description of a quantum mechanical state, however, requires many more numbers than a corresponding classical state (for example, the state of a spin-2 object in classical physics is described by two numbers while in quantum mechanics it is described by nine), and there is a corresponding greater number of "measurable quantities".'

28 Dirac 1935, p. 32.

29 After Dirac 1935, pp. 30, 44.

$$\varphi(t) = e^{-iEt/\hbar},$$

where E is the total energy of the particle in the system. That is, the eigenfunctions oscillate harmonically with the frequency $2\pi\nu = E/\hbar$, or $\nu = E/h$.[30]

As for the function $\psi(x)$, which specifies the space dependence of the wave function $\Psi(x,t) = \psi(x)\varphi(t)$, it is a solution to the differential equation

$$\left[-\frac{\hbar^2}{2m}\frac{d^2\psi(x)}{dx^2} + V(x)\right]\psi(x) = E\psi(x),$$

called the *time-independent Schrödinger equation*.[31] The equation is time independent because the time variable t does not enter into it. Accordingly, it is also called the *stationary state* (or *steady state*) form of the Schrödinger equation. Since stationary states have wave functions which are eigenfunctions of the energy operator, they always have solutions of the form $\Psi = \psi e^{-iEt/\hbar}$, where ψ is position dependent only, and all the time dependence is cyclic and in the exponential.[32]

In addition to the time-independence/dependence, three other important distinctions between the time-independent and the time-dependent equations are: (a) the stationary state form does not contain the imaginary number i, and so its solutions $\psi_n(x)$ need not be complex; (b) it contains explicitly the total energy E; and (c) since the system doesn't gain or lose energy and the energy is well-defined, all probabilities remain constant over time. In a series of repeated measurements of a dynamical variable in such a state, the individual values obtained will fluctuate from one experiment to the next, but the probability of obtaining a given value will be independent of the time that has elapsed since the state was prepared. This is in contrast to a wave function which is not an eigenfunction of the energy and which moves through space and spreads out so that the probabilities change with time.[33]

From the time equation $\varphi(t) = e^{-iEt/\hbar}$, the product form of the wave function may be written as

$$\Psi(x,t) = \psi(x)e^{-iEt/\hbar},$$

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30 See Bohm 1951, pp. 227-8 for relevant detail.

31 Eisberg & Resnick 1974, p. 164. In one dimension it is not a partial differential equation because it involves only one independent variable, x, unlike the time-dependent Schrödinger equation which involves two independent variables, x and t. Note that since the quantity within the square brackets is the total energy operator $\hat{H}$, the above equation may also be written as $\hat{H}\psi(x) = E\psi(x)$.

32 Longini 1970, p. 15.

33 Point (c) is after Bohm 1951, pp. 225-6.

where E is the total energy of the particle.[34]

The set of the eigenfunctions $\psi_n(x)$ is the set of admissible solutions to the time-independent Schrödinger equation for the potential $V(x)$. The set constitutes the set of the admissible states of the system. The corresponding set of eigenvalues o_n constitutes the set of the predicted allowed values for the observable aspect of the system represented by $\hat{O}$.

Thus, the eigenfunctions $\psi_n(x)$ exist only for certain values of the energy, E_1, E_2, E_3, ..., E_n, where the energies are the eigenvalues belonging to $\hat{H}$.[35] Corresponding to each eigenvalue there is an eigenfunction (sometimes more than one), $\psi_1(x)$, $\psi_2(x)$, $\psi_3(x)$, ..., $\psi_n(x)$, which satisfies the eigenvalue-eigenfunction equation $\hat{H}\psi_n = E_n\psi_n$. Each eigenfunction is a solution to the time-independent Schrödinger equation for the potential $V(x)$, i.e. an eigenfunction of $\hat{H}$. For each eigenvalue there is a corresponding wave function $\Psi_1(x,t)$, $\Psi_2(x,t)$, $\Psi_3(x,t)$, ..., $\Psi_n(x,t)$, each of which is a particular solution to the Schrödinger equation for the potential $V(x)$. From the equation $\Psi(x,t) = \psi(x)e^{-iEt/\hbar}$, we know that these wave functions are

$$\Psi_1(x,t) = \psi_1(x)e^{-iE_1t/\hbar}, \Psi_2(x,t) = \psi_2(x)e^{-iE_2t/\hbar}, \Psi_3(x,t) = \psi_3(x)e^{-iE_3t/\hbar}, ...,$$
$$\Psi_n(x,t) = \psi_n(x)e^{-iE_nt/\hbar}.$$

If the system is described by the wave function $\Psi_n(x,t)$, it is said to be in the quantum state n. The index n is called the *quantum number*. It takes on successive integral values, and designates a particular eigenvalue and its corresponding eigenfunction and wave function.[36]

In general, then, Schrödinger's time-independent equation can be solved only for certain values of the energy E.[37]

The importance of the time-independent Schrödinger equation is that it promises to give all the solutions of physical interest in the non-relativistic quantum domain.[38] For example, all solutions of the time-dependent equation can be obtained by superposing stationary-state solutions possessing different frequen-

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34 After Eisberg & Resnick 1974, pp. 164-70.

35 The eigenvalues occurring early in the list may be discretely separated in energy, but generally become continuously distributed beyond a certain energy. The total energy for a free electron, with $E > 0$ is not quantized at all, but may take any value. (Eisberg & Resnick 1974, p. 120.)

36 After Eisberg & Resnick 1974, pp. 179-80.

37 To solve it for a given system means to find an eigenfunction which not only obeys Schrödinger's stationary state equation and the relevant boundary conditions, but also fulfils the general requirements for an acceptable wave function – in particular that its derivatives be continuous, finite and single-valued. If there is no such solution, then the system cannot exist in a stationary state.

38 Eisberg & Resnick 1974, p. 167.

cies/energies (see [d] below). That results in destructive and constructive interference of the wave functions belonging to the different energies, such interference changing in position with time.

It is this *changing position of interference* that governs the motion of the wave packet, and thereby governs the changes of probability with time. Motion in quantum mechanics is therefore described in an essentially nonclassical way.[39] Moreover, changing probabilities (change in motion) can occur only when there is a range of energies present, i.e. when the energy is somewhat indefinite. This feature of the theory ensures that Heisenberg's indeterminacy relation between energy and time is automatically contained within the theory.[40] Here are these two important points in Bohm's own words:

> It is very significant that the way quantum theory describes changes of probability with time is through the terms involving the interference of the contributions of different stationary states. Motion is, therefore, described in an essentially nonclassical way. The change of any particular probability distribution is produced simply by the changing phase relations between different components of the wave function corresponding to different stationary states. Here we see a simple case of how the phase difference between two stationary states has physical significance; namely, it controls the change of probability with time. Because the process of motion is described in terms of the interference of wave functions belonging to different energies, we conclude that changing probabilities will exist only when there is a range of energies present or, in other words, when the energy is somewhat indefinite. In this way, the uncertainty principle between energy and time is automatically contained in the theory.[41]

The time-independent Schrödinger equation and its solutions are the quantum-mechanical equivalents of the time-independent differential equation and its solutions for classical wave motion. In particular, the energy quantization in solutions of Schrödinger's equation is analogous to that of standing waves in a stretched string of length L, fastened at both ends. In the latter case, waves are propagating in both the $+x$ and $-x$ directions simultaneously. As Arthur Beiser[42] notes, 'An acceptable function $y(x,t)$ for the displacement must, with its derivatives, obey the same requirements of continuity, finiteness, and single-valuedness as Ψ and, in addition, must be real since y represents a directly measurable quantity. The only solutions of the [classical] wave equation

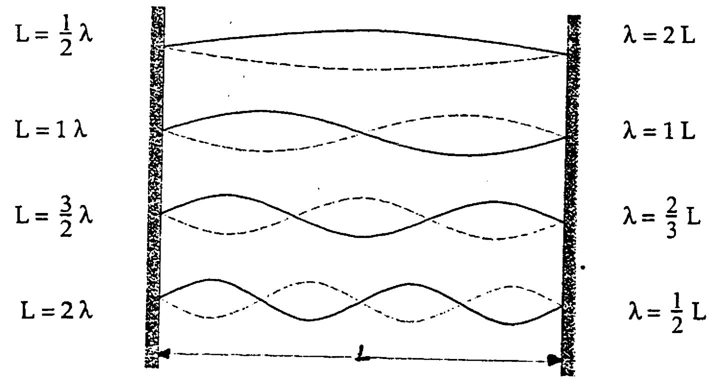
39 This point is after Bohm 1951, p. 228.
40 Bohm 1951, p. 228.
41 Bohm 1951, p. 228.
42 The author of *The Proper Yacht* (Adlard Coles, 1966), who also moonlighted as a professor of physics at New York University.

$$\frac{\partial^2 y}{\partial x^2} = \frac{1}{\upsilon^2} \frac{\partial^2 y}{\partial t^2}$$

that are in accord with these various limitations are those in which the wavelengths are given by

$$\lambda_n = \frac{2L}{n+1} \qquad n = 0,1,2,3,...,$$

where L is the straight-line length between the ends of the string. It is the *combination* of the wave equation and the restrictions placed on the nature of its solution that leads us to conclude that $y(x,t)$ can exist only for certain wavelengths λ_n.'[43]



STANDING WAVES IN A STRETCHED STRING FASTENED AT BOTH ENDS

We see that the quantum numbers indicated by the index n in $\Psi_n(x,t)$ appear to be related in a natural way to the number of nodes in a vibrating system. Consistent with this analogy is the fact that the solution of Schrödinger's equation is obtained by breaking up the wave function into an infinite series of eigenfunctions which are all natural harmonics of one another – meaning that their frequencies and wavelengths are related in the ratio of whole numbers – each of which is a particular solution of the equation.

(d) *Expansion postulate*

An arbitrary wave function Ψ for a physical system can be expanded in terms of a complete set of linearly independent, orthonormal[44] eigenfunctions ψ_n of the

[43] Beiser 1969, pp. 165-7.

[44] 'Orthonormal' here means that the eigenfunctions are both orthogonal and have been normalized. We've seen that 'normalized' means that the probabilities obtained on the basis of the expansion coefficients must sum to one. As for 'orthogonal', it means mutually at right-angles in the mathematical state space of quantum mechanics. Note that 'orthogonality' in quantum mechanics need not correspond to 'at right angles' in ordinary space. For example, an electron has two possible spin states along any selected axis in ordinary space – 'up' (↑) or 'down' (↓) Even though these states are *oppositely* directed spin angular momentum vectors in ordinary space, they are *orthogonal* to each other in the abstract state space of quantum mechanics.

Schrödinger equation:

$$\Psi = \sum_n a_n \psi_n,$$

i.e.

$$\Psi = a_1 \psi_1 + a_2 \psi_2 + \ \dots \ a_n \psi_n + \ \dots$$

where both the coefficients a_n and the values of the functions ψ_n are generally complex numbers. This is known as the *expansion postulate.* It means that the general state of the system can be expressed as a *coherent linear superposition* of states, with complex-number expansion coefficients (amplitudes or 'weighting' factors) of all the possible measurable alternatives available to the system.[45] The coefficients a_n determine the probability of the system being in one of the eigenstates, namely the eigenstate ψ_n. Assuming that the ψ_n are normalized, the expansion imposes a restriction on the values of the coefficients, namely $\sum_n a_n^* a_n = 1$.

The above conception of the general state of a system has some noteworthy features, in particular its linear superposition of states, complexity, self-interference, and non-separability. Here are some details of each.

Linear superposition of states. Whenever a system is in one particular state, it is also partly in each of two or more other states. The original state is the result of a superposition of two or more other states, in an infinite number of ways for dynamical systems.[46] This is so also in classical wave theory, as we have seen in (a) above. In classical wave theory, if Ψ_1 and Ψ_2 are possible wave functions

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More generally, in quantum mechanics 'orthogonality' refers to states that are *independent* of one another. The different position states that a particle may have are all independent of one another (they are orthogonal to one another), as are all the momentum states that a particle may have (they, too, are orthogonal to one another). But the position states are not independent of the momentum states because of quantum complementarity, and so position states are not orthogonal to momentum states. Similarly, the different possible spin states of a particle along any single selected space axis, e.g. the x axis, are orthogonal, but the spin states along two or more different space axes, e.g. the x and the y axis, are not orthogonal – they are not independent of each other because of quantum complementarity. The property of orthogonality between superposed states is preserved by the Schrödinger evolution of the wave function. (For a discussion, see Penrose 1989, pp. 257-68, 1994, pp. 281-2.)

Mathematically, 'orthogonality' is defined as the vanishing of the scalar product (the 'dot' or 'inner' product) between two vectors or states drawn from a common point: $\mathbf{u} \cdot \mathbf{v} = |\mathbf{u}|\,|\mathbf{v}| \cos \theta = 0$, where θ is the angle between them.

45 By *coherent* superposition, the following is meant: Take two solutions $\Psi_1(x,t)$ and $\Psi_2(x,t)$ of Schrödinger's equation. According to the principle of linear superposition, the linear combination $\Psi(x,t) = \alpha\Psi_1(x,t) + \beta\Psi_2(x,t)$, where α and β are arbitrary complex numbers, is also a solution of Schrödinger's equation. It is evident that the absolute square of the superposition depends crucially on the relative phase of Ψ_1 and Ψ_2. Such a superposition is called 'coherent'. (Goswami 1997, pp. 21-2.)

46 Dirac 1935, p. 12.

representing states of the system, then any linear combination of them, $a\Psi_1 + b\Psi_2$, where a and b are arbitrary real-number weighting constants [also known as *amplitudes*], is also a possible wave function Ψ, and therefore a representation of the state of the system.

But there are significant differences between the quantum and classical wave-theoretical conceptions of 'state' pursuant to the principle of linear superposition. Take a quantum state formed by the superposition of two states A and B, which are eigenfunctions belonging, respectively, to the eigenvalues a and b.[47] When a measurement is made on the system in the superposed state, the result of the measurement will sometimes be a and sometimes b, the relative frequency of each result depending on the weighting constants of each possibility and given by the quantum probability law (see [e] below). In contrast to superposed classical waves, the result of any one measurement will never be *intermediate* between a and b. Only the *probability* of a particular result for an observation is intermediate between the corresponding probabilities for the original states.[48]

Here is a specific example. Take a beam of plane-polarized light passing through a crystal of tourmaline. The beam is polarized at 45°, to the optic axis of the crystal. For ease of visualization, imagine the optic axis to be pointing straight up with respect to the floor of our laboratory. The crystal acts as a polarizing lens. It allows to pass only waves that vibrate in the plane of the crystal (sometimes called its plane of polarization), this plane being perpendicular to the optic axis of a polarizer such as tourmaline, i.e. *horizontal* in the present example. Now, if the incident beam were polarized either perpendicular or parallel to the optic axis, classical theory says that all the incident light would be passed by the crystal in the former case and blocked in the latter. When the beam is polarized at 45°, the theory predicts that only the horizontal component, $\varepsilon_o \sin \theta$, of the electric field would get through. The energy of the light wave on the other side of the crystal would be attenuated accordingly.

We now go to the quantum-mechanical analysis. According to quantum mechanics, the incident beam is made up of photons, each pictured, roughly speaking, as plane-polarized in the same direction as the beam. Now, if the beam of photons were polarized either perpendicular or parallel to the optic axis, the result in term of observables would be classical. In the former case all the photons would be passed by the crystal, and in the latter case blocked by it. Even in the more interesting case when the incident beam is at 45°, the result is classical on the level of observables and in terms of probabilities, since around half of the total number of photons are passed with the remainder blocked. That is, just half of the total incident energy is passed by the crystal. But how are we to understand this on an individual photon basis? What happens to each photon when it

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47 In this case the states A and B are orthogonal states.

48 After Dirac 1935, p. 13.

gets to the crystal?

To sharpen up the analysis, we go to an experiment in which the incident beam is a single photon, polarized diagonally to the optic axis as above. According to quantum mechanics, that state of diagonal polarization is also correctly described as a linear superposition of states of vertical and horizontal polarization (0° and 90°), with some amplitude of each. Its state is written as a linear superposition of those two states as follows:

$$\Psi = \frac{1}{\sqrt{2}}\left(\psi_{\updownarrow} + \psi_{\leftrightarrow}\right).$$

We would find, on the basis of many measurements of identically prepared systems (i.e. described by the same wave function), that the photon either gets through, or is blocked, with an even chance of each. If it gets through, it emerges polarized not diagonally but horizontally, its axis of polarization rotated by 45°. (We take it that the photon is horizontally polarized because additional polarizers placed in its path at the same angle as the one through which it just passed do not block it.) However, this state, too, is correctly described as a linear superposition, namely of states of diagonal (45°) and slant (135°) polarizations, with equal amplitudes of each. At other angles to the optic axis, the probabilities work out differently. If we were to repeat the experiment many times at various angles θ, we would find the general rule that the photon has a probability $\sin^2\theta$ of getting through, and a probability of $\cos^2\theta$ of being absorbed or deflected. E.g., at 90° to the optic axis, a photon gets through every time, and at 0° it never does.

Compare this with the prediction of the classical theory, according to which only some *fraction of the energy* that is sent comes through the polarizer. But in quantum mechanics there is no such thing as a fraction of a photon. Instead, quantum theory says that *all* the energy is there *some fraction of the time.*[49]

It is evident that the concept of *state* in quantum theory differs from the concept of state in classical physics. If two or more physical systems are put into identical states in classical physics, their subsequent behaviour will be identical (*mutatis mutandis*). But according to quantum theory, if two or more quantum systems are put into identical states, i.e. states described by the same wave function, as illustrated in the single-photon case above, their subsequent behaviour will generally differ, even though the wave function is said to contain complete information about the physical system. The above example illustrates just what that means in physical terms. In quantum theory, all that can be said is that each of the systems has the same *probability* for the development of its various 'potentialities' – but the probabilities do not of course translate into certainties.

The intermediate character of the new state shows, writes Dirac, that

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[49] Feynman, Leighton & Sands 1965, III-11, pp. 10-11.

'[t]here is an entirely new idea involved, to which one must get accustomed and in terms of which one must proceed to build up an exact mathematical theory, without having any detailed classical picture'.[50]

Complexity. The assumption that the expansion factors can be complex numbers is necessary for the mathematical formulation of the quantum-mechanical principle of superposition. It is necessary in the sense that the account must not only be internally consistent but also accord with the known results of experiment.[51] For example, the self-interference and non-separability properties of quantum systems (described below) are made possible by the essential complexity of the quantum conception of 'state'. We shall go on to look at the complexity of quantum mechanics in more detail in (k) below.

Self-interference. The superposition of complex states permits the self-interference of particles, suggesting a peculiar kind of nonlocality. In the early days of wave mechanics, people realized that the connection between light and photons must be of a statistical character. It was thought that the wave function describes an ensemble of many photons. In that case, the intensity (amplitude squared) of the wave function at any place ought to be proportional to the number of photons at that place (e.g. passing through a given area per unit time). They didn't realize that the wave function gives information about the probability of *one* photon being in a particular place, not about the probable *number* of photons in that place.[52] It turned out that the quantum-mechanical wave function differed in an important way from a classical wave function, in that it appeared to be something *more* than just a mathematical representation of a classically describable ensemble of objects.[53]

To appreciate the importance of the above distinction, consider a beam of light consisting of a large number of photons, split into two component beams of equal intensity by a beam-splitter consisting of a half-silvered mirror, with the two beams brought back together enabling them to interfere.[54] If the wave function represented nothing more than a classically describable ensemble of objects, we would expect that half of the photons go into one component and half into the other. But this is not what happens. Instead, each photon (according to the

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50 Dirac 1935, p. 12.

51 See Dirac 1935, pp. 16-17. Also Feynman, Leighton & Sands 1965, III-1, p. 6.

52 After Dirac 1935, p. 9.

53 See e.g. Penrose 1989, pp. 238-9, 289. Penrose specifically states that for individual photons, the photon states are complex (1989, p. 289n).

54 Strangely enough, the best half-silvered mirror is just a thin piece of transparent material of exactly the right thickness in relation to the wavelength of the light. See Penrose 1994, pp. 305-6, n. 7. For a discussion of the quantum-mechanical theory of the transmission and reflection of light by glass, see Feynman's fascinating *QED: The Strange Theory of Light and Matter,* Chapter 1.

standard interpretation) goes partly into each of the two components. This explanation is indicated by the results of making the two component beams come together again and interfere. On the assumption that the intensity of a beam is connected with the probable number of photons in it, a photon in one component ought to be able to interfere, either destructively or constructively, with a photon in the other component. If destructively, these two photons would mutually annihilate; if constructively, they would produce four photons. Such interference would violate the conservation of energy and must be ruled out. However, the conservation of energy is not violated in a theory which connects the wave function with the probabilities for only *one* photon, by making each photon go partly into each of the two components. When the two beams are brought together, *each photon then interferes only with itself, never with another photon.*[55] Moreover, the intensity of the light can be made so low that only a single photon is emitted, which photon then proceeds to interfere with itself – with physically observable consequences, as we shall see in (e) below.

Similar results are obtained in experiments utilising neutrons. For a description and analysis of recent neutron self-interference experiments, showing the nonlocality of a neutron leaving the interferometer, see Aerts & Reignier 1991; see also some remarks in Leggett 1987b, pp. 86-7.

The above feature is totally unexpected on a classical conception of 'state', as it seems to indicate a breakdown, in a certain sense, of both locality and genidentity.

Non-separability. The apparent breakdown of locality and genidentity is particularly evident in the quantum conception of state for *multi*-particle systems, especially as evidenced in the singlet-state case. Take a system of particles of half-integer spin (for details of 'spin', see [i] below). There are two directions associated with the spin of such particles: whenever a spin-half particle's spin is observed, it is always found to be either 'up' (↑) or 'down' (↓) along a reference axis, i.e. parallel or antiparallel to an applied magnetic field along that axis, with a component of spin angular momentum of $\pm\frac{1}{2}\hbar$ along that axis. ($\frac{1}{2}\hbar$ is the fundamental unit of spin angular momentum. The spin can be either $\frac{1}{2}\hbar$ or $-\frac{1}{2}\hbar$, the difference between the two being $\hbar$ [see (i) below].) The spin angular momentum values $\frac{1}{2}\hbar$ and $-\frac{1}{2}\hbar$ are the eigenvalues of the spin component along the selected axis. Since there exist no other possible eigenvalues of the spin, there are only two possible spin-states for a single fermionic particle: (a) ↑, + (b) ↓. Any possible spin state of a single particle can be represented as a linear superposition of just these two orthonormal *base* states. Since so, we say that the *basis* for the spin states of a single particle consists of these two (base) states. For two particles that are independent of each other (and assuming for the sake of sim-

55 After Dirac 1935, p. 9.

plicity that they are distinguishable from each other), there are four possible spin-states: (a) ↑↑, + (b) ↑↓, + (c) ↓↑, + (d) ↓↓. Again, any possible state of two-particle spin can be represented as a linear superposition of just these four states (the basis for the spin states of a two-particle system consists of four states). With three independent particles, the number of possible states is increased from four to eight: (a) ↑↑↑, + (b) ↑↑↓, + (c) ↑↓↑, + (d) ↑↓↓, + (e) ↓↑↑, + (f) ↓↑↓, + (g) ↓↓↑, + (h) ↓↓↓. With four independent particles the number of states is increased from eight to sixteen, and so on. Each time another (independent and distinguishable) particle is added to the picture, the total number of states which we must take into account when constructing an arbitrary wave function representing the system increases by a factor of two, this factor deriving from the number of possible orthonormal or base spin-states for a single spin-half particle, namely two. If there were, say ten such possible states, then the factor would be ten, in which case a three-particle system would have a thousand base states. In general, the total number of base states for any particular property of interest is a function of the number of particles comprising the system and the number of the base states of each particle for that property.[56]

When there is more than one particle in a system and the particles are *indistinguishable* from one another, i.e. when there is appreciable overlapping of the wave functions of indistinguishable particles in a system, the rules are somewhat different, and important non-classical effects arise from their indistinguishability. Moreover, the rules are different in a different way for bosons and fermions. In quantum statistics, particles that are represented by symmetric eigenfunctions are called *bosons* and particles represented by antisymmetric eigenfunctions are called *fermions*. The eigenfunction for a system of several identical fermions changes sign if the labels of any two of them are interchanged, whereas the eigenfunction for a system of several identical bosons does not change upon such label interchange (see (i) & (j) below for details).[57] An important implication of the rule as regards the quantum-mechanical concept of 'state' is that no two fermions can be in the same state.

To take a more specific example of the above quantum superposition for a multi-particle system, and the ensuing nonlocality, consider a system consisting of two spinning independent, distinguishable particles, each of spin $(\pm)\frac{1}{2}\hbar$, the (+) and (–) signs indicating that the spin angular momentum S_i along the selected axis for the measurement of the spin is either 'up' or 'down'. The particles could be for example an electron and a positron. This system has four basic wave functions, from which an arbitrary wave function representing the state of the system can be constructed. They are:

56 After Penrose 1989, Ch. 6.

57 Eisberg & Resnick 1974, p. 411.

(a) $|\psi_a\rangle = |\alpha\uparrow\rangle|\beta\uparrow\rangle$; (b) $|\psi_b\rangle = |\alpha\downarrow\rangle|\beta\downarrow\rangle$; (c) $|\psi_c\rangle = |\alpha\uparrow\rangle|\beta\downarrow\rangle$;
(d) $|\psi_d\rangle = |\alpha\downarrow\rangle|\beta\uparrow\rangle$,

where α and β, respectively, are the wave functions representing the spins of the two particles A and B.[58]

Suppose that our two-particle system of interest is in a so-called *singlet state*, meaning that their total spin angular momentum (a knowable quantity) is zero. An example is an unstable positronium 'atom', constituted of an electron and positron orbiting each other. The spins of such a particle-pair, even when the atom decays and the particles go their separate ways) are described by a single wave function that is a superposition (a linear combination)[59] of the basic wave functions (c) and (d) above, each possessing two eigenstates of zero total angular momentum. We may write it as follows:

$$|\psi_0\rangle = \frac{1}{\sqrt{2}}(\psi_c - \psi_d).$$

Writing out (c) and (d) (i.e. ψ_c and ψ_d) in full, we have:

$$|\psi_0\rangle = \frac{1}{\sqrt{2}}\left(|\alpha\uparrow\rangle|\beta\downarrow\rangle - |\alpha\downarrow\rangle|\beta\uparrow\rangle\right),$$

where the '–' sign refers to an *alternative*: the two wave functions ψ_c and ψ_d are mutually exclusive, in the sense that only one of the two conjunctions will be found upon measurement. Suppose that the measurement axis is the z axis. Particle A can be either z-spin up or z-spin down, and particle B must be either z-spin down or z-spin up. The factor $1/\sqrt{2}$ indicates that in the present example either conjunction is equally likely.[60] The key point about the above equation is that it depicts a state that is *irreducibly* a state of the two particles considered as a *composite* system. As Lockwood notes, it can't be equated with *any* combination of spin states of the two particles, considered individually. (These would always be of the form $|\psi_1\alpha\rangle|\psi_2\beta\rangle$, say $|\psi_1\uparrow\rangle|\psi_2\downarrow\rangle$, as opposed to the required

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58 Following Dirac, the quantum wave function is often denoted by the symbol $|\ \rangle$, called a 'ket'. The other half of the symbol, which we shall encounter later (e.g. in [e] below) is $\langle\ |$, called a 'bra' – hence the symbol $\langle\ |\ \rangle$ ('bra-ket', read as 'bracket'), representing a scalar product. A ket is the initial state and bra the final state of a system. The notation $|\ \rangle|\ \rangle$ refers to the tensor (or cross or outer) product of the two wave functions. It represents the quantum-mechanical 'and', meaning that there are two (independent) particles, one at A *and* one at B.

59 Recall that a linear equation is one in which the sum of two of its solutions is also a solution of the equation.

60 A '+' sign for the combination of ψ_c with ψ_d would indicate that their total spin angular momentum is $\hbar$ rather than zero (but with a zero z-value of the angular momentum). (Bohm 1951, p. 616.)

form as in $|\psi_0\rangle = 1\sqrt{2}\big(|\psi_1 \uparrow\rangle|\psi_2 \downarrow\rangle - |\psi_1 \downarrow\rangle|\psi_2 \uparrow\rangle\big)$.

Bohm points out that the selection of sign is crucial in determining the combined spin, which shows that the total angular momentum of the system is an *interference property* of both ψ_c and ψ_d.[61] On the other hand, the only states in which each particle has a definite spin opposite to that of the other are represented by ψ_c or by ψ_d separately.

> [I]n any state in which the value of σ_z for each particle is definite, the total angular momentum must be indefinite. Vice versa, whenever the total angular momentum is definite, then neither atom can correctly be regarded as having a definite value of its own spin, for if it did, there could be no interference between ψ_c and ψ_d, and it is just this interference which is required to produce a definite total angular momentum.[62]

Besides leading to a definite value of the combined spin, the definite phase relations between ψ_c and ψ_d have additional physical significance. They imply that the spins of the particles will be found correlated if the same component of the spin of each particle is measured even when the particles may be light-years apart. This is simply because the pair are described by a single wave function that has *definite phase relations* between ψ_c and ψ_d. In this sense the pair of particles constitute a *single system*. But as soon as a given component of the spin of either particle is measured, the phase relations are destroyed and the system goes into either the state ψ_c or the state ψ_d in both locations, as the measurement will reveal.[63] Suppose for example that the spin of particle A is measured first, and is found to be $\uparrow$ (along the axis of interest). This immediately tells us that the system has ended up in state ψ_c. We then know, because of the spin correlation of the particles in state ψ_c, that particle B's spin will be found to be $\downarrow$ when it is later measured along the same axis. The correlated actualisation of the potentialities of the states ψ_c and ψ_d appears to be a *nonlocal* process in a certain sense, albeit one that does not permit direct faster-than-light communication.

It is evident that the concept of *state* in quantum theory differs from the concept of state in classical physics. The above result is inexplicable in terms of classical physics.

(e) *Measurement postulate*

Upon measurement of a physical system described by the wave function $\Psi = \sum_n a_n \psi_n$, the coherent linear superposition of states (eigenfunctions of the operator $\hat{O}$) represented by the wave function instantaneously reduces to some

61 Bohm 1951, p. 616.
62 Bohm 1951, p. 616.
63 Bohm 1951, p. 621.

particular one of the eigenfunctions of $\hat{O}$, and the measurement yields one of $\hat{O}$'s eigenvalues o_n. Such reduction is also known as the 'collapse of the wave function'. It may be understood (in terms of the space-time description) as follows. A good measurement effectively destroys definite phase relations between eigenfunctions of the measured variable. The interaction between the observed system and the measuring apparatus always multiplies each part of the wave function corresponding to a definite value of the measured variable, e.g. spin, by a random phase factor, $e^{i\alpha_a}$. The resulting destruction of interference is the standard interpretation's 'collapse' of the wave function.[64]

The *probability* of obtaining the eigenvalue o_n belonging to $\hat{O}$ in any particular measurement of the physical observable O is given by $|a_n|^2$. This is known as the measurement postulate. The measurement postulate represents a physical interpretation of the expansion coefficients in terms of probabilities, the probabilities, however, depending quadratically on these wave functions. This is a crucial conception of quantum theory, in that it provides a connection between the seemingly incompatible wave and particle descriptions of quantum systems.

The average of many measurements of the observable O on identically prepared systems is known as the *expectation value* of the observable O:

$$\bar{o}_\Psi \equiv \sum_s \int \Psi^*(q_1\ldots,s\ldots,t)\hat{O}\Psi(q_1\ldots,s\ldots,t)(dq_1\ldots)$$
$$= \sum_n |a_n|^2 o_n,$$

where $\bar{o}_\psi$ is the expectation value of the observable, Ψ^* is the complex conjugate of Ψ, dq is an element of volume (= dx, dy, dz for a simple particle), s is the spin, and for a normalized eigenfunction $\psi(q)$, $\Sigma_n |a_n|^2$ is unity. In general, when a system is in a given quantum state (when the system's wave function Ψ is given), and provided that Ψ is not an eigenfunction of the operator $\hat{O}$, the observed value of any observable O cannot be predicted. Instead, an average of many measurements is needed to obtain the expectation value.[65] The observed value of the observable O fluctuates about some mean, namely the expectation value.

If the eigenvalues are not discrete but continuous, as is the case with position, probabilities have to be replaced by *probability densities*. The relation between the probability density $P(q,t)$ and the wave function Ψ is

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64 Bohm 1951, pp. 600ff.

65 If, however, as we saw in (c) above, the state of the system is chosen such that $\hat{O}\psi_n = o_n\psi_n$, i.e. where both (i) o_n is an eigenvalue of the operator $\hat{O}$ and (ii) the chosen wave function ψ_n is an eigenfunction belonging to the eigenvalue o_n, then the observable O, e.g. the momentum p_x, has a predictable and reproducible value which never fluctuates. In that case, it is its conjugate variable x which fluctuates, becoming completely indefinite. (After Bohm 1951, pp. 209-10.)

$P(q,t) = \Psi^*(q,t)\Psi(q,t)$.[66] The probability density is used to specify the probability $P(q,t)dq$ of finding the particle associated with the wave function $\Psi(q,t)$ in the infinitesimal volume element dq in the vicinity of q at time t. Using the probability density, we can obtain the expectation value of the particle's position by weighting each position q with its associated probability density and integrating:

$$\bar{q} = \int qP(q,t)dq = \int \Psi^*(q,t)\hat{q}\Psi(q,t)dq.$$ [67]

A consequence of the fact that physical significance of Ψ is confined to its absolute square is that Ψ is not simply a classical probability function representing our knowledge of the system. That's because the superposed alternative possible states of the system can interfere with each other. An example is provided by Young's two-slit experiment, where, on the quantum level, one possibility is reinforced and the other wiped out by interference of the system with itself, as the macroscopic interference pattern obtained reveals.

Consider a modern electron version of Young's 1900 two-slit experiment in the context of Born's interpretation of matter waves. An electron gun at s is used to send electrons, one by one, toward a screen with two tiny slits in it set close together. The electrons all have the same initial momentum, and therefore the

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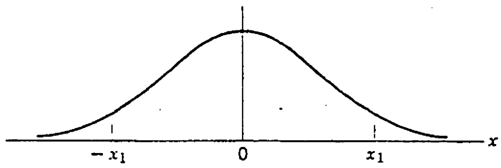
66 This is in position space. In momentum space the corresponding relation is given by $P(\mathbf{k}) = \Psi^*(\mathbf{k})\Psi(\mathbf{k})$, where $\mathbf{k}$ is the propagation vector (the direction in which the phase of the wave changes). Its magnitude or spatial frequency k is $2\pi/\lambda$ [$= \mathbf{p}/\hbar$].
(Bohm 1951, p. 93.)

67 Note that $P(q,t)dq = \Psi^*(q,t)\Psi(q,t)dq$ is an actual probability: a real number – the probability that the particle will be located in the selected infinitesimal volume between q and dq at time t, whereas $P(q,t) = \Psi^*(q,t)\Psi(x,t)$ is the probability density: a *function* – a probability per volume element for a particle to be located near the coordinate q at time t. This is likely to be different at different coordinate-points, which is why it is a function. In the figure below, a probability density P is plotted (in one dimension) as a function of x by the solid curve. The *probability* that a measurement of the location of the particle will find it in an element of the x axis between x and dx is equal to Pdx.



When we make the above measurement of the position of the particle, we may find the particle at any x coordinate in the interval x to $x + dx$, provided the wave function is non-zero in that interval. For that reason, we cannot state that the x coordinate of the particle has a certain definite value. However, we can specify an *average* position of the particle by performing many measurements on identical systems (an *ensemble* of identically prepared systems) described by the same wave function $\Psi(x,t)$, always at the same value of t and recording the observed values of x at which we find the particle. We then take the *average* of the observed values to characterize the position at time t of a particle associated with the wave function $\Psi(x,t)$. This average is the expectation value of x.

same wave function. Each slit is ~~smaller~~ a little larger than the electron wavelength, and the slits are separated by distance of the order of the electron wavelength. There is a second, detecting screen, say a photographic plate, some distance behind the slits (shown to the right of the first screen in our diagram below). What is the probability that an electron will pass through one or the other of the two slits to hit the detecting screen? Well, in classical physics, to find the probability of two independent events, one simply adds the probabilities of each. Classically, the probability of a particle passing through one or the other of the two slits to hit the second screen is given by the probability of the particle passing through one slit *plus* the probability of its passing through the second slit.

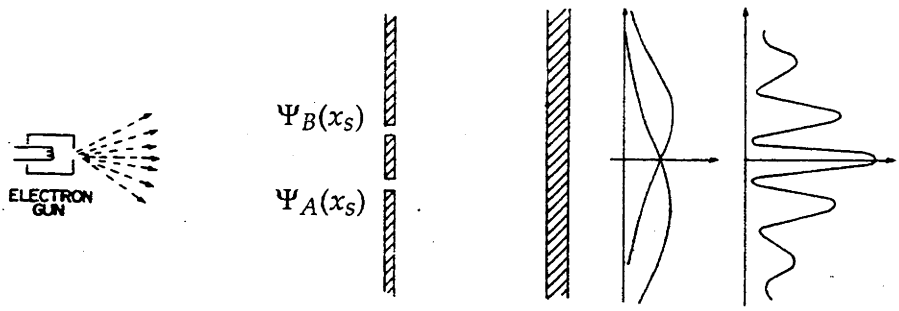
Denote the wave function at slit A by $\Psi_A(x_S)$, and the wave function at slit B by $\Psi_B(x_S)$, where x_s is the value of the coordinate at an arbitrary point in the plane of the slits. Denote the wave function at an arbitrary point behind the slits by $\Psi(x) = \Psi_A(x) + \Psi_B(x)$, where $\Psi_A(x)$ represents that part of the wave reaching the point x that has come from slit A, while $\Psi_B(x)$ represents that part which has come from slit B. (We make the assumption that the experiment is set up so that all contributions to the wave function behind the slits come either from slit A or from slit B.) If only slit A is open, the probability that the particle reaches the point x is given by $P_A(x) = |\Psi_A(x)|^2$, while if only slit B is open, the probability of its doing so is given by $P_B(x) = |\Psi_B(x)|^2$. When *both* slits are open, and if we assume that the wave function is nothing more than a classical probability function, the combined probability that the particle reaches the point x is the simple sum of the probabilities of the positions for each slit taken separately: $P(x) = P_A(x) + P_B(x)$, i.e. $P(x) = |\Psi_A(x)|^2 + |\Psi_B(x)|^2$.

However, electron diffraction experiments show that the wave functions for the electron don't combine in this simple way. If both slits are open, an interference pattern of bright and dark fringes is built up on the detection screen, bright where the waves from the two slits have arrived in phase, and dark when they've arrived out of phase. The bright fringes consist of many tiny white dots each of which is produced by the arrival of an individual electron. The dark fringes indicate the arrival of few or no electrons. The pattern of bright and dark fringes emerges, albeit slowly, a dot at a time, even if the intensity is made so low that only one particle traverses the slit-system at a time – or even if many different photographic plates from different, otherwise identical experiments are superposed.

The interference pattern shows that we need *first* to add up the wave functions (or amplitudes) corresponding to the electron entering slit A and slit B (these being the superposed possibilities) and *only then* square their sum to get the correct probability (rather than squaring each wave function separately and then adding the squares as above). Thus, if both holes are open, the probability $P(x)$ that the electron will reach the point x is generally not, as the classical theory of probability would imply, $P_A(x) + P_B(x)$. Instead, the probability is given by

$$P(x) = |\Psi_A(x) + \Psi_B(x)|^2 = |\Psi_A(x)|^2 + |\Psi_B(x)|^2 + \Psi_A^*(x)\Psi_B(x) + \Psi_B^*(x)\Psi_A(x).$$ [68]

The last two terms are known as *cross terms* (also *interference terms*), and are additional to the single-slit terms $|\Psi_A|^2$ and $|\Psi_B|^2$. The cross terms are generally different from zero and would not be present if the experiment involved a probability distribution of classical particles, coming either through slit A or slit B. The presence of the cross terms is characteristic of the behaviour of waves, and is taken to indicate in the standard interpretation that we've encountered the *wave properties of matter*.[69]



The behaviour of our electron wave functions is similar in many respects to that of ordinary superposed waves, e.g. water waves. The latter can interfere either constructively (when the waves are in phase), or destructively (when the waves are out of phase). It is because of the interference that only the *amplitudes* of the waves are additive, not their *intensities* (given by the square of the amplitude). Likewise, in the case of quantum-mechanical waves, it is only their amplitudes that are additive, not their probabilities (given by the [absolute] square of the

68 Bohm 1951, p. 121.

69 The sum of the last two terms (the cross terms) may also be written as the product $2|\Psi_A(x)||\Psi_B(x)|\cos\theta$, where θ is the phase; i.e. $a_A^* a_B + a_B^* a_A = 2|a_A||a_B|\cos\theta$. This term is equivalent to the $2ab \cos C$ term of the cosine rule of trigonometry, except that the former is complex whereas the latter is not.

The value of $\cos\theta$ can range over -1 to $+1$. Suppose that θ lies in the range of 0° to just under 90°. Then $1 \geq \cos\theta > 0$. In that case the correction term cuts in, ensuring that the two alternative quantum possibility waves constructively interfere, reinforcing each other because they're *in phase*, so that the total probability is greater than the sum of the individual probabilities. When θ lies in the range just over 90° to 180°, i.e. $0 > \cos\theta \geq -1$, the two quantum waves destructively interfere because they're *out of phase*, ensuring that the total probability is less than the sum of the individual probabilities. Only when $\theta = 90°$, i.e. when $\cos\theta = 0$, do the probabilities add up in the classical way.

For *macroscopic* systems, the correction terms average out, leaving us with classical probabilities. The system behaves as if the correction terms didn't exist. (After Penrose 1989, p. 241.)

amplitude). That is why the interference pattern formed by the positions of the particles hitting the detecting screen, with both slits open, is not the simple sum of the probabilities of the positions for each slit taken separately. The mathematics, it turns out, is the *same* as for water-waves, as Feynman points out,[70] save that the amplitudes of the quantum waves are complex rather than real.

The ability of electrons to exhibit the wave-like property of self-interference is characteristic of all quantum-mechanical systems.[71] This property seems to entail a breakdown of the realist attributes of locality and physical identity (or 'genidentity'),[72] because the electron must somehow be able to be in two places at once, else there could have been no self-interference.[73] Being in two places at once is consistent, however, with the electron being a *wave* of some kind prior to observation, and so the locality problem can be evaded for the moment by postulating that the electron is indeed a wave. Upon observation though, the wave needs to undergo a most unwavelike *collapse* from a broad front to a narrow region, so as to be consistent with the electron having a localized position, and to that extent acting like a *particle*. But then it is the *collapse* that seems to entail a breakdown of locality, apparently requiring action at a distance to the extent that the wave function is instantaneously affected throughout all space between the slits and the measuring screen. Either way, the interference spells trouble for our usual picture of reality. And there is even more trouble to come from the same source.

That is because the quantum theory of measurement predicts that the self-interference can be made to take place or not take place at will even *after* the electron has already passed through the slits and travelled much of the way to the detecting screen. Only at that point in the experiment does the experimenter (or a random-number generator) take the decision as to the measurement strategy, using a simple but fast mechanism, thereby determining, as John Wheeler put

70 Feynman, Leighton & Sands 1965, III-1, p. 6.

71 For a detailed discussion of the two-slit experiment, the role of the cross terms in measurement and the quantum theory of measurement generally, see Bohm 1951, Ch. 6, §§3-8, & Ch. 22.

72 See Reichenbach's discussion (1956, pp. 224-36) of material and functional genidentity.

73 With regard to the self-interference, it needs to be borne in mind that Schrödinger's wave mechanics is just one of the formalisms of quantum mechanics, albeit the most simple and natural one, at least according to Bell (1987, p. 187). There are equivalent and alternative (standard) formalisms which make no reference to waves, e.g. Heisenberg's matrix mechanics and Dirac's postulational approach using complex vectors. Apparently, Landé, too, has shown that quantum theory can be 'very efficiently developed without the analogy of wave motion' (Zimmerman 1966, pp. 485-6). What all the standard formalisms have in common, though, is the representation of the state of a microphysical system by a linear combination of eigenfunctions/eigenvectors, and the presence of interference between the possible states of the system, and the calculation of probabilities from averages taken over ensembles. What is really at issue is the interpretation of this quantum-mechanical picture, and in particular, the self-interference.

it, 'what kind of indelible evidence shall be produced: "which-slit" evidence, or "double-slit" evidence',[74] i.e. evidence consistent with a scatter pattern, or with an interference pattern.[75]

Such 'retroaction', Bohr explicitly pointed out in 1949,[76] is to be *expected* on his (Copenhagen) interpretation of quantum mechanics with its doctrine of *complementarity*, according to which the dynamic attributes such as position and momentum do not exist until they are actually observed – and even then they are relational – manifestations of the entire experimental arrangement. In particular, once we locate the electron, we lose information about its momentum. As soon as we do so, we also lose information about its wavelength, as is implied by de Broglie's relation $\lambda = h/p$ connecting wavelength and momentum. But if the interference fringes still existed, we could measure the wavelength from their spacing, in violation of the Heisenberg indeterminacy. Therefore the interference pattern itself must be destroyed. This can also be seen if we apply the Heisenberg indeterminacy principle not just to quantum entities such as electrons, but also to the macroscopic measuring apparatus such as the two-slit screen used in the experiment. If the position of the slits can be known only to an accuracy equal to or greater than the separation between the fringes, the fringes will be impossible to observe, as Bohr gleefully pointed out to Einstein in their original Solvay debate.[77]

Following up on Bohr's remark, Wheeler in 1977 described seven different versions of a *gedankenexperiment* in which such retroaction would be expected to occur, their common feature being that each imposed a choice between complementary modes of observation.[78] Wheeler's experiment (beam-splitter version) was successfully carried out five years later by groups working independently at the Universities of Maryland and Munich.[79]

The lesson seems to be, as Heisenberg once put it, that we learn, not about nature itself, but nature exposed to our methods of questioning. Indeed, according to Wheeler and the Austin School of the Copenhagen interpretation, the lesson is that 'the past has no existence except as it is recorded in the present'. And more generally, no phenomenon is a phenomenon until it is an *observed* phenomenon. 'The universe does not "exist, out there", independent of all acts of

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74 Wheeler 1978, p. 28.

75 Notice that it is the *type* of pattern that will be observed that can be selected retrospectively, but not where the individual hits will occur on the screen in each type of pattern.

76 Bohr 1949, p. 230.

77 This last point is after Goswami 1997, p. 109.

78 J.A. Wheeler, 'The "past" and the "delayed-choice" double-slit experiment'. Presentation made to a conference at Loyola University, New Orleans, June 2-4, 1977, reproduced in Marlow 1978, pp. 9-48.

79 Reported by Horgan 1992, p. 75.

observation. Instead, it is in some strange sense a participatory universe.'[80]

There are other consequences of the standard interpretation of quantum mechanics connected with measurement. There is the consequence that later knowledge replaces earlier knowledge in a classically unexpected way. Suppose that we know the wave function Ψ of a system. The wave function provides a complete description of the system. Suppose also that we know that the value of a physical variable A of the system, e.g. the momentum of a particle at time t_0 is a. In that case, a is one of the eigenvalues of the physical variable represented by the operator $\hat{A}$, and it has an associated eigenfunction ψ_a. We say the system is in the state $\Psi = \psi_a$, and that ψ_a completely specifies that state. Seeing that it completely specifies the state, ψ_a now plays a role analogous to that played by the boundary conditions in classical mechanics. Future predictions rely on this specification of the 'initial' state of the system.

Now suppose that later on, at time t_1 we know the value of another physical variable B, the position (which we've measured), and that value is b. The question is, how is this additional information encoded into the theory? We've seen that in classical mechanics, to do so would be redundant, seeing that our knowledge of the boundary conditions was complete. The additional information is is far from redundant in quantum mechanics, though. Seeing that the second piece of information is later than the first, and that the first provided us only with a *probability* for obtaining the value b (in the present case actually giving no useful information whatever), clearly the later information supersedes the earlier.[81] Additionally, finding out the value of the position b has, in a sense, 'destroyed' our earlier-obtained knowledge of the value of the momentum a, since a and b relate to incompatible measurements (the measured momentum of the system could now have any value). In the circumstances, it is clear that only the *new* information can be taken as specifying the boundary conditions of the system. That is, for all even later predictions of measurement results, e.g. at time t_2, we must use the wave function $\Psi = \psi_b$, rather than the wave function $\Psi = \psi_a$ which would be useless. Eddington described the physicist-observer as being 'like the comedian with an armful of parcels; each time he picks up one he drops another'.[82]

Notice that the above-kind of loss of information reveals more than just the indeterminacy of quantum theory. It also shows that not just any kind of indeterminacy will do. The indeterminacy must be specifically rule-like to ensure consistency with what is observed. In particular, the precise nature of the indeterminacy must be consistent with both (a) the reduction postulate of orthodox quantum theory and (b) its principle of complementarity (or their equivalents in a

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80 Wheeler 1978, p. 41.

81 After Unruh 1995, pp. 44-9; Davies 1984, pp. 61-2.

82 Eddington 1935, p. 98.

non-orthodox interpretation). Here is why.

Take (a). Consider the coherent linear superposition of eigenfunctions represented by the wave function $\Psi = \sum_n a_n \psi_n$, where ψ_n are eigenfunctions of the operator $\hat{O}$. We've seen that upon measurement, the superposition instantaneously reduces to some particular one of the eigenfunctions, and the measurement yields one of $\hat{O}$'s eigenvalues o_n, with a certain probability for each. After a particular measurement has yielded the eigenvalue o_n belonging to the operator $\hat{O}_n$, the system must *remain* in the state described by the eigenfunction ψ_n – else an immediate repeat measurement wouldn't generally yield the same result. It is easy to imagine non-deterministic models that would violate this requirement.

Now take (b). For previous knowledge to be replaced by new knowledge in the above sense, the later measurement must be of an *incompatible* variable. For example, if the initial sharp measurement was of momentum, the new sharp measurement must be of position, the two being complementaries. Mere indeterminacy doesn't require this, but only a certain *kind* of indeterminacy – namely one that is consistent with the complementarity of quantum mechanics, i.e. the existence of the canonically conjugate relation between certain pairs of variables, described by Bohr's principle of complementarity and the Heisenberg indeterminacy relations.

There are also rules to calculate the probability of *compound* events – events that can be broken down into a sequence of steps, each of the steps having some probability. For example, the amplitude for a particle to go from the electron gun at s to point x on the detecting screen by way of, say, slit A in the above experiment, is equal to the amplitude to go from s to slit A (i.e. the amplitude to go part-way) *multiplied* by the amplitude to go from slit A to x (i.e. the amplitude to go the rest of the way). That is, we do not *add* (superpose) the amplitudes, but multiply them. Once we've done this, we proceed in the usual way and take the absolute square of the *resulting amplitude* to obtain the probability for the entire sequence of steps.[83]

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83 Using Dirac's notation, the amplitude for the above sequence could be written as $\langle x|s\rangle_{via\,A} = \langle x|A\rangle\langle A|s\rangle$. The amplitudes are to be read from right to left. Thus, $\langle A|s\rangle$ expresses the amplitude for the particle to arrive at slit A when it is let out at the source s, and $\langle x|A\rangle$ the amplitude for it to arrive at arbitrary point x on the detecting screen having passed through slit A. In each amplitude the expression to the right of the vertical line always gives the starting condition, and that to the left of the vertical line the finishing condition. For example, in the right-hand amplitude, $|s\rangle$ gives the starting condition – the particle is emitted at s, and $\langle A|$ the finishing condition – it arrives at slit A. Likewise, in the left-hand amplitude $|A\rangle$ gives the starting condition – the particle leaves slit A, and $\langle x|$ the finishing condition – it arrives at x. The amplitude for the entire sequence, obtained by multiplying the two constituent amplitudes, is expressed by $\langle x|s\rangle_{via\,A}$. The probability for the particle reaching x via slit A is then given by $|\langle x|s\rangle_{via\,A}|^2$. For a discussion, see Feynman, Leighton & Sands 1965, III-3, pp. 3-7. They write (p. 3), 'In summary, if events occur in succession – that is, if you can ana-

(f) *Reduction postulate (also known as projection postulate)*

We have seen that upon measurement, the coherent linear superposition of states or eigenfunctions represented by the wave function $\Psi = \sum_n a_n \psi_n$, where ψ_n are eigenfunctions of the operator $\hat{O}$, instantaneously reduces (Ψ collapses) to some particular one of the eigenfunctions, and the measurement yields one of $\hat{O}$'s eigenvalues o_n, with a certain probability for each. After a particular measurement has yielded the eigenvalue o_n of $\hat{O}_n$, the system *remains* in the state described by the eigenfunction ψ_n – hence an immediate repeat measurement yields the same result.[84] In other words, once we've obtained such an eigenfunction, we must be able to go on, at least in principle, to measure the observable again and again, in time so short that the wave function hasn't changed significantly (except for the phase factor which isn't relevant), obtaining the same result each time.[85]

The time period between the original and repeat measurements must be short in order to obtain the same value because, unless the ψ_n is also an eigenfunction of $\hat{H}$, the system does not remain in that state. Instead, the function develops in accordance with Schrödinger's equation.

The conditions for the actualisation, in the reduction process, of any particular one in preference to another of the various superposed complex-number-weighted possible states or *potentia* is nowhere made explicit in the formalism, the theory giving only the *probabilities* for such actualisation. Indeed, according to the standard interpretation, no such conditions for the actualisation of individual potentia exist. This lack leads to fundamental interpretational difficulties of the kind exemplified in the 'Schrödinger's cat' thought experiment.[86] A related difficulty, touched on in our discussion of Schrödinger's cat, is that the reduction postulate seems to entail an absolute frame of reference – that of the measuring apparatus, making it internally inconsistent when taken together with special relativity.

(g) *Use of macroscopic measuring apparatus*

The measurement postulate is usually taken to presuppose that all measurements in quantum mechanics are to be made with macroscopic observing instruments, i.e. classically describable measuring apparatus, and that macro-observables

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lyse one of the routes of the particle by saying it does this, then it does that – the resultant amplitude for that route is calculated by multiplying in succession the amplitude for each of the successive events.' See also Feynman 1985, pp. 59-63ff.

84 Goswami 1997, p. 68.

85 After Bohm & Hiley 1993, p. 18.

86 According to Shimony, this lack is a crucial weakness in the framework of quantum mechanics. (Shimony 1989, p. 389.)

retain sharp values at all times.[87] For example, position measurements are to be made with macroscopic rods placed between macroscopically separated marks, and time is to be read by a macroscopic physicist from a macroscopic clock.[88] Bohr, in particular, always insisted on the 'indispensable use of classical concepts in the interpretation of all proper measurements'.[89] Consequently, quantum theory seems to require that the world be divided into two – a quantum-mechanically described system, and a 'classical' remainder. The division may be made in particular applications in one way or another according to the degree of accuracy and completeness aimed at. Thus, there appears to be an essential and arbitrary cut between measuring and measured systems, as Bohr, Schrödinger and Bell have all emphasized.

Such a cut exists even in the Dirac/von Neumann approach, in which the world is represented entirely in quantum-mechanical terms. In that approach, the problem is even more intractable than in Bohr's approach. If *everything*, including measuring instruments, is to be represented quantum-mechanically in terms of quantum waves undergoing unitary evolution, then there is nothing *special* about a measuring instrument that could bring about a wave function collapse. Therefore, rather than collapsing, the wave function necessarily develops into a sum of parts that corresponds to incompatible *macroscopic* possibilities. But that seems wrong as such are never observed. A cut needs to be put in by hand somewhere in the chain of measurement to accord with the fact that macroscopic observables have determinate values. This is the *measurement problem* of the standard interpretation.

It seems to follow, as Bohm writes, that quantum theory 'does not deduce classical concepts as limiting cases of quantum concepts' after all, differing in this regard from relativity theory in which Newtonian concepts are indeed deduced as limiting cases of the theory.[90] Instead, quantum theory simply *presupposes* the classical level and the general correctness of classical concepts on that level. In other words,

> quantum mechanics... contains classical mechanics as a limiting case, yet at the same time it requires this limiting case for its own formulation.[91]

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87 The retention of sharp values is connected with the postulate of linear superposition. As we saw in a note to (a) above, it is assumed that in a quantum-mechanical measurement, the measured system does not act back on the macroscopic measuring apparatus.

88 Zimmerman 1966, pp. 489-90.

89 Bohr 1935b, p. 701. (In Wheeler & Zurek 1983, p. 150.)

90 Bohm 1951, p. 625.

91 Landau & Lifshitz 1965, p. 3.

(h) *Heisenberg's indeterminacy principle*

Heisenberg's indeterminacy principle is unremarkable in the context of the preceding postulates. That is because two non-commuting operators $\hat{O}$ and $\hat{O}'$ cannot have the same eigenfunctions.[92] That being the case, the theory predicts that any ensemble of particles will have a spread of eigenvalues for the observables represented by $\hat{O}$ and $\hat{O}'$, e.g. x and p_x, such that $\Delta x \ \Delta p_x \geq \frac{\hbar}{2}$. That is, if many particles are assembled within a small space (Δx), the group must have a large spread of x-momenta ($\Delta p_x \geq \frac{\hbar}{2}\frac{1}{\Delta x}$). Alternatively, if a group of particles all having about the same x-momentum is assembled (Δp_x is small), they must be spread over a large region of space ($\Delta x \geq \frac{\hbar}{2}\frac{1}{\Delta p_x}$). Likewise, for an ensemble of radioactive or unstable particles or microphysical systems, the spread of the energies ΔE which will be observed and the spread of the Δt at the time of emission will be related by $\Delta E \ \Delta t \geq \frac{\hbar}{2}$. That is, the members of the ensemble will not all radiate precisely the same energy, nor will they all radiate at the same time.

There are as many indeterminacy relations as there are pairs of operators not having the same set of eigenfunctions.[93] These relations also apply when we go from an ensemble of particles to the single particle case, i.e. to an ensemble of measurements of identically prepared single particles. Again, over many runs of the experiment, the same relations will be found to apply. For example, taking the latter one, it will be found that the spread of the energies ΔE which will be observed and the spread of the Δt at the time of emission will be related by $\Delta E \ \Delta t \geq \frac{\hbar}{2}$.

Another way of proceeding is to derive the indeterminacy relations by combining the de Broglie-Einstein relations, $p = h/\lambda$ and $E = h\nu$ with simple mathematical properties that are universal to all waves, namely $\Delta x \Delta k \geq 1/4\pi$, and

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[92] Take the operators for position and momentum. They do not commute, i.e. the results of $\hat{x}.\hat{p}$ and $\hat{p}.\hat{x}$ applied to the same wave function are different. Consequently, we cannot identify a function that would be an eigenfunction of both position and momentum. It follows from the above postulates of quantum mechanics that there can be no state in which both the physical observables x and p_x have a well-defined value. (After Prigogine & Stengers 1984, p. 223.) See also Sachs 1988, pp. 130-2 for some discussion.

[93] After Zimmerman 1966, pp. 493-4. Zimmerman remarks (p. 494) that the time-energy indeterminacy relation has often been interpreted as saying that, if the energy of the system is to be measured with an accuracy ΔE, then the experiment to measure E must have a duration at least as long as the Δt given by the inequality. However, the analysis of Aharonov & Bohm 1961 (see the notes to §1.3.2) seems to show that this is incorrect, and that an arbitrarily accurate measurement of energy can be carried out in an arbitrarily short period of time.

$\Delta t \Delta \nu \geq 1/4\pi$ (where k is the spatial frequency or wave number, $1/\lambda$, i.e. the number of waves per unit length).[94] The reason why the de Broglie-Einstein relations are combined with properties universal to all waves is because of 'wave-particle duality'. To calculate anything in quantum mechanics, such as the probable future history of a particle, we need to treat the system in question including the particle itself as a wave of some kind. This is the main significance of de Broglie's relations.

Now consider the indeterminacy relation $\Delta p_x \Delta x \approx h$ in the context of de Broglie's $\lambda = h/p$ (or $p = hk$). The de Broglie equation creates a relationship between wave numbers (or spatial frequency) and momentum, which is not present in classical waves. A classical electromagnetic wave with a given wave number k, for example, can have arbitrary amplitude and, therefore, arbitrary momentum.[95] The position is different in quantum mechanics. Take a material particle having a definite momentum p_x, i.e. the momentum is such that it is fully known; there is no indeterminacy in it ($\Delta p_x = 0$). According to de Broglie's relation, such a particle is associated with a matter wave of *single wavelength* λ *and single frequency* ν (a sine wave); we know from de Broglie's relation that if there is no indeterminacy Δp_x in the associated particle's momentum, there can be no indeterminacy in the wavelength [or wave number] ($\Delta\lambda = 0$).[96] And vice versa: given a wave of determinate wavelength or wave number, we would immediately know that the momentum $p_x = h/\lambda$ of the associated particle must also be definite. That's because, according to de Broglie's relation, if there is no indeterminacy in the wavelength of the matter wave, there can be no indeterminacy in the momentum of the associated particle. The wave function of particle in such a state is called a momentum eigenfunction. When the momentum isn't known, the particle is represented in a superposition of momentum eigenfunctions.

So far, so good. But consider this. The matter wave (a sine wave) associated with the particle of definite momentum p_x has the same (sinusoidally varying) *amplitudes* over the entire range of x at a given time, simply by virtue of the fact that it is a sine wave. Now, the absolute square of the amplitude of a matter wave at any location gives the *probability* of finding the associated particle at that location. Therefore the probability of finding the particle is not confined to any particular location, or range of x. The particle could be located *anywhere* within that range, and the probability of finding it at any particular location must be zero. Thus the indeterminacy in its location is infinite ($\Delta x = \infty$).[97]

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[94] The following discussion of the derivation of the uncertainty relations loosely follows Resnick 1972, pp. 185-91; Eisberg & Resnick 1974, pp. 83-4. The reader is referred to these texts for the mathematical details.

[95] Bohm 1951, p. 100.

[96] 'Because of the de Broglie relation, a definite momentum implies a definite wave number k' [where $k = 2\pi/\lambda$]. (Bohm 1951, p. 100.)

[97] More explicitly, the wave function of a free particle in one dimension is written:

Hence, if the momentum of the particle is perfectly determinate, *ipso facto* its position must be completely unknowable. Its state involves a superposition of *all different positions at once*.[98] In this way, we can see how the uncertainty principle has predicted a limitation (a halving in a certain sense), in the number of independent classical dynamic variables. We started by treating *momentum* as a classical variable, i.e. as both real and knowable in principle to an arbitrary degree of accuracy. But when we did so, we found that we could no longer treat *position* as such a variable. Note that such a halving is implicit in the operator formalism in its alternative 'position space' and 'momentum space' representations of the same quantities. In position space, both the position and momentum operators are expressed in terms of (derivatives of) x alone; in momentum space, both operators are expressed in terms of (derivatives of) p alone. We always use *either* a position space representation *or* a momentum space representation to describe the system. We never use both at once.

The only way the particle can be sharply localized is by associating it with a wave consisting of an infinitely large number of superposed sine waves, differing infinitesimally in λ and ν, the combination of which gives a *group* wave having an infinitesimal spread in space. A group wave is also called a *wave packet*. A wave packet is defined as a group of waves of limited duration having a range of values of frequency and wavelength so chosen that their amplitudes interfere constructively over only a small region of space, outside of which they interfere destructively thereby producing amplitudes that rapidly approach zero. The wave packet has the shape of the Gauss error curve. A wave packet can be made by superpositions of many different kinds. The wave function of a particle represented by such a group wave is called a position eigenfunction (delta function). The group or packet could constitute a single sharp

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$$\Psi(x,t) = r(\cos\theta + i\sin\theta),$$

where the wave function represents a pair of sinusoidal waves displaced by a quarter wavelength, and r is the amplitude of the waves. We may write this in brief as: $\Psi(x,t) = R(x,t) + iI(x,t)$, where $R(x,t)$ and $iI(x,t)$ are its respective real and imaginary parts, in this case representing $r(\cos\theta)$ and $r(i\sin\theta)$, respectively. To obtain the quantum mechanical probabilities, we multiply this expression by its complex conjugate, $\Psi^*(x,t) \equiv R(x,t) - iI(x,t)$, obtaining

$$\Psi^*(x,t)\Psi(x,t) = [R(x,t)]^2 + [I(x,t)]^2,$$

which is the sum of the squares of two real functions. (After Eisberg & Resnick 1974, p. 148.)

Now, for two sinusoidal waves separated by a quarter wavelength, the sum of the squares of their amplitudes is always 1 if the amplitude r is 1 (if the wave is normalized), i.e. $\cos^2\theta + \sin^2\theta = 1$, which is just an expression of Pythagoras' theorem. In other words, the probability of finding the particle is the same everywhere in space, and the particle could be anywhere – we have no information as to its whereabouts. For some discussion, see e.g. Barbour 1999, Ch. 13.

98 Penrose 1994, p. 278.

pulse, the time of arrival of which is known with certainty ($\Delta t = 0$). Hence we would thereby also know with certainty, on the basis of de Broglie's relation, the *position* of the particle associated with the wave pulse ($\Delta x = 0$). But in this case, the superposed waves would have wavelengths and frequencies ranging from zero to infinity. It follows that we could know nothing about the frequency of the pulse itself ($\Delta \nu = \infty$).[99] That being the case, de Broglie's relation tells us that we could know nothing about the *momentum* of the particle associated with the pulse ($\Delta p_x = \infty$). (Its state involves a *superposition of all different momenta.*) Again, we can see how the uncertainty principle has predicted a limitation (a halving) in the number of independent classical variables. In this case we started by treating *position* as a classical variable, i.e. as both real and knowable in principle to an arbitrary degree of accuracy. But when we did so, we found that we could no longer treat *momentum* as a classical variable.

Heisenberg's indeterminacy relations reflect for a physical particle the above remarkable duality in the mathematical representation of the particle by superposition of waves. Still staying with the special case of wave functions that are eigenfunctions, if we know a particle's position, it is represented by one kind of waveform, which is sharply localised. But if we know its momentum, it needs be represented by another kind of waveform, spread out everywhere, and incompatible with the first. And going to the general case, any wave function which is *not* an eigenfunction of position or momentum may be regarded as a superposition of *either* momentum or position eigenfunctions. The functions representing the two waveforms are Fourier transforms of each other.[100]

It is clear that Heisenberg's indeterminacy relations are a necessary consequence of wave-particle duality (or more generally, quantum complementarity). If there existed no quantum complementarity in the world (if Planck's constant had the value of zero), there would exist no Heisenberg indeterminacy relations. For example, the momentum-position relation would have the form $\Delta p \Delta q \geq 0$, and both Δp and Δq could be zero. Likewise, the closely related energy-time relation would have the form $\Delta E \Delta t \geq 0$, and both ΔE and Δt would be zero.

As Heisenberg succinctly put it:

> We have not assumed that the quantum theory, as opposed to the classical theory, is essentially a statistical theory, in the sense that only statistical conclusions can be drawn from exact data... In the formulation of the causal law, namely, 'If we know the present exactly, we can predict the future', it is not the conclusion, but rather the premise which is false. We *cannot* know, as a matter of principle, the present in all its details.[101]

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99 Resnick 1972, p. 187.

100 Fourier's theorem states that any wave can be written as a unique sum of sine waves.

101 Quoted in Eisberg & Resnick 1974, p. 88.

It is evident that there is nothing remarkable about the indeterminacy relations themselves, given quantum complementarity. The entire mystery of quantum mechanics lies, not in the indeterminacy but in the *complementarity*, and its interpretation – which is inseparable from an interpretation of h.

(i) *Spin*

Identical particles in Schrödinger quantum mechanics need to be ascribed an additional degree of freedom that has no exact classical counterpart. That degree of freedom is the particle's *spin*, which may be described as a kind of intrinsic angular momentum, present even when the particle is otherwise at rest. It is often denoted by S, and is of magnitude $s\hbar$, where s is either an integer (0, 1, 2, ...) or a half-integer (1/2, 3/2, ...). Particles with integer spins are called *bosons*. Examples are the photon, which has spin 1, the pion with spin 0 and the hypothetical graviton with spin 2. Particles with half-integer spins are called *fermions*. Examples are the electron, proton, neutron, neutrino, and their antiparticles, all of which have spin 1/2. Another example is the omega baryon, which has spin 3/2. The component of the spin vector **S** of any elementary particle in any reference direction along which the spin may be measured (such reference direction usually defined by a magnetic or electric field) can generally take on $2s + 1$ possible values, from $-s$ to $+s$ in increments of 1. This classically unexpected feature is known as space quantization. It means that whichever reference axis is selected, the only possible values that can be obtained for the spin component along that axis are $\pm 1/2\hbar$. This is so even if the spin was known, pursuant to a previous measurement, to point along a *different* axis.[102]

For example, the 'north pole' of an electron (in effect, a tiny spinning magnet) may point either parallel or antiparallel to the applied magnetic field, but in no other direction. In other words, the electron's spin vector can take only one of two possible spin orientations with respect to the reference direction: $S_i = -1/2$ or $+1/2$ (antiparallel or parallel to it), with a component of spin angular momentum of $\pm 1/2\hbar$ along that axis. Thus, the intrinsic spin angular momentum can be either $-1/2\hbar$ or $+1/2\hbar$, these being the eigenvalues of the spin component in the reference direction, the difference between the two being $\hbar$. Any possible spin

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[102] The space quantization makes quantum spin different from ordinary spin. Consider the spin angular momentum of a macroscopic object such as an apple. We can represent the angular spin momentum vector of the apple by an arrow piercing it through the core. A projection of this arrow in the direction of any coordinate axis in 3-space would be a component of the apple's spin along that axis. It's easy to see that in the case of the apple, the spin vector (the arrow) must have some particular direction, and so a corresponding component of spin relative to the coordinate system. Depending on the direction of the spin vector, the component could be any value from zero to the total angular momentum of the ~~earth~~ apple. This is not the case for quantum spin.

state of a single particle can be represented as a linear superposition of just these two orthogonal or *base* states.

The photon, on the other hand, has three possible spin orientations available to it: S_i = –1, 0, or +1 (antiparallel, perpendicular, or parallel). In practice, though, owing to the fact that photons travel at the speed of light, only the states –1 or +1 (antiparallel and parallel to the direction of propagation) can manifest themselves. These two states correspond to the two independent classical states of polarization of light.[103]

If the spin of a particle has been determined to point in some particular direction along a reference axis, say, 'up' the z axis (i.e. $+1/2\hbar$), a repetition of the measurement along that axis will give the same result every time. But if some other axis is chosen for the measurement of the particle's spin, inclined at an angle θ to the original, then *probabilities* as to whether the spin will be 'up' or 'down' along the new reference axis enter the picture in an essential way. If the angle is small, it is still likely that the same spin direction will be obtained. But as the angle is increased, the likelihood reduces. For particles of half-integer spin, the probability that the spin is up along the new reference axis z' is given by $\cos^2(\theta/2)$, while the probability that the spin is down has a probability of $\sin^2(\theta/2)$. The corresponding rules for particles of integer spin are, respectively, $\cos^2\theta$, and $\sin^2\theta$, as we saw in our discussion of photon linear superposition in (d) above.[104]

Two important related features of spin should be noted. The first is that spin cannot be derived from Schrödinger's theory, but must be introduced in that theory as a separate postulate. The reason is that the theory is an approximation which ignores relativistic effects. The spin can be derived, however, from Dirac's relativistic theory, which uses the same postulates as Schrödinger's theory, but replaces the classical energy equation $E=\left(p^2/2m\right)+V$ by its relativistic equivalent $E=\left(c^2p^2+m_0{}^2c^4\right)^{1/2}+V$.[105]

In the non-relativistic Schrödinger formalism, some of the effects of spin can be incorporated into the Schrödinger equation by allowing the wave function Ψ(q,t) to become the *two*-component object

$$\begin{bmatrix}\Psi_1(q,t)\\ \Psi_2(q,t)\end{bmatrix}.$$

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103 After Shu 1982, pp. 49-50.

104 There the rule was given with respect to the optic axis, *perpendicular* to the plane of the polarizing material. Here the rule is given with respect to the plane of polarization of the polarizing filter. For that reason the roles of $\cos^2\theta$ and $\sin^2\theta$ in giving the probabilities are interchanged.

105 Eisberg & Resnick 1974, pp. 301-2.

The Schrödinger equation then reads

$$\hat{H}\begin{bmatrix}\Psi_1(q,t)\\ \Psi_2(q,t)\end{bmatrix}=i\hbar\frac{\partial}{\partial t}\begin{bmatrix}\Psi_1(q,t)\\ \Psi_2(q,t)\end{bmatrix},$$

where $\hat{H}$ is the total Hamiltonian, generally consisting of both space and spin dependent components.[106]

The second is that, unlike ordinary angular momentum, spin is not a function of time and position, meaning that two otherwise identical states can have different spins. In fact, whenever two states exist having the *same* space and time dependence, they *must* have different spins. Thus, *spin must be considered as part of the wave function itself.* All of the eigenfunctions in the expansion of a wave function of a single particle must be of the same spin (as the particle itself).[107]

And finally, it is worth mentioning that a spin 1/2 particle (such as an electron or neutron) *needs to rotate twice, i.e. by 4π, or by 720°, to return to its initial physical state.* This is indicated by its spin being $1/2\hbar$ and not $\hbar$, i.e. h/720° and not h/360°. After only a 360° rotation, the particle's spin eigenfunctions are the *negatives* of the initial spin eigenfunctions, and so differ by a phase factor.[108] A further rotation of 360° is required to restore the original state. This is the reason why the magnetic field – and so the gyromagnetic ratio – due to the electron's spin, is twice the value expected on the basis of using a classical model such as an electrically charged ball. A similar property would be possessed by a traveller on a surface with the connectivity of a Möbius strip. The traveller would need to circle twice (rotate by 720°) to return to his/her starting configuration.

(j) *The Pauli exclusion principle*

The principle states that no two particles with half-integer spins, such as electrons, can be in precisely the same state (described by the same wave function), when spin is included in the description of the state. The origin of the principle is mathematical, to do with the existence of symmetric and antisymmetric eigenfunctions and the effects of the exchange of particle labels such as 'right' and 'left'. We shall not go into the details here – save to mention that two-particle systems of identical bosons are described by *symmetric* (or 'even') wave functions, whereas two-particle systems of identical fermions are described by *antisymmetric* (or 'odd') wave functions. The difference between the two kinds of wave functions is that symmetric wave forms are unchanged by reflection (exchange of right and left), whereas antisymmetric waveforms reverse sign under

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106 Davies 1984, pp. 84-5.
107 This paragraph is after Longini 1970, p. 39.
108 Davies 1984, p. 83.

reflection (more correctly, under parity reversal *P*).[109] In 1925 Wolfgang Pauli found that this change of sign under *P* implied that if two particles are described by an antisymmetric total eigenfunction, they cannot both be in the same state with the same space and spin quantum numbers. (The value of the eigenfunction would be zero for such a case.) Therefore, in a multielectron atom there can never be more than one electron in the same quantum state. This is Pauli's exclusion principle. An alternative, stronger expression of it is: '*A system containing several electrons must be described by an antisymmetric total eigenfunction.*'[110]

The exclusion principle, when coupled with the existence of quantized spin, leads to an explanation of a great many otherwise puzzling features of the world, including, just to name a few, the periodic table of elements, the difference in the behaviour of electric conductors and insulators, superconductivity, the existence of dwarf and neutron stars, and the fundamentally different statistical behaviour of particles of integer and half-integer spin. This latter difference is a big one in physics. The rules obeyed by integer-spin particles are called Bose-Einstein statistics, and those followed by half-integer spin particles are called Fermi-Dirac statistics.[111] These rules explain why, for example, we perceive well-defined electromagnetic waves such as light waves and radio waves but never electron waves, even though electrons possess an associated wave just like photons do.[112]

(k) *Essential complexity of the quantum-mechanical description of state*

The quantum wave function $\Psi(x,t)$ is complex. The complexity of the wave function means that there are two parts or two functions to the full function, a real part and an imaginary part. This is in contrast to the wave functions of classical mechanics, such as that modelling for example a vibrating string which has only a real part to it.

To take a simple example of a complex wave function, the wave function for a free particle (i.e. in the absence of an accelerating field of force) in one dimension has the form

$$\Psi(\mathrm{x},\mathrm{t}) = A[\cos(\mathrm{kx} - \omega\mathrm{t}) + i\,\sin(\mathrm{kx} - \omega\mathrm{t})],$$[113]

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109 Parity is a mathematical property of the quantum wave function, related to but not equivalent to mirror reflection invariance), and it has two values – even or odd. If a wave function remains unchanged when the sign of one of the three spatial variables is reversed, it has 'even' parity, if not, it has odd parity; more specifically, eigenfunctions satisfying the relation $\Psi(-x, -y, -z) = \Psi(x, y, z)$ are said to be of even parity, while eigenfunctions satisfying $\Psi(-x, -y, -z) = -\Psi(x, y, z)$ are said to be of odd parity.

110 Eisberg & Resnick 1974, p. 335.

111 For a discussion of the two kinds of rules, see e.g. Eisberg & Resnick 1974, Ch. 11; Penrose 1989, pp. 277-8; Feynman, Leighton & Sands 1965, III, Ch. 4.

112 See e.g. Davies 1984, pp. 144-5; Gribbin 1985, pp. 95-9.

113 The corresponding classical wave function describing a simple sinusoidally travel-

where A is the amplitude or maximum displacement of the wave, $k = 2\pi/\lambda$ is the wave number, and $\omega = 2\pi\nu = ck$ is the angular frequency, from $c = \nu\lambda$. (The use of k and ω are convenient in calculation because they keep variables out of denominators, and 'absorb' a factor of 2π that would otherwise appear every time we write a wave function.) Note that λ (in $2\pi/\lambda$ above) is the *de Broglie* wavelength of a particle moving with a clearly defined momentum and energy defined by $\lambda = h/p$, or, more specifically in our one-dimensional case, $\lambda = h/p_x$. It is basically the inverse of the particle's momentum. (If we set $h = 1$, then $\lambda = 1/p$, and vice versa.) The phase velocity $V = \nu\lambda$ is the velocity at which any one crest travels along the x axis at distance λ in time τ (where τ is the *period* or the constant time between crests), i.e. $V = \lambda/\tau = \nu\lambda$. As for the notation $\Psi(x,t)$, it's there simply to remind us that the amplitude or displacement is a function of both the location along the axis of propagation of the wave and the time t.

What is the physical significance, if any, of the fact that the quantum wave function is complex?

The first thing to say is that, since no complex quantity can be measured by any actual physical measuring instrument, we know that we cannot ascribe a physical existence to the wave described by the wave function, at least in the same simple way that e.g. water waves have a physical existence.

The second thing to say is that complex numbers occur in the equations of classical physics, too. Complex numbers have become, since Faraday, an indispensable part of physics, entering into its equations both as a kind of mathematical 'shorthand' or mere computational aid to avoid having to do trigonometry.[114] When Richard Feynman described classical waves in *The Feynman Lectures on Physics*, he defined the intensity of the waves as the mean over time of the square of the wave amplitude, and then used complex numbers as a mathematical 'trick' to simplify the analysis.[115] But the trick does not work in quantum mechanics, as Feynman admits – in quantum mechanics, the connection between complex quantities and theory seems more intrinsic. Complex numbers

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ling wave (of frequency ν, wavelength λ, and of constant unit amplitude moving with constant velocity in the direction of increasing x) is $\Psi(x,t) = A \sin 2\pi (x/\lambda - \nu t)$; i.e. $A \sin(kx - \omega t)$. See Eisberg & Resnick 1974, p. 78 for details.

114 Cramer, for example, writes, 'Complex functions are also found in classical physics, but are invariably interpreted either (1) as an indication that the solution is unphysical, as in the case of the Lorentz transformations with $v > c$, or (2) as a shorthand way of dealing with two independent and equally valid solutions of the equations, one real and one imaginary, as in the case of complex electrical impedance. In the latter case the complex algebra is essentially a mathematical device for avoiding trigonometry, and the physical variables of interest are ultimately extracted as the real (or imaginary) part of the complex variables. Never in classical physics is the full complex function "swallowed whole" as it is in quantum mechanics. This is the problem of complexity.' (Cramer 1986, p. 653.)

115 In Vol. I, Ch. 23 he spells out just how the mathematical trick works.

occur in its equations in a way that seems more fundamental than just computational shorthand for the mathematical convenience of physicists, e.g. enabling them to avoid having to do trigonometry. Complexity seems forced on them by the nature of the underlying phenomena that their equations attempt to describe or model. As Feynman puts it:

> [I]n quantum mechanics it turns out that the amplitudes *must* be represented by complex numbers. The real parts alone will not do.[116]

The assumption that the amplitudes (the expansion or weighting factors) can be complex numbers is necessary for the mathematical formulation of the quantum-mechanical principle of superposition – necessary in the sense that such formulation must accord with experiment.[117] Dirac writes that the need for the allowing of complex-number expansion coefficients is evident in the photon split-beam and photon polarization examples, both of which are described in our account of the expansion postulate above: see (d) 'Expansion postulate'. These examples show, says Dirac, that 'from the superposition of two given states a *twofold* infinity of states may be obtained.'[118] In the photon polarization case, for example, 'there are just two independent states of polarization for the photon, which may be taken to be the states of linear polarization parallel and perpendicular to some fixed direction, and from the superposition of these two a twofold infinity of states of polarization can be obtained, the general one of which requires two parameters to describe it'.[119] Likewise, for the photon split-beam case, 'a twofold infinity of states of motion may be obtained, the general one of which is described by two parameters, which may be taken to be the ratio of the amplitudes of the two wave functions that are added together and their phase relationship'.[120] Suppose now that in the superposition equation $\psi = a_1\psi_1 + a_2\psi_2$, the coefficients a_1, a_2 were restricted to only real numbers. In that case there would be only a simple infinity of states obtainable from the superposition, since it is only the ratio of the coefficients that is important in determining the direction of the resultant vector ψ when ψ_1 and ψ_2 are given. The allowing of complex coefficients increases this to a twofold infinity.

> Our assumption of complex coefficients implies that in every case of superposition of two different given states, a twofold infinity of states may be obtained. The vectors ψ representing the states are complex vectors, there

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116 Feynman, Leighton & Sands, 1965, III-1, p. 6.

117 After Dirac 1935, p. 16.

118 Dirac 1935, p. 16.

119 Dirac 1935, p. 16. Dirac is referring to the fact that although the number of possible polarizations of a photon is infinite, the number of possible polarizations observable in a single measurement is two, namely spin up or spin down along the selected axis of the measurement.

120 Dirac 1935, pp. 16-17.

> being a twofold infinity of them with extremities on any given line in the vector space.[121]

In turn, the ability of an individual quantum system to be able to self-interfere and generally to exhibit a range of associated subtle, non-classical physical properties depends crucially on its being in a linear superposition of states in which the weighting factors are *complex*, and not mere ratios of probabilities. The superposed states and their complex-number weightings play a physical role.[122] That is why the complexity in quantum mechanics is more than just a device for solving algebraic equations, although of course *used* for that purpose.

The third thing to be said here about the significance of complex numbers in quantum theory is to do with the calculation of the transition probabilities between a quantum system's various superposed states. Upon measurement there is a transition of one of the superposed possibilities from the quantum to the classical level, whereby it becomes real. It is in this *transition*, as Penrose observes,[123] that complex numbers become *probabilities* of the kind familiar in quantum mechanics through having their moduli squared.

The moduli are squared (probabilities are obtained) by multiplying the quantum wave function by its *complex conjugate*. The result of doing so is always equal to the absolute square of the wave function. The absolute square of the wave function $\Psi(x,t)$ gives the probability density $P(x,t)$ of some particular state of the system being found, e.g. of an electron being found at the coordinate between x and dx, at time $t \pm dt$.

Recall that the complex conjugate of a complex wave function is obtained by reversing the sign of i in it. Any complex wave function, such as $\Psi(x,t)$ may be written

$$\Psi(x,t) = \mathrm{R}(x,t) + i\mathrm{I}(x,t),$$

where $\mathrm{R}(x,t)$ and $i\mathrm{I}(x,t)$ are its respective real and imaginary parts, in this case representing $r(\cos\theta)$ and $r(i \sin\theta)$, respectively. The complex conjugate of $\Psi(x,t)$ is then defined as

$$\Psi^*(x,t) \equiv \mathrm{R}(x,t) - i\mathrm{I}(x,t).$$

The two are multiplied together, giving

$$\begin{aligned}\Psi^*\Psi &= (\mathrm{R} - i\mathrm{I})(\mathrm{R} + i\mathrm{I}) \\ &= \mathrm{R}^2 - i^2\mathrm{I}^2\end{aligned}$$

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121 Dirac 1935, p. 17.

122 See also Bohm & Hiley 1993, p. 22.

123 Penrose 1994, p. 264.

$$= R^2 + I^2$$

(since $i^2 = -1$). Thus

$$\Psi^*(x,t)\Psi(x,t) = [R(x,t)]^2 + [I(x,t)]^2,$$

which is the sum of the squares of two real functions. Hence $\Psi^*(x,t)\Psi(x,t)$ must be real, and non-negative.[124]

The procedure for obtaining the probabilities by multiplying the wave function by its complex conjugate has the character of a *deus ex machina*. It is postulated – and it works.

The fourth and final thing to say, related to the second and the third, is that the complexity in Schrödinger's equation was forced upon Schrödinger. His equation is complex because it relates a first time derivative to a second space derivative, which is necessary because the equation is based on the energy equation which relates the first power of total energy to the second power of momentum.[125] It turned out that it is just not possible in the non-relativistic theory to have other than an equation that is of first order with respect to time and a complex wave function.[126] This requirement was a surprise to Schrödinger.

It is true that we can always represent the complex wave function of quantum mechanics as a pair of real (i.e. non-complex) functions. Let's for example write for the electron wave function $\Psi = R + iI$. Inserting into Schrödinger's equation gives

$$\frac{\partial R}{\partial t} = -\frac{\hbar}{2m}\frac{\partial^2 I}{\partial x^2} \quad \text{and} \quad \frac{\partial I}{\partial t} = \frac{\hbar}{2m}\frac{\partial^2 R}{\partial x^2}.$$

We see that R and I are a pair of real functions. But neither of them alone is a solution of Schrödinger's equation. Hence it is essential to have both R and I. Both contribute to physical results, as is evident from the definition of the probability, $P = \Psi^*\Psi = R^2 + I^2$.

Since R and I are coupled in the above expressions, we cannot just discard one of the two real solutions, as we do in classical mechanics where i is simply a mathematical shorthand or computational aid to avoid having to do trigonometry, and only the *real* function gives the behaviour of things in the real world. It simply is not possible to set up an acceptable theory using only a single real wave function.[127] Therefore quantum mechanics is complex in a way in which classical mechanics never is.

Even so, it is always possible to maintain that the complexity in the formal-

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124 After Eisberg & Resnick 1974, p. 148.
125 Eisberg & Resnick 1974, p. 147.
126 For a discussion, see Bohm 1951, pp. 84-8.
127 After Bohm 1951, pp. 84-5.

ism of the quantum wave function is of no particular significance because its use of complex number is nothing but a shorthand notation for representing two real wave functions. There is, however, a price to pay for downplaying the significance of the mathematical formalism of quantum mechanics and trying to keep things as 'classical' as possible at least in *that* regard ('classical' in the sense of classical physics – more closely in accord with our everyday experience and ordinary intuitions). The two functions may now be real (though coupled) and in that sense 'classical', but the general quantum state itself – the state of linear superposition of the various possibilities described by the coupled functions – remains utterly mysterious and *non*-classical, as does the collapse of the wave function when an observation is made. We have a pair of nice, real wave functions (albeit coupled) – and everything else is like magic. To say that there is no particular significance, requiring analysis, to the presence of complexity in the quantum wave function on the above grounds is like saying that there is no particular significance to the expansion and measurement postulates of quantum mechanics – or indeed, to wave-particle duality – on the grounds that even though the general state of the system is postulated to be the sum of all its possible complex states, yet when a measurement is made, the wave function always collapses to yield a real variable.

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