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REVIEW ARTICLE

The View from No-when[1]

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1 Introduction

In Philip K. Dick's *Counter-Clock World* the direction of time flips in 1986, putting the Earth into what its inhabitants call the 'Hogarth Phase'. Named after the scientist who predicted that 'time's arrow' would change direction, the Hogarth Phase is a period in which entropy decreases instead of increases. During this time the dead call from their graves to be excavated, people clean their lungs by 'smoking' stubs that grow into mature cigarettes, coffee separates from cream, and so on. Although such reversals may be familiar from works of fiction, we are utterly unfamiliar with them in experience. The processes of nature behave in a temporally asymmetric manner. Once the cream mixes with the coffee, it stays that way, never to return to its original separated state. Neither cigarettes nor people ever fully reconstitute themselves. This kind of asymmetric behaviour is ubiquitous in thermodynamic, radiative, and quantum mechanical phenomena. But we also find our common-sense impression of the world painted with temporally asymmetric concepts. Time feels like it is 'flowing' forward and causation appears to be an asymmetric relation.

These phenomena and impressions stand in sharp contrast with the world as perceived through eyes tutored by modern science. Spacetime, rather than

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[1] Review of Huw Price [1996]: *Time's Arrow and Archimedes' Point: New Directions for the Physics of Time*, Oxford, Oxford University Press.

space and time, becomes the fundamental entity. Time is an unchanging dimension on a four-dimensional manifold. More importantly, the fundamental laws of nature appear to be time-reversal invariant (TRI). This means, loosely, that the laws of nature are indifferent between the past and future directions of time. It is not contrary to the laws of physics that the milk separates from coffee or for the air in a room to concentrate spontaneously in a small corner.

Temporally asymmetric phenomena reach deeply into our conception of ourselves and the world. It is not surprising, therefore, that we have a hard time cleanly separating the world suggested by ordinary experience from the world suggested by science. In *Time's Arrow and Archimedes' Point* (henceforth *Time's Arrow*) Huw Price seeks to remedy this situation by disentangling these two images of the world. What does the world look like when it is shorn of the effects of our temporally asymmetric prejudices? In answering this question Price takes the reader through an entertaining tour of the temporal asymmetries of thermodynamics, contemporary cosmology, radiation, causation, and quantum mechanics (QM). In each case he sifts the asymmetries that are human in origin from those existing independently of us. A recurring theme is that many of us, even our best scientists, have failed to come fully to grips with the time symmetry suggested by physics.

Time's Arrow is an exceptionally readable and entertaining book. Intended for readers without physics or philosophy backgrounds, it successfully proves that a book can be both popular *and* philosophically sophisticated. The liveliness of the writing, the interesting topics, and the lack of philosophical and scientific jargon make it mostly attractive to the lay reader, though I suspect she may lose her patience somewhere in the thicket of counterfactuals in Chapter 7. Nevertheless, Price does not pull any philosophical punches; the chapters on causation are quite demanding, for instance.

Time's Arrow is also a highly original and important contribution to the philosophy and physics of time. It is path-breaking in many areas, since it covers topics rarely treated by philosophers and offers novel solutions to many problems: among them, discussions of time asymmetry in contemporary cosmology, a new interpretation of the radiation asymmetry, a sophisticated variant of the 'agency' theory of causal asymmetry, and a proposal that advanced action is at the heart of the quantum mysteries. Those interested in the philosophy and physics of time owe Price a debt of gratitude for (re)introducing these topics into the field. Throughout *Time's Arrow* Price unfailingly exposes the peculiar kind of double-thinking that characterizes much of the literature on time's arrow.

A mark of a good book is that it opens up discussion. With an eye towards sparking some discussion of the above issues, I offer a critical commentary on some of Price's arguments. The plan is as follows. After introducing in Section 2 the problem of the direction of time, I present in Section 3 Price's 'basic dilemma' for those seeking to answer it. I examine a potential loophole in this dilemma provided by S. Hawking's quantum cosmology and Price's criticism of it. Much of the talk about time's arrow in quantum cosmology, I will argue, is riddled with confusion. I then briefly discuss the possibility that we live in a time-symmetric 'Gold universe', one much like that described by Dick in *Counter-Clock World.* Section **4** presents an objection to Price's *a priori* argument for time symmetry. This argument does a lot of work throughout *Time's Arrow*, and yet it seems to me to be very shaky. Section **5** turns to an question Price doesn't discuss (but perhaps should have), whether time's arrow really requires an explanation, and Section **6** scrutinizes his discussion of correlations in the microscopic realm. Section **7** investigates his ambitious project regarding an advanced action interpretation of QM. Finally, Section **8** looks into the asymmetries of causation and counterfactual dependence.

2 Boltzmann, time's arrow, and the Past Hypothesis

The problem of the direction of time has its source in the debates over the status of the second law of thermodynamics between L. Boltzmann and some of his contemporaries, notably J. Loschmidt, E. Zermelo, and E. Culverwell. Chapter 2 of *Time's Arrow* sketches what Price takes to be the main lessons of these debates. Although he does not delve into debates in the foundations of statistical mechanics, the account of entropy increase he offers is clearly Boltzmann's. Many philosophers and scientists still do not appreciate Boltzmann's subtle reasoning, so I think it worthwhile to spend a moment repeating what may be a familiar story.[2]

Boltzmann hoped to discover the mechanical underpinning of the second law of thermodynamics. He devised a particularly simple explanation for why systems tend toward equilibrium. Consider a macroscopic system, say, an isolated gas of N particles in a box. For the sake of familiarity we will work with classical mechanics, although this is not essential. We can characterize the gas by the coordinates and momenta x_{iN}, p_{iN} of each of its particles and represent the whole system by a point $X = (q, p)$ in a 6N-dimensional phase space known as Γ, where $q = (q_1 \dots q_{3N})$ and $p = (p_1 \dots p_{3N})$.

It was Boltzmann's great insight to see that the thermodynamic entropy 'reduced' to the volume in Γ picked out by the macroscopic parameters of the system. Let's see how this works. The key ingredient is partitioning Γ into compartments, such that all of the microstates X in a compartment are macroscopically indistinguishable, i.e. they share the same thermodynamic features. To each macrostate M, there corresponds a volume of Γ-space $|\Gamma_M|$

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[2] For excellent presentations of Boltzmann's views, see J. Lebowitz [1993] and J. Bricmont [1996]. Evidence of the confusion still surrounding Boltzmann's theory can be found in the replies to Lebowitz [1993] in the November 1994 issue of *Physics Today*.

whose size will depend on the macrostate in question. Simply for combinatorial reasons, it turns out that almost all of Γ corresponds to a state of thermal equilibrium. The reason for this is simply that there are many more ways to be distributed with uniform temperature and pressure than ways to be distributed with nonuniform temperature and pressure. Consequently, there exists a vast asymmetry in Γ between the states in thermal equilibrium and the states in thermal nonequilibrium. We can now introduce Boltzmann's famous entropy formula (up to an additive constant):

$$S_B(M(X)) = k \log |\Gamma_M|$$

where $|\Gamma_M|$ is the volume in Γ associated with the macrostate of interest, and k is Boltzmann's constant. S_B can be thought of as providing a relative measure of the amount of Γ corresponding to each M. Given the above asymmetry in Γ, almost all microstates are such that their entropy value is overwhelmingly likely to increase with time. When the constraints are released on systems initially confined to small sections of Γ, typical systems will evolve into larger compartments. Since the new equilibrium distribution occupies *almost all* of the newly available phase space, nearly all of the microstates originating in the smaller volume will tend toward equilibrium. Except for those incredibly rare microstates conspiring to stay in small compartments, microstates will evolve in such a way as to have S_B increase. Though substantial questions can be raised about the details of this approach, and philosophers can rightly worry about the justification of the standard probability measure on Γ, this explanation seems to offer the correct *framework* for understanding why the entropy of systems tends to increase with time. About it, Price makes the important point that it is not entropy *increase* that needs to be explained. If one accepts the Boltzmann account, one sees that 'thermodynamic equilibrium is a natural condition of matter' (p. 39). Unfortunately this point is lost on many contemporary philosophers and physicists who fail to appreciate Boltzmann's theory.

Loschmidt and Zermelo launched well-known objections to Boltzmann's notorious H-theorem. But an objection in their spirit can also be advanced against Boltzmann's later view sketched above. Loosely put, because the classical equations of motion are time-reversal invariant (TRI) (see Callender [1995]), nothing in the original explanation necessarily referred to the direction of time. Though I just stated the Boltzmannian account of entropy increase in terms of entropy increasing into the future, the explanation can be turned around and made for the past temporal direction as well. Given a gas in a box that is in a nonequilibrium state, the vast majority of microstates that are *antecedents* of the dynamical evolution leading to the present macrostate correspond to a macrostate with *higher entropy* than the present one. Therefore, not only is it highly likely that typical microstates corresponding to a nonequilibrium state will evolve to higher entropy states, but it is also highly likely that they *evolved from* higher entropy states. Obviously this is simply not what we experience. However we choose to use the terms 'earlier' and 'later', clearly entropy doesn't increase in both temporal directions.

Without changing the TRI laws of nature, there is no way to eliminate this problem as a matter of nomic necessity. One must solve it instead by appealing to temporally asymmetric boundary conditions. That this is necessary was seen by Boltzmann, as well as many of this century's greatest scientists, e.g. A. Einstein, R. Feynman, E. Schrödinger. By assuming that what we call the earlier states of the universe are of comparatively low entropy with respect to what we call the later states, we remove the unwelcome consequence of the TRI of the laws. Earlier states do not have higher entropy than present states because we make the cosmological posit that the universe began in an extremely tiny section of its available phase space. David Albert calls this assumption the *Past Hypothesis* (p.c.), and since it's convenient terminology, I will follow suit.

3 Why isn't the world a big cold soup?

After making the point that entropy increase is not surprising in itself, Price directs the reader to what he considers the proper source of puzzlement. Given that equilibrium is the 'natural' state of matter, why did the universe ever exist outside of equilibrium? In the terminology of Section 2, the claim is that the Past Hypothesis needs an explanation.

Intuitively this certainly seems right. An early cosmological state whose entropy is as low as required is almost unimaginably improbable (according to the standard Lebesgue measure in statistical mechanics). Moreover, the Past Hypothesis gives a rather unsatisfying explanation of the thermodynamic asymmetries. Gases everywhere expand throughout their available volumes. We seem to desire a local causal explanation of why this happens and we're answered with a global consistency constraint. It is as if potatoes everywhere suddenly started looking like Richard Nixon and this were explained to us with the claim that the initial conditions of the universe must have been compatible with this. Dissatisfaction with this kind of answer leads many to seek to explain the Past Hypothesis. We'll discuss whether they *ought* to seek an explanation shortly.

Perhaps the principal claim of *Time's Arrow* is that such a search is doomed to failure if the laws are TRI, unless one is willing to posit a so-called 'Gold universe' (explained in a moment). Price's argument is strikingly simple. To explain the entropy gradient between the boundary conditions we need to account for the relative specialness of the initial conditions. Doing so means supplying some convincing reason to believe that those initial conditions are not unnatural or unexpected. Suppose we say entropy values vary directly with the size of the radius of the universe. (How or why we say this is unimportant for present purposes.) Hence entropy is predicted to be low at the Big Bang and higher now. Suppose further that we live in the familiar, positive-curvature Friedmann spacetime. After exploding in a Big Bang the universe expands until reaching a maximum radius and then recontracts until its annihilation in a Big Crunch. Clearly, if we are to be consistent, we should agree that entropy should decrease between the time of maximum radius and the Big Crunch. This is (in barest outline) what the cosmologist Thomas Gold believes. Believing that entropy values covary with the size of the universe, Gold asserts that at the maximum radius the thermodynamic arrow will 'flip' due to the recontraction.

For such a theorist not to posit a Gold universe in this spacetime would be to commit what Price calls a 'temporal double standard'. Claiming that entropy is positively correlated with the radius of the universe and yet not applying this to both the Big Bang and Big Crunch is inconsistent. Assuming we live in a universe with TRI fundamental laws,[3] there simply is no objective distinction between 'initial' and 'final' states, and thus, there is no way to consistently apply this explanation of the Past Hypothesis solely to the Big Bang. The laws of physics do not discriminate between the Big Crunch and Big Bang, and so we should treat both evenly. Obviously the particular details of the explanation of the Past Hypothesis don't affect the logic of the situation, and so what holds for the 'expansion of the universe' explanation holds for all other explanations too. Without the objective distinction between 'initial' and 'final' states provided by non-TRI laws of nature, there is no way to consistently explain the Past Hypothesis without also explaining the 'Future Hypothesis'. We thus meet Price's *basic dilemma*: either we explain the Past Hypothesis Gold-style or it is inexplicable by time-symmetric physics (p. 82). Because there is no net asymmetry in a Gold universe, we might paraphrase Price's conclusion in a more disturbing manner as the claim that the thermodynamic arrow is explicable just in case there isn't one.

Surprisingly, Price never asks whether the Past Hypothesis *ought* to be explained. Yet since explaining the Past Hypothesis is tantamount to trying to explain a boundary condition of the universe—and trying to do that has justifiably had a bad rap since Hume and Kant—I think it is a question we must

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[3] Quantum field theory and the so-called CPT theorem suggest that neutral kaon decay is not TRI. Because this departure from TRI is so slight and so puzzling, Price proposes to leave it to one side throughout the book. One might object that laws of nature do not have their symmetries in degree: a little bit of non-TRI makes the laws non-TRI. Though this is certainly true, I would not recommend an uncritical acceptance of the non-TRI of neutral kaons. One wants some assurance that the concept of TRI used in the CPT theorem is the same as that used in the present context, for instance. Since this topic and others have not been investigated, Price's strategy is perhaps not unreasonable.

seriously consider. First, however, let me say a word about two possible ways of explaining the arrow and about Price's *a priori* argument for time symmetry.

3.1 Hawking's No Boundary proposal and time's arrow

Much ink has been spilt on what S. Hawking and J. Hartle's 'No Boundary' proposal in quantum cosmology entails about the direction of time. Hawking [1994] announces that his original claim about this implication constituted his 'greatest mistake.' This admission, originally made in 1985, attracted attention and sparked a debate about whether Hawking is right, or whether perhaps his greatest mistake instead lay in calling his earlier claim his greatest mistake, and so on. After taking on Hawking in the pages of *Nature*, Price devotes part of a chapter to Hawking's alleged loophole (discussed below) in the above basic dilemma.

While this debate has been entertaining from a sociological perspective, I doubt that it yields new philosophical insights. The reason is simple: quantum cosmology is too immature a discipline to imply much that can be transferred reliably to the philosophical problem of the direction of time. Philosophers should not be studying the implications of proposals in quantum cosmology, assuming the proposals are right; rather they should be studying the presuppositions of the proposals themselves. Price may think that one can leave that to the experts and still profitably discuss the implications, if any, of Hawking's work for time's arrow. But I don't think it's possible to excise the implications from the general programme in such a neat way, for reasons to be discussed shortly.

Quantum cosmology, as opposed to quantum gravity, takes as its *raison d'être* the goal of explaining the boundary conditions of the universe. Hawking writes, 'we shall not have a complete model of the universe until we can say more about the boundary conditions than that they must be whatever would produce what we observe,' ([1987], p. 163). Hawking's goal is for the boundary conditions themselves to emerge as the result of nomic necessity. Quantum cosmology contrasts sharply with the more modest project of quantum gravity—the project of devising a theory reconciling the apparent conflict between general relativity and QM. The goal of merely having a consistent fundamental theory drives the latter, whereas the philosophically questionable idea of explaining away boundary conditions drives the former.

The object of study in quantum cosmology is the 'wavefunction of the universe'. This is a functional over spatial three-metrics that is governed by the Wheeler–DeWitt equation, the so-called 'Schrödinger equation for the universe'. Hawking and Hartle's No Boundary proposal, which I haven't space to describe adequately here, can be thought of as a boundary condition for this equation. The wave functional is expressed as a path integral in which the sum is over Euclidean geometries. The 'no boundary' part of the proposal is the restriction of this sum to compact manifolds with only one boundary (so, yes, perhaps 'One Boundary' proposal is a better name). Hawking thinks his model informs us that the universe 'began' in a Euclidean de-Sitter regime from which the current Lorentzian regime 'emerged'.

Natural questions arise about exactly what this talk of 'emerging' means, given that time doesn't exist in the Euclidean regime. One might also want to ask what possible justification there could be for the choice of path integral. There are mathematical problems too: the above path integral is not bounded from below, and thus may not converge, so it is not clear that this proposal is even mathematically well defined.

Recall that Price's dilemma is that either we explain the Past Hypothesis Gold-style or we cannot explain it with time-symmetric physics. Hawking's loophole to this dilemma is supposed to be as follows: the equation governing the wavefunctional of the universe is TRI, yet it describes an ensemble of time-asymmetric individual solutions. Loosely speaking, the idea is that the wave-function is a sum of low-to-high entropy universes and high-to-low entropy universes, each type predicted with the same probability. Price illustrates this idea with an example of a factory that produces equal numbers of left-handed and right-handed corkscrews. Hawking originally thought that entropy would decrease as the radius of the universe decreased, and therefore, that entropy would be low at both the Big Bang and Big Crunch. Further investigation suggests that this is not the case, that entropy would increase (or decrease) without ever turning around. Repairing the 'great mistake' centred on the observation that Hawking's entropy is not solely dependent on the radius of the universe.

Price believes the entropy in Hawking's model depends on size of the universe alone (pp. 92–3), and has charged Hawking with putting the asymmetry in by hand. Yet Hawking [1994] explicitly denies that the entropy depends only on the size, which seems to undercut Price's objection. There are some physicists who hold that Hawking's mistake was not a mistake. This turns on a technical dispute over how one should understand the No Boundary proposal, a point I am not competent to adjudicate (see Zeh [1994] and the subsequent questions by Hawking). In any case, the complaint made by the physicists against Hawking is not the same as the one made by Price.

Whether Price emerges the winner of this debate is not my main concern. My worry is that Hawking's theory may not have *anything to do* with Price's dilemma. By ignoring the details of Hawking's project, Price hasn't realized how alien it is to his own project. One way to see this is to reconsider Price's corkscrew-factory analogy. This analogy is not a good one. In three spatial dimensions, there really is a difference between right-handed and left-handed corkscrews, i.e. they are enantiomorphs. But as Price has cogently argued, yet seems temporarily to have forgotten, from the atemporal standpoint there is *no* *genuine difference* between low-to-high and high-to-low universes. They are the same universe differently described. But if *all* the solutions are asymmetric *in the same way* (say, right-handed) then the equation can't be a symmetric one.[4] Price cannot make sense of what he claims is Hawking's 'loophole' without resolving this puzzle. This requires delving into the details of Hawking's theory. Once there, the philosopher will find himself in a strange new land, one brimming with questions that need answering before contemplating time's arrow.

The answer to the puzzle of understanding Hawking's alleged loophole is that there is an equivocation on 'time symmetric' in the statement of the loophole. The Wheeler–DeWitt equation is supposed to be a time-symmetric equation with temporally asymmetric solutions describing with equal probability high-to-low-entropy universes and low-to-high-entropy univefses. Notoriously, however, this equation has *no* time variable in it.[5] In what sense is it time symmetric, then? Hawking *defines* an operation for the equation, *calls* it time reversal, and shows that the equation is invariant with respect to this operation. Hawking splits the three-metric into what is known as a 'conformal three-metric' and the trace of the extrinsic curvature of the three-space, K. The time-reversal operation takes the complex conjugate of the wavefunctional and changes the sign of K. Hawking takes K to be an internal time parameter; that is, he essentially (and controversially) *defines time as K*. The Wheeler–DeWitt equation is time symmetric, therefore, in the sense of being K-symmetric. But the solutions to this equation (more precisely, the WKB solutions in the semi-classical oscillatory region of superspace) are time symmetric with respect to a different 'time'. In the semiclassical region time 'appears' through a procedure involving a WKB approximation, and this time is not defined everywhere K is. For every solution in this region describing matter modes evolving according to the Schrödinger equation, there is another describing these modes evolving to the time-reverse of the Schrödinger equation. It is only by equivocating that we can have two different asymmetric universes being solutions to a symmetric equation. Once this is recognized, Hawking's model appears to have little relevance for Price's problem.

Another major difficulty with transferring what Hawking says to the philosophical domain arises over the definition of entropy. Hawking thinks of entropy in terms of gravitational perturbations in the WKB solutions, high-amplitude perturbations being high entropy and low-amplitude perturbations low entropy. What philosophers need before they can say anything sensible about Hawking and time's arrow is some confidence that there is a connection

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[4] Contrast with Maxwell's equations, which are TRI but have asymmetric solutions, the retarded and advanced ones. Because they describe processes evolving *in* time, these two solutions really are different, unlike 'retarded' and 'advanced' universes.

[5] This is the origin of the so-called 'problem of time' in quantum gravity; see Callender and Weingard [1996] and references therein.

between Hawking's definition of entropy and the Boltzmann entropy discussed above. To my knowledge, there does not yet exist anything but a heuristic notion of gravitational entropy, much less a precise link between it and the statistical mechanical entropy.

As one can see, the relevance of Hawking's model to the problem of the direction of time is obscure at best. By ignoring the details of Hawking's work Price assumes that 'time symmetric' and 'entropy' have the same referents as usual in philosophical contexts. He also assumes that the model has a coherent physical interpretation and that it is mathematically well defined. Given that all of these assumptions are very controversial, surely it is premature to investigate what quantum cosmology suggests about time's arrow. We haven't even satisfactorily worked out what thermodynamics looks like in a relativistic world, much less a quantum cosmological one! Before philosophers explore what the No Boundary proposal implies about time's arrow, or scientific realism, or the mind-body problem, therefore, I suggest they begin answering more modest questions. The following seems like a good start: Is time plausibly 'reduced' to the trace of the extrinsic curvature (or some similar quantity)? Is the recovery of time in the WKB regime a sensible procedure? How is the measurement problem solved in quantum cosmology? How should we interpret the universal wavefunction and probabilistic predictions from it? Ought we pursue quantum cosmology in the first place?

3.2 Gold universes

If we're going to explain time's arrow, according to Price, we must do so in a consistent, time-symmetric way. If our explanation works for one end of the universe, then if there is another similar end in the future our explanation had better apply there too. We must be prepared to accept that we may live in a Gold universe, a universe wherein time's arrow 'flips' as it does in the Hogarth Phase of Dick's story.

By the time the reader of *Time's Arrow* gets to the discussion of the Gold universe (p. 100), Price has equipped her with the skills necessary to disarm many objections to the possibility of such a universe. It is no objection to point out that the Hogarth Phase would be dominated by seemingly unlikely events—cigarettes reconstituting themselves and heat flowing from cold to hot. In a thought experiment Price uses successfully throughout the book, he asks us to view our region of the universe from the equally legitimate 'reverse' temporal perspective. From that standpoint it is *our* region of the universe that is dominated by improbable events. Since neither perspective is objectively distinguished, we know that the Hogarth Phase is no more unlikely than the anti-Hogarth Phase.

After defending the coherency of the Gold universe from this and other objections, Price raises a number of fascinating issues. For instance, it seems that there must exist advanced effects in any observation of the Hogarth Phase. If we aim a telescope at a region of the sky suffering a Hogarth reversal, and we place a black plate at the back of the telescope, then from the point of view of the astronomer in the Hogarth Phase it will look like the black plate is absorbing radiation. By contrast, from our point of view absorption looks like emission, and the black plate should appear to cool just prior to being pointed at a Gold universe.

In this section I wish to point out that there *may* be reasons for thinking that we do not live in a Gold universe that *do not* rely on temporally asymmetric prejudices. We can divide Gold universes into two types, relaxed and unrelaxed. Relaxed Gold universes are ones wherein the universe lasts a long time compared to the relaxation time for the processes in it. Described from one temporal perspective, they begin in a very low entropy state and gradually relax to equilibrium. There they undergo transitions between equilibrium states for a while, until entropy gradually starts to decrease. As time goes on, entropy decreases more and more until a final, low-entropy Big Crunch. Unrelaxed Gold universes, by contrast, have relaxation times that are long compared to the age of the universe. These universes start with low entropy, begin to relax, but then start to 'feel' the final low-entropy constraint. That is, consistency with the fact that they must eventually evolve into a low-entropy condition forces them to make less and less probable transitions. (If they had time to relax to equilibrium they could make the most probable transitions all the way to the point of maximum radius.) Entropy will occasionally decrease before the point of maximum radius, just as from the perspective of the Hogarth Phase, entropy will start to decrease so that it may eventually return to our initial low-entropy condition. In unrelaxed universes, consequently, there exists a kind of statistical 'pre-effect' of the time-symmetric final boundary condition. Simple statistical models of time-symmetric boundary conditions bear this out (see Cocke [1967]). In principle, this statistical pre-effect should be observable, giving us the opportunity to determine whether or not we live in an unrelaxed Gold universe.[6]

Since we haven't noticed any such statistical pre-effect, it seems we do not live in an unrelaxed Gold universe of a certain size. That fact of course doesn't imply that we don't live in a Gold universe. We might live in a relaxed one, or a very long-lasting unrelaxed one. If we are allowed some freedom in the expected age of the universe and its relaxation time, then we might suppose the universe is very long-lasting and the relaxation of the processes in it takes

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[6] A. Kent [1997] has recently shown that we could use this statistical pre-effect to send superluminal signals. If we can detect *any* regularities in the final state of the universe then in principle we may be able to send superluminal signals. Indeed, many wild possibilities open up in a Gold universe (or at least one with a time-reversed galaxy in it); for a great science fiction story involving one such possibility, see G. Egan [1995].

no more than slightly over half its duration. Then the possibility that we inhabit a Gold universe is a live one, right?

Wrong. The problem is that there are some processes in nature which (like my neighbour's dog) never relax. The future light cone is effectively transparent to light (and gravitational radiation). Except for the relatively rare occasions when light will meet other bodies, light can travel unabsorbed from the present epoch through to the Hogarth Phase. Time symmetry implies that light emanating from the Big Crunch can travel unabsorbed throughout our present epoch. Using this reasoning, D. Craig [1996] has recently predicted that 'no matter how long our universe will live, the time symmetry of the universe implies that the extragalactic background radiation be at least twice that due to the galaxies to our past'. He then argues that if this is right the time symmetry of the universe is a property directly accessible to experiment. Moreover, he claims that current measurements of the background radiation make it *unlikely* that we live in a Gold universe.

It would be premature to take Craig's results as definitely eliminating the possibility that we live in a Gold universe. Craig's argument seems to presuppose that both ends of a Gold universe have the same, or nearly the same, state, whereas we need only assume they are both low-entropy states. Nevertheless, his paper does show us that the loophole of supposing the universe very long-lived in order to 'save' the possibility that we inhabit a Gold universe may not always be available to us.

4 Price's *a priori* argument for time symmetry

From Boltzmann's debates with Loschmidt, Zermelo, and Culverwell we learn that we cannot derive temporally asymmetric behaviour directly from TRI laws of nature. No asymmetry in, no asymmetry out. Price sees his basic dilemma as an analogue of this conclusion. The Past Hypothesis is manifestly time asymmetric. If we wish to propose new TRI laws of nature to explain this, then consistency demands that they predict low entropy states at both ends of the universe. Thus the dilemma: either a Gold universe or no explanation from time-symmetric laws.

Quite so. But why assume that the explanation must come from TRI laws? In the Boltzmann case the context demands this—we're trying to explain how thermodynamic behaviour might arise in a world governed by the TRI classical equations of motion. When trying to explain the Past Hypothesis (assuming there is a need to) we're inevitably entertaining *new* laws of nature. That the *old* laws of nature weren't sufficient is the reason why we needed the Hypothesis in the first place. Given that we're now talking about new laws, why assume they must be TRI? Why restrict ourselves to Gold cosmologies?

We see hints of Price's answer in his discussion of R. Penrose's [1988]

'Weyl curvature hypothesis'. Penrose's thesis is that our universe's boundary conditions are governed by a time-asymmetric law. Penrose conjectures that if the conformal spacetime structure is smooth, then this will bring about the low-entropy state necessary for thermodynamics. He takes it as a new fundamental law of nature that *past* Big-Bang-type singularities have vanishing Weyl tensors (if the Weyl tensor is zero or very small compared to the Ricci tensor then spacetime will have a 'smooth' structure). As H. D. Zeh [1989] has remarked, this is essentially a gravitational version of the Sommerfield radiation condition. The law is unabashedly non-TRI:

> The difference between these latter types of singularity [black holes and the Big Crunch] and the singularity of the big bang is that they are *future* singularities . . . whereas the big bang was a past singularity . . . . The rules for past singularities must be different from those which apply at future singularities (Penrose [1988], p. 322).

Past and future directions are regarded as physically significant properties to which Penrose's law is sensitive (this is just what it means for a law to be non-TRI). One consequence of this is that, on Penrose's proposal, a Gold universe is physically impossible. The universe cannot return to a low entropy state simply because the Big Crunch is a *future* singularity.

When reviewing this proposal, Price claims that Penrose had better have 'some good reasons' for rejecting the Gold view and accepting a temporally asymmetric hypothesis (p. 94). Penrose could accept just as easily the time-symmetric law stating that the Weyl curvature approaches zero toward the universe's extremities, Price notes. This would explain the low entropy associated with what we call the Big Bang and also predict that the Big Crunch is a low-entropy state. Low entropy would not be 'triggered' by past singularities, but merely by singularities in general. Notice that this 'symmetrized' proposal would also be testable in ways that Penrose's proposal could not be. To verify the original claim we would need to examine *past* white holes other than the Big Bang to see whether they were associated with low entropy. Since there do not seem to be any, Penrose's proposal is safe in the waters of unfalsifiability, at least until we get to the Big Crunch. But on Price's version, black holes we meet *in the future* should also be associated with low entropy, and thus they should be associated with *anti-thermodynamic* behaviour. Preceding any singularity there should be a period of heat going from cold to hot, spherical waves converging on single points, perhaps a few dead coming back to life, etc. Penrose is wrong to think, as he does, that this thermodynamic behaviour toward the past is any less probable than thermodynamic behaviour toward the future. Nevertheless I don't see any particularly compelling reason why he should be forced in Price's direction. Why should Penrose alter the content of his theory to accommodate a TRI law instead of a non-TRI law?

Here for the first time we meet Price's *a priori* argument for time symmetry. Like the chemist G. N. Lewis who declared 'the symmetry of time' a fundamental law of nature ([1930], p. 570), Price thinks it is a serious advantage of a fundamental law that it not introduce time asymmetry (p. 94). This *a priori* preference for TRI laws runs throughout the book. For example, much later it is the basis of his 'temporal asymmetry objection' against standard quantum theory (pp. 206–9). Quantum measurements are time-asymmetric according to the standard interpretation of QM. If we make a position measurement on the superposition $|x_1\rangle + |x_2\rangle$ and collapse the state to (say) $|x_2\rangle$ at t, then for times after t, but not before t, the state $|x_2\rangle$ evolves according to the Schrödinger equation. Price asks what justification there is for this asymmetry (actually, for a similar one involving photon polarization). He believes that the asymmetry is neither of thermodynamic nor of anthropocentric origin and views this as a genuine difficulty. I cannot find the problem, however. Asymmetry need not be either thermodynamic or anthropocentric. It could be nomological instead—indeed this is precisely what we find in the quantum-mechanical case. Here the temporal asymmetry is simply the well-known asymmetry of the Projection Postulate in the orthodox interpretation of QM. According to this interpretation we must regard the Projection Postulate as a law of nature, just as the collapse rules are nomological in other interpretations, e.g. the spontaneous reduction interpretation known as GRW (see Albert [1997] for discussion and references). According to other interpretations (for instance Bohm's theory), the asymmetry turns out to be, like the thermodynamic one, dependent upon special initial conditions (see Arntzenius [1997]). Without the *a priori* prejudice against non-TRI laws, this objection, like the previous one against Penrose, falters. Is this prejudice epistemically justified?

Price seems to think proposals for new laws of nature are more likely to be true if TRI than if non-TRI. This is a strong claim, and unfortunately it receives no real defence in the book. Many, including myself, prefer symmetry and thus TRI laws. But I know of no general way to elevate this aesthetic rationale into an epistemic one. Why prefer a new fundamental TRI law to a new fundamental non-TRI law? In a reply to Callender [1997b], Price writes, 'I do think there is an epistemic imbalance between symmetric and asymmetric rivals in science. Roughly speaking, a symmetric theory is always preferable, *ceteris paribus*. (I acknowledge that it is a difficult issue what justifies this assumption, but it seems to be an important part of the general preference for simplicity in scientific theories)' (Price [1997], p. 81). I'm not sure a convincing argument for this is forthcoming, however. Symmetry should not be mistaken for simplicity, first of all. The simplest theory may well be non-TRI. Secondly, wouldn't any non-empirical argument preferring a particular nomic symmetry be unpleasantly similar to the old arguments that spacetime must be Euclidean?

5 Do we need to explain the Past Hypothesis?

As I mentioned in Section 1, it is surprising that Price doesn't consider the question of whether the Past Hypothesis ought to be explained. Since explaining the Past Hypothesis is effectively explaining the boundary conditions of our universe, it seems a philosophically respectable position to maintain that the Past Hypothesis doesn't need explanation. Certainly the idea of providing one deserves comment.

What are the motivations for trying to explain the Past Hypothesis? The main one is that the Hypothesis commits us to an initial state of the universe that is monumentally improbable. That such an unlikely state of affairs exists seems to demand explanation. Another reason, already discussed, may be that in some sense the Hypothesis doesn't explain thermodynamic behaviour. Why don't the milk and coffee ever display anti- or a-thermodynamic behaviour instead? The Past Hypothesis essentially answers that this is a consequence of a feature of the initial conditions, and this doesn't seem very satisfying to some.

The problem seems isomorphic to many others facing contemporary science. To account for the (nearly) isotropic microwave radiation the standard cosmological model must assume that spacelike related sections of the universe began in the same temperature. It must also assume an extraordinarily special value for the ratio of the energy density of the universe to its critical density if the universe is to look even remotely as it does. In particle physics it is a well-known complaint that the standard model requires eighteen undetermined parameters to describe the phenomena adequately. The problem in all these cases is that, according to some 'natural' measure on the space of all the models of the theory of interest, the space of solutions giving rise to a universe like ours is tremendously small. Universes like ours seem horribly unlikely. It is not obvious that this provides a philosophically respectable reason for demanding a new theory. But there is no doubt that many scientists see it as providing such a reason. As a result, we have the advent of the inflationary scenario in cosmology and GUTs and superstrings in particle physics. In each of these cases scientists hope to show that the required special parameters are really a natural product of some new dynamics.

The question of whether we are epistemically obliged to seek new theories when our present ones require special parameters is a deep one. Consider two fundamental theories T_1 and T_2, both empirically adequate. T_1 contains models that represent the actual world, but these models are very atypical in the space of all the models of the theory. T_2, however, is such that most of its models describe worlds that may represent the actual world. T_1 requires very special initial conditions to describe the actual world, while T_2 does not. *Ceteris paribus*, is T_2 more likely to be true than T_1?

As I have discussed elsewhere (Callender [1997b]), the answer depends on one's broad stance in the philosophy of science. Empiricists who think the primary goal of scientific inquiry is empirical adequacy will not find any *epistemic* reason to prefer T_2 to T_1 if both are empirically adequate. Here the models used to describe the phenomena are what counts. Whether one chooses to pick out the class of relevant models with laws alone or with laws plus boundary conditions does not matter. Some may even view this as merely a difference in language. Scientific realists, by contrast, are not constrained solely by empirical adequacy in their search to find epistemic reasons to prefer a theory. Consequently, they may have reasons to prefer T_2 to T_1. It might be the case, for instance, that some of the properties important to realists *track* dynamical explanations. That is, given a choice between dynamical and non-dynamical explanations of a phenomenon, it may be that the dynamical explanations tend to be (e.g.) more unifying or simple than the non-dynamical explanations. In such a case scientific realists will have epistemic cause to prefer dynamical explanations of the Past Hypothesis to non-dynamical ones. For these complex reasons and others, I believe it is far from obvious that the Past Hypothesis demands explanation.[7]

In addition, it is not clear that the sort of explanation Price seeks for the Past Hypothesis is the kind that generally appeals to scientists. When physicists posit gravitational inflation or string theory, for instance, they are offering new dynamics according to which (they hope) the special parameters are no longer special. The analogue of these moves in the present case is to do something most would consider extreme: look for a new dynamics to replace classical mechanics or QM.[8] According to these new dynamics the Past Hypothesis would not turn out to be special or needed. But Price is looking at *non-dynamical* explanations like Penrose's hypothesis and '*super-dynamical*' ones like those offered by quantum cosmology. Further investigation is required before we can hold that these explanations for the Past Hypothesis have the same justificatory status as dynamical explanations eliminating the need for special parameters.

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[7] One related issue I haven't space to discuss is the question of whether the Past Hypothesis should be treated as a law of nature. If we ought to consider it lawlike, then this would seem to shelve the demand for its explanation. As I have pointed out in Callender [1997c], certainly it seems like it *would* emerge as lawlike according to the so-called Ramsey-Lewis 'best system' theory of lawhood (see e.g. D. Lewis [1986]). According to this theory, the laws of nature are the axioms of those true deductive systems with the greatest balance of simplicity and strength. Since the Past Hypothesis conveys an awful lot of information about the world with minimum specification, it seems likely that it would emerge from the 'best-system competition' as a law of nature.

[8] The new dynamics posited by the GRW interpretation of QM has the consequence of eliminating special initial conditions (Albert [1997]). It does not eliminate the need for the Past Hypothesis, but it does imply that microstates underlying macrostates need not be 'typical' (in the sense described in Section 2). In this case the new dynamics enters as an interpretation of QM rather than a replacement of it.

6 The principle of microscopic innocence

Chapter 5 sets the stage for much of the subsequent argumentation in *Time's Arrow*. In it, Price claims that there are conflicting intuitions in contemporary physics. The conflict is supposed to exist between the alleged TRI of the basic laws of physics and an idea called the *principle of microscopic innocence* (following Price, henceforth 'μInnocence'). μInnocence states that interacting particles exhibit postinteractive but not preinteractive correlations (in a sense independent of the thermodynamic asymmetry). Particles impinging on one another are 'innocent' of what will happen, whereas postinteractive particles are tainted with the 'knowledge' of interaction. μInnocence is said to enjoy 'almost unchallenged status' in physics (p. 122).

However, as I've said before (Callender [1997b]), I don't think physics is guilty of μInnocence. Anyone who understands what the TRI and determinism of classical physics means, for instance, will know that there are as many preinteractive correlations as postinteractive correlations between particles in the state space of the interacting system. If physicists think to the contrary—and I doubt many would—they are simply wrong. If they believe in μInnocence due to QM, and they hold the Projection Postulate or some other non-TRI law, then μInnocence is true and we cannot fault them. So where is the conflict? μInnocence is either true due to the existence of non-TRI basic laws or it is false due to the TRI of the laws. People may like to believe in TRI laws and μInnocence, but that does not reveal any confusion within physics.

Now it is true that some physicists have offered principles whose truth would imply the truth of μInnocence. Most well known is perhaps O. Penrose and I. C. Percival's [1962] 'law of conditional independence', which holds that influences from different directions of space are uncorrelated. But I suspect that the number of physicists who subscribe to this is low. Furthermore, it is worth noting that Penrose and Percival advertise their principle *as a law* and assert it in the context of search for 'a fundamental asymmetric law' (p. 609). If we treat it as such then we can understand them as proposing a new non-TRI law, thereby warranting their belief in μInnocence.

Still, I agree with Price that there is a temporal bias operating in physics, though we disagree on the nature of the bias. I think it is a natural reaction to a very remarkable feature of the world. The bias is that physics usually describes the behaviour of systems in terms of laws and *initial* conditions, not laws and final conditions or laws and both initial and final conditions. That physicists may operate pragmatically with a temporal bias is therefore not surprising, given the remarkable fact that laws plus *initial* conditions *alone* have sufficed throughout history to describe matter in motion. No doubt we devise theories this way partly because we are prisoners of temporal asymmetry (the knowledge asymmetry is particularly relevant). Nevertheless the fact remains that nature could have been less kind to us. She might have made the world such that we need to specify both initial and final conditions for science to get anywhere. The temporal bias in physics is thus not solely a result of the fact that physicists are human beings wearing temporally asymmetric blinders; it is also in part due to the way the world is.[9] In Section **8** I will describe a similar disagreement. Indeed, if I'm right, we might view the present point as a symptom of a general tendency Price has to 'over-subjectivize' the temporal asymmetries in the world.

7 Advanced action quantum mechanics

In Chapters 8 and 9 Price launches what is perhaps the most ambitious part of the book, the case for advanced action within QM. What would QM look like if the founding fathers of the theory had adopted the atemporal viewpoint advocated in *Time's Arrow*? At the level of motivation, I have much sympathy with this project. Reflecting upon the persistent conflation of epistemology and metaphysics in the standard Copenhagen interpretation, one naturally wonders how much of QM is of anthropocentric origin.

The main burden of Chapter 8 is to explain Bell's Theorem and its consequences and to describe some of the interpretational issues that arise. Price divides the interpretations of QM into two kinds, the complete- and incomplete-description views. The complete view holds that the quantum formalism offers a description of the world that is representationally complete. The incomplete view maintains that there is more in the world than is represented by the quantum formalism. As is well known, QM predicts that there will be irreducible statistical correlations between the results of measurements of some systems that have interacted and then separated, e.g. between two spin-1/2 particles in the singlet spin state. Bell's theorem suggests that there can be no local hidden-variable explanation of these correlations. Here Price makes the important point that Bell's Theorem affects both types of interpretation of QM equally. It is not a 'blow' to hidden-variable theories any more than it is to advocates of the complete-description view. The complete-description view requires wave-function collapses, which in order to satisfy Bell's Theorem must occur instantaneously (or nearly instantaneously) over spacelike distances. Likewise, proponents of the incomplete-description view who add 'hidden' variables to QM need to make their 'beables' (in Bell's terminology) influence each other over spacelike distances. Given Price's view that there is an epistemic imbalance

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[9] Price agrees that the way the world is plays a role in the creation of the temporal bias. That we wear temporally asymmetric 'blinders' has an objective basis in the various physical asymmetries. What I am pointing out is that the bias is not solely the result of us wearing these blinders; someone not wearing these blinders might be biased in favour of initial conditions (rather than final, or initial and final conditions) due to the success we have had theorizing with them.

between TRI and non-TRI theories, he seems to prefer the incomplete views because they may be TRI. Because all of the interpretations seem committed to some kind of nonlocality, and nonlocality appears to conflict with relativity, Price is unhappy with any current interpretation of QM.

Chapter 9 brings together all the strands running throughout the book. The proof of Bell's Theorem requires a temporally asymmetric assumption known as 'Independence': the hidden variables at the time the two systems interact are independent of later measurement settings. In other words, Bell assumes *later* measurement settings do not affect the values of *earlier* measurement results. This is to assume there is no backward causation in QM. Yet once we are freed from believing in μInnocence, Price maintains, there is no reason to hold this or to affirm Independence. If we allow the results of measurements to depend on later measurement settings, then it is in principle easy to reproduce the Bell phenomena with a local theory. This point is 'an old idea on the fringes of quantum mechanics' (p. 241).

In its defence, with Price's help, we can point out that a temporally nonlocal theory seems no worse than a spatially nonlocal one. From the Archimedean perspective backward causation is no more troubling conceptually than forward causation. And once we abandon action by contact, what does it matter whether the influences are spacelike or timelike related to each other? Furthermore, because the influences are timelike related to one another Price's theory holds out the hope of an easy coexistence with relativity. Except for the Many Minds interpretation, backwards causation QM (BCQM) looks like the only choice for a local realistic model of QM. Since such a model is the holy grail of contemporary foundations of physics, BCQM seems a worthwhile place for further investigation.

It is puzzling that Price doesn't leave the reader with this modest but fair claim. Instead he brings out the *a priori* considerations of Chapters 5 and 7 in favour of BCQM: 'it restores to microphysics a temporal symmetry which we have no good reason to doubt in the first place' (p. 251); it is the theory one '*ought to have expected*' if one rejected μInnocence (p. 124); 'the puzzling consequences of superposition in QM are the sort of phenomena *we might have expected,* if we had already given advanced action the consideration that, in hindsight, it seems to have long deserved' (p. 257, emphasis mine). As opposed to the other interpretations, BCQM meets with the approval of his *a priori* argument for temporal symmetry. Once again, one wonders what possible support there could be for such an argument. If it has to do with simplicity, as Price suggests, how does he know BCQM is simpler than alternatives, *when there doesn't even exist a coherent model of BCQM*? (We'll return to this point.) Also, it is worth pointing out that both Bohm's theory and the Many Worlds interpretations seem to be counterexamples to Price's above claims, as they are both perfectly TRI and deterministic. That is, either they are counterexamples or (because they are as time-symmetric as Newtonian physics) Price ought to hold that people should have been suspicious of Newtonian physics because it is not time-symmetric enough.

Price complains about the lack of interest in BCQM and chalks this up to ignorance and our temporally asymmetric prejudices. But surely *part* of the explanation of the lack of interest in BCQM is that no remotely plausible model incorporating backwards causation exists. Y. Aharonov, L. Vaidman, and others have recently developed the so-called time-symmetric formalism of QM into the 'two-state' formulation of QM (see Vaidman [1996]). However, it is probably best to think of this formulation as an extension of the standard formalism rather than as a new interpretation. The only really developed theory that implements backwards causation is the 'transactional interpretation' due to J. Cramer [1986], and it is far from satisfactory (see T. Maudlin [1994] for a devastating critique). So there simply does not exist a plausible model of BCQM. For this reason Price's suggestion that BCQM is superior to existing theories is off the mark. Price has mistaken an interpretation *schema* for an interpretation. Who knows how plausible BCQM will be if someone can develop it?[10]

Part of the problem, I think, stems from a possible confusion about the foundational problems of QM. Price groups the problems of nonlocality and measurement together in a peculiar way. For instance, he understands μInnocence to give rise to *all* of the puzzles commonly associated with QM, such as nonlocality, indeterminacy, etc. (p. 126). Retraction of that principle is viewed as a general panacea for all the ills of a quantum world. This is mistaken. The problem of nonlocality concerns the implications of Bell's Theorem and the problem of indeterminacy the implications of the measurement problem. Rejecting μInnocence doesn't help us see that we can solve the measurement problem.

The measurement problem consists 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.

If we grant all three of these propositions then systems in microscopic superpositions will (when measured) balloon into macroscopic superpositions of the sort made famous by Schrödinger's cat.

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[10] Price seems to waver on whether he thinks he is offering a genuine interpretation of QM or not. In fn. 8, Ch. 9, he sounds like he is not; in fn. 22 he sounds like he is. That his 'interpretation' doesn't yield answers to the questions I ask later suggests that fn. 8 is closer to the truth.

Contrary to what Price suggests, rejecting μInnocence doesn't solve this problem. Nor does rejecting μInnocence open the door to the rejection of any of these propositions, since the denial of each of them has been well explored. Price obviously wants to deny proposition 1). The rejection of μInnocence, or my alternative characterization of the temporal bias, shows those denying 1) that the hidden variables might lie in the future light cone of the particles. If the rejection of the temporal bias opens the door to anything, it is to spatially *local* hidden-variable theories, not to hidden-variable theories in general.

As I said, BCQM is a promising schema, but it is not a full-blown interpretation of QM. For it to reach that status, those denying 1) must supplement the QM formalism with an ontology and with some plausible physical laws describing how this ontology behaves. This is a highly non-trivial task, requiring that one devise a 'natural-looking' theory that reproduces the phenomena described and predicted by QM. No one has yet done it for BCQM, and many questions about such a theory arise. Three that immediately strike me are the following. First, it appears to be impossible to use the nonlocal correlations of QM to send superluminal signals, i.e., there is no Bell telephone. Why is this so according to BCQM? Second, the nonlocal connections enforce the conservation of spin and other quantities between the two wings of a Bell-type experiment. Without this nonlocal connection, why is spin conserved according to BCQM? Are these conservation rules derivable from BCQM, are they imposed by the adoption of extremely special initial conditions, or must we take them as basic? If the last, should we still consider the theory local? Third (and this is related to the first), what are the probabilities in QM *probabilities of* in BCQM?

8 Are the asymmetries of dependence in us or in the world?

Widespread dissatisfaction with the Tories caused the Labour landslide of 1997; the landslide didn't cause the dissatisfaction. In this case and in an indefinite number of similar ones we meet our strong intuition about the asymmetry of causation. Effects never, or hardly ever, precede their causes. Note that this asymmetry presupposes a further asymmetry, that the cause-effect relation is itself asymmetric. If we couldn't already distinguish causes from effects it wouldn't make sense to ask about the direction of causal transmission. What explains these two asymmetries? Hume famously tied the two asymmetries together by linguistic convention. What distinguishes causes and effects is that the term 'cause' ('effect') refers to the earlier (later) member of a pair of causally related events. So of course causes precede their effects. As Price points out, the difficulties with this view are that it seems both too weak and too strong. It is too weak because it makes it merely a linguistic matter that we can affect the future but not the past. It seems too strong because it makes backwards causation logically impossible. In Chapters 6 and 7 Price advances a sophisticated version of Hume's conventionalism and tries to answer these two objections.

Price answers the first problem by 'beefing up' the conventionalist strategy. He presents a theory of causal asymmetry that grounds the asymmetry in our perspective as agents. The key to explaining the above two asymmetries lay in the fact that deliberation seems to be essential to being an agent in the world. Arguably, deliberation is an intrinsically asymmetric process; moreover, we deliberate about what to do in the future and not the past. This asymmetry seeps into our view of the world and causes us to see the world through a temporally asymmetric 'lens'. The asymmetry of causation is a projection of this internal asymmetry upon dependencies that we find in the world. Here Price does an admirable job of persuading people (like myself) who are, perhaps by temperament, reluctant to place the asymmetry of causation 'in us' rather than 'in the world'.

The discussion then turns to counterfactual dependence, rather than simply causal dependence. Price explains the asymmetry of counterfactual dependence, the fact that the future seems to depend on the past but not vice versa, in terms of the asymmetry of agency. We think that counterfactuals like

(1) If a huge asteroid had not struck the Yucatan region 65 million years ago, then the dinosaurs would not have disappeared so quickly

are true, but counterfactuals like

(2) If the dinosaurs did not disappear so quickly, then a huge asteroid would not have struck the Yucatan region 65 million years ago

seem false. In judging the truth of counterfactuals we seem to use an asymmetric principle. We hold fixed the course of events *prior* to the time mentioned in the antecedent and consider the subsequent course of events. This is why (1) but not (2) seems correct. Price then argues that this 'hold the past fixed' convention follows from our role as deliberating agents.

How do we answer the second problem for conventionalism? As Price agrees, many philosophers have argued convincingly that backwards causation is at least logically, perhaps physically, possible. It seems wrong to rule this out by fiat. In response Price develops a loophole available to him thanks to M. Dummett's [1964] classic discussion of backwards causation. Dummett notes that the 'bilking arguments' against backwards causation can be circumvented when knowledge of the earlier effect is not accessible to the agent when the cause is to be 'bilked'. Dummett asks us to assess counterfactuals not with the 'hold the past fixed' convention, but instead with 'hold fixed what we have epistemic access to at the time of the antecedent condition'. Because we know much more about the past than the future, usually the two conventions will agree in their judgements. That is, the past and what we have epistemic access to will largely coincide. But in cases where we are ignorant about the past Dummett's convention will allow the past to depend upon the future. The state of a photon from the sun impinging on my sunglasses may depend on the 'setting' of my sunglasses. This is how Price escapes the second problem for conventionalism. By using Dummett's convention instead of the 'hold the past fixed' criterion, we free dependence from a definitional tie to the past. No longer need the conventionalist rule out backwards dependence as a logical impossibility.

While I sympathize with the conventionalist strategy, I would like to express a reservation about this loophole. I agree that the knowledge asymmetry certainly plays a role in the explanation of the asymmetry of counterfactual dependence (and perhaps vice versa). However, I'm sceptical that it is as crucial as Price needs in order to exploit this loophole. Adopting Dummett's convention makes the asymmetry of dependence wholly a matter of the information available to an agent at a time. This doesn't seem right. My ignorance about regions of the past doesn't incline me to think that such regions are 'open' like the future is 'open'. The fact that I don't know the state of an incoming photon only implies that I can't run a bilking argument against the supposition that my subsequent measurement of the photon affects its earlier state. But the fact that I *can't prove* there is no backwards causation happening doesn't mean that I think *there is* backwards causation occurring. I'm completely ignorant about vast periods of history, but I don't think anything I do now can affect any of it. I don't know anything about these periods, nor do I deliberate about them in the same way I deliberate about how to 'change' the future. Dependence just doesn't seem to track knowledge in the right way to exploit this loophole. If this is right, and closes the loophole, then the conventionalist still has some work to do.

Price makes a strong case for thinking that agents in a world like ours will project their 'internal' asymmetry on to the world. On this view the asymmetry of dependence has subjective origins (although Price accounts for these, as above, with some objective facts). Isn't that only half the story? I admit that I still hanker after a more objective account of the asymmetry of dependence. I don't have the full story, but it seems to me that we do not explain the asymmetry of counterfactual dependence merely by the fact that our asymmetric world produces agents who colour the world in asymmetric ways. Thermodynamic behaviour not only shapes asymmetric agents but also provides an objective asymmetry in the world. Perhaps it provides the resources for claiming that there is a sense in which the future objectively depends on the past in a way the past doesn't depend upon the future. The thermodynamic asymmetry implies, for example, that the future behaviour of macroscopic systems is extraordinarily stable against perturbations whereas their past behaviour is unstable against perturbations. Obviously I cannot develop this idea here, but the thought is that this kind of objective asymmetry is part of the explanation of why we believe the future is mutable and the past is not—the future *is* mutable in a sense the past is not thanks to the low-entropy initial condition. Given the close connection between the arrow of mutability and the asymmetry of counterfactual dependence, it is natural to suspect that the latter asymmetry has objective as well as subjective roots.

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