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Measures, Explanations and the Past: Should 'Special' Initial Conditions be Explained?

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ABSTRACT

For the generalizations of thermodynamics to obtain, it appears that a very 'special' initial condition of the universe is required. Is this initial condition itself in need of explanation? I argue that it is not. In so doing, I offer a framework in which to think about 'special' initial conditions in all areas of science, though I concentrate on the case of thermodynamics. I urge the view that it is not always a serious mark against a theory that it must posit an 'improbable' initial condition.

1 Introduction

Your hand accidentally makes contact with a hot pan on a stove. Immediately you flinch, because you know what is going to happen: pain. You have internalized the generalization that heat always flows from hot to cold and not vice versa. But why does this regularity obtain? After all, fundamental physics appears to tell us that it is perfectly possible for heat to flow from cold to hot. And why, for that matter, do gases always expand throughout their available volumes despite the mechanical possibility of them not doing so?

After much controversy in physics and philosophy, the broad answer to these questions—and similar ones about any other process governed by the second law of thermodynamics—is more or less standard. It has two steps.

First, according to combinatorial arguments made famous by Boltzmann, the microstate evolving toward macroscopic equilibrium is more likely, according to the microcanonical probability distribution, than the microstate evolving away from equilibrium. Hence, it is more likely for temperatures to be uniform throughout the joint system than not, and thus more likely for heat to flow from hot to cold than from cold to hot.

Notoriously, a lacuna in this argument demands a second step. The Boltzmannian explanation works in both temporal directions. Neither the combinatorial arguments nor the laws of physics introduce a temporal asymmetry, so on this theory, entropy, which is maximized at equilibrium, would increase toward the future *and* the past, contrary to the observed facts. The second step stipulates a solution to this problem by positing a cosmological hypothesis that breaks the symmetry; namely, that in the distant past the global macrostate posited by cosmology is one of very low entropy and that the microstate actually occupied was 'typical' for that macrostate.[1] How low would the entropy have to be? *Really* low: low enough to make thermodynamic generalizations applicable for the roughly 15 billion years we think these generalizations held. Call this hypothesis, suggested by Boltzmann and adopted by Schrödinger, Feynman and others, the 'Past Hypothesis', and call the cosmological state it posits the 'Past State'. If the Past Hypothesis is true, then the most probable history of the universe is one wherein entropy rises. Entropy has risen throughout history because it started off very low and yet it is likely to rise.

Huw Price ([1996]) argues that when one appreciates the above situation the appropriate question to ask in foundations of statistical mechanics is no longer 'why does entropy rise?' but rather 'why was it ever low to begin with'? In Callender ([1998]), I asked whether we could expect an answer to this question, for it is asking for an explanation of a boundary condition of the universe—and we should not explain those, I said. Sklar ([1993], pp. 311–8) voices similar worries. Price disagrees, criticizing this position as either 'perilously close to a kind of global explanatory nihilism' or as committing a 'temporal double standard' ([2002], p. 115).

Although I like the nihilist appellation, my position is at best a local form of explanatory nihilism—and one that doesn't commit double standards. In what follows, I clarify my claim that one ought not to explain the boundary conditions of the universe, show how it escapes Price's criticisms, and then try to

[1] Let me not pretend that there is not controversy over this Boltzmannian explanation. See Callender ([1999]); Goldstein ([2001]); Sklar ([1993]) for an entry into the literature. If you are worried after reading the work of Jos Uffink that the second law doesn't really govern these processes, don't worry: these processes still exist and are lawlike, however we characterize them (as perhaps the 'minus first' law in Brown and Uffink [2001]). Albert ([2000]) coined the phrase 'Past Hypothesis'.

turn the tables on Price. In so doing, I will offer a framework in which to think about 'special' initial conditions in all areas of science. My paper, therefore, will touch on issues in scientific methodology, explanation, and laws of nature. My hope is that these comments will help address the frequent claim that it is a serious mark against a theory that it must posit an 'improbable' initial condition.

2 Price's objection

My objection to the idea of explaining boundary conditions originates in classic arguments in Hume, on which I will expand in Section **5**. Briefly, Hume taught us, for reasons anyone familiar with Hume can reproduce, that we ought to be skeptical of any grand principle dictating how initial conditions are distributed. To this kind of point Price responds:

> However, it seems to me that this attitude to the explanation of initial conditions is on shaky ground. Would it take the same view of the need to explain an equivalent condition at any other time? If so, it is perilously close to a kind of global explanatory nihilism, which answers every 'Why?' question with the answer that things had to be some way, so why not this way? If not, on the other hand, then the proponent of this 'no need to explain initial conditions' view needs to tell us what is special about (what we call) initial conditions.
>
> The threat here is a temporal double standard—an unjustified discrimination on the basis of temporal location or orientation. ([2002], p. 114)

He then describes the threat with a vivid example:

> Suppose, for a moment, that in the past thirty years or so, physics had discovered that the matter in the universe is collapsing toward a Big Crunch, fifteen billion years or so in our future and that as it does so, something very, very extraordinary is happening [. . .]. Somehow, by some unimaginably intricate balancing act, the various forces are balancing out, so that by the time of the Big Crunch, matter will have spread itself out with great uniformity. A butterfly—nay, a molecule—out of place, and the whole house of cards would surely collapse.
>
> As a combination of significance and sheer improbability [. . .] this discovery would surely trump anything else ever discovered by physics [. . .]. If this discovery did not call for explanation, then what conceivable discovery ever would?
>
> In my view, however, this state of affairs is *exactly* what physics has discovered [but just described with unusual temporal conventions]. ([2002], pp. 114–5)

Price may be correct that the Humean position outlined above, left unadorned, threatens a global explanatory nihilism. It suggests that no probability metric can be put over initial conditions at all, since we do not know how often any given initial condition would occur if the world were 'rerun'. Sklar describes this complaint as questioning the

> very propriety of attributing 'probabilities' to these initial or overall conditions of the world at all. Attributions of probability [. . .] depend upon observed relative frequencies in the world [. . .] from which probabilities are inferred. To talk about probability of a universe is, from this point of view, incoherent. ([1993], p. 313)

Though I am sympathetic with this reaction, for present purposes I do not want to rest my claim on it.[2] Maybe there is some way of understanding the Boltzmannian story without an *initial* probability distribution. If so, all to the better. Until then, however, I want to help myself to the standard probability metric in statistical mechanics. The Boltzmannian needs the Lebesque measure projected onto the hypersurface in phase space corresponding to the energy of the system. Probabilities devised from this measure and from conditionalizing on the Past State provide the predictive components of statistical mechanics. Without this, or something close to it, the whole Boltzmannian story collapses. I do not want to be a global explanatory nihilist: some of the events that happen are more likely (according to this distribution) than some of the other events that happen.

3 What we want explained

Something *is* special about initial conditions and final conditions: they are the boundary of spacetime.[3] For initial conditions, this means that there are no conditions before them; for final conditions, there are no conditions after them. *If* explaining initial or final conditions entails describing what happened at a state that doesn't exist, this strikes me as a big problem. But need explaining these conditions do so?

Some might argue that initial conditions are explanatorily special for the following reason. Good scientific explanations, at least most of them, are causal explanations. Causation is typically temporally asymmetric, for usually causes precede their effects. Therefore, final conditions can be explained because there are possible causes that precede final conditions, but initial conditions cannot be explained because there are no times that precede them. I do *not* want to make this argument, or any variant of it. To do so would be to commit Price's temporal double standard. It is entirely possible, assuming the

2 Besides, there do exist some empiricist-friendly ways of understanding these probabilities (see Loewer [2001] and North [2002]).

3 Of course, there may not be any initial conditions or final conditions in our spacetime. But since the laws allow that the past or future *could* be finite, it turns out not to harm our discussion if we pretend they are. For simplicity, I will assume that the universes we discuss admit a global time function and can be foliated via equivalence classes of embedded space-like hypersurfaces without boundary.

laws of nature are time-reversal invariant, to suppose that our universe be closed, have a final time, and that a 'Future Hypothesis' be true: that is, that the final macrostate of the universe be one of extraordinarily low entropy—say, the same entropy as the initial macrostate—and the microstate 'typical' for that macrostate. Fifteen billion years of backward history from the 'Future Hypothesis' might look exactly like our first fifteen billion years in reverse. Creatures at that end of the universe would presumably typically explain events in terms that we would call 'later' states rather than 'earlier' states. Yet according to the position I just described, these stories they tell would not count as explanations. I do not want to be temporally prejudiced with the concept of explanation. Even if the concept we use is sensitive to temporal direction, I do not want the argument to rely on temporally-biased features of our language. If explanation is causal explanation, then let us use a concept of causation without a built-in time preference (there are plenty on offer).

One can imagine various scenarios in which an explanatory temporal bias might be justified. For instance, the laws of nature might be time-reversal non-invariant. In such a case, we would have an objective, not merely linguistic, time asymmetry to which explanations might be sensitive. If the laws govern only present-to-future transitions of state, it would be natural for explanations to follow suit (see North [2002]). The bias might be entirely justified because it would be the world, not the philosopher, imposing it. In any case, for the problem I want to consider here I will assume that the laws are time-reversal invariant. (On the question of what that means and whether they really are, see my [2000]).

Since I do not want to commit a temporal double standard, I want to treat initial and final conditions equally. To my claim against explaining initial conditions, I should and do admit that we also should not try to explain final conditions of the universe. However, the issue is not really about initial conditions versus final conditions, nor even about boundary conditions, whether initial, final, or in-between. Let me explain.

We can explain the final state of the universe (i.e., why it is what it is) in terms of the state just before. For instance, if the world were classical, we might write down all the positions and momenta of matter on a spatial hypersurface before the final state, let it evolve in time until the final state, and this would explain why that state is what it is. But we can also explain the initial state of the universe in terms of a state after it, if the laws are time-reversal invariant. For instance, classically, we might write down all the positions and momenta of matter on a spatial hypersurface after the initial state, let it evolve backward in time until the initial state, and this would explain why that state is what it is. Backward explanation, it seems to me, is perfectly possible. For the same sorts of reason, we can explain the boundary of a singularity that is not a past or future boundary singularity in the same way: evolving microstates forward or backward to the boundary to show that it is compatible with what 'came out'.

Price and others are after something more than this kind of explanation. When people want the Past State explained, they are not asking merely for a precise specification of a spatial hypersurface in time and a calculation showing that its backward time evolution leads to a microstate realizing a low entropy macrostate. All hands agree that that is at least in principle possible. Rather, the feature that cries out for explanation is that the Past State is a state that is incredibly improbable according to the standard measure on phase space. Penrose ([1987], p. 344) estimates that the probability of this particular type of past state occurring is 1 out of $10^{10^{123}}$. Kiessling ([2001]) estimates that it is infinitely improbable! I have some reservations about both calculations, especially Penrose's (which uses the Beckenstein-Hawking entropy, not the Boltzmann entropy, in its derivation). But clearly, however it is calculated using the standard measure, this initial state is going to be monstrously unlikely.

Can anything explain this unlikely state? Price sometimes says he wants 'some sort of lawlike narrowing of the space of possibilities, so that such a universe [one with a Past Hypothesis] no longer counts as abnormal' ([2002], p. 116). I am skeptical that one might do this while retaining Boltzmann's story; and more importantly, I am skeptical about the initial motivation to explain the Past State. Here it is interesting to note that scientists also appear to disagree about whether it should be explained. Boltzmann, for instance, writes that a low entropy past 'is a reasonable assumption to make, since it enables us to explain the facts of experience, and *one should not expect to be able to deduce it from anything more fundamental*' (quoted in Goldstein, [2001], p. 50; emphasis mine). By contrast, Kiessling and others think that it points to a 'need for a deeper postulate' ([2001), p. 86). As I will show momentarily, this tension within science and philosophy about explanation has echoes in many other areas of science. Before arguing against explaining the Past Hypothesis, I would like to compare and contrast our problem with similar issues that arise elsewhere.

4 A range of unlikely initial conditions

What does it mean to say that an initial condition is unlikely? In an intuitive sense, *everything* that happens is unlikely. In 1994 twins named Lorraine and Levinia Christmas, each driving their own car to deliver Christmas presents to the other, met in a head-on collision on Christmas Eve near Flitcham, England. Think of the odds! And think of how small the set of initial conditions (on the standard measure) are that lead to this. Do we feel the need to explain this fact? Probably not; it is a coincidence. Coincidences abound; but some of them we elevate to being genuinely improbable and some of those we deem needy of explanation.

The topic of interest is the striking correlations that exist among all thermal phenomena. All thermal phenomena seem to obey the same simple generalizations, in particular the second law of thermodynamics. The set of classical initial conditions needed for this to occur is astoundingly small on the standard measure. Apparently similar problems occur elsewhere; most notably, in standard Big Bang cosmology (that is, expanding Friedman solutions of the Einstein field equations). Both the horizon and the flatness problems, to name two, threaten to impose severe constraints on what initial data one can specify on an early spatial hypersurface. Take the horizon problem first. The temperature of the cosmic background radiation is uniform to at least one part in 10,000 in every direction astronomers look. But in a Friedman universe (which our universe seems to approximate), not all bodies could have been in causal commerce with each other (hence 'horizons'). Due to the curvature of spacetime, many regions of our night sky were most likely never in causal commerce with one another. How did they arrive at the same temperature? As with the Past Hypothesis, the problem is not that there is any inconsistency between the observed facts and the relevant laws of physics. The problem is instead that the set of conditions leading to the same temperature everywhere is perceived as exceedingly small with respect to some natural measure. So-called 'inflationary scenarios' are the responses to this problem in contemporary cosmology.

The flatness problem is similar. In a Friedman universe, departures from 'flatness' (zero curvature) should grow larger with time. By now, all sorts of initial minute irregularities should have been enlarged and be obvious. Since the universe presently appears to be roughly flat, it must have been even closer to flat much earlier. The model describes this feature as Ω currently being close to 1, where $\Omega - 1 = k/(\mathbf{a}^2H^2)$, H is the Hubble parameter, and **a** is the spatial scale factor, normalized so that the constant k can equal either -1, 0 or 1. But for Ω to now fall in the range $0.1 \leq \Omega \geq 10$ it is claimed that Ω at the Planck time must have been equal to 1 within 59 decimal places. As with the horizon problem, this initial condition is deemed monstrously unlikely. Are these cosmological problems the same as ours?[4]

Not yet. Unlike in the statistical mechanical case, there is considerable uncertainty regarding which probability measure is appropriate—and even whether there exist well-defined measures on the relevant spaces of cosmological histories. Evaluating the situation, Ellis ([1999], A61) writes:

> We have at present no fully satisfactory measure of the distance between two cosmological models [. . .] or of the probability of any particular model occurring in the space of all cosmologies. Without such a solid base,

[4] One can find a similar issue in the case of time travel, for it is well known that there are possibly severe constraints on what initial data one can put on a time slice in the presence of closed timelike curves. The philosophical literature speaks of the 'improbability' of there being enough banana peels around for would-be violators of consistency to step on.

> intuitive measures are often used [...] the results obtained are dependent on the variables chosen, and could be misleading—one can change them by changing the variables used or the associated assumptions. So if one wishes to talk about the probability of the universe or of specific cosmological models, as physicists wish to do, the proper foundation for those concepts is not yet in place.

If the flatness problem is a serious failing and we really mean 'unlikely', then we need a normalized probability measure to make sense of this complaint. The measure used to generate the trouble is one roughly flat about $\Omega = 1$ as time $t \rightarrow 0$. There are other measures, of course. Arguably some are even 'natural'. Evrard and Coles ([1995]) argue that a uniform measure is not consistent with Jaynes's maximum entropy recipe for constructing priors. After constructing such a measure, they argue that 'values of Ω not exactly equal to 1 are actually infinitely far from this value.' On their measure, we should not be surprised that $\Omega \approx 1$ at time t_o. We should only be surprised if $\Omega \neq 1$.

Do we have a flatness problem or not? That depends on how seriously we should take these measures and their associated probabilities. At one end of the spectrum, certain special initial conditions may be surprising, but no more improbable than any other condition. For instance, certain hands dealt to one in cards are surprising (and so result from a surprising initial condition of the pack), but are really no more improbable than any other hand of the same size (unless one uses a probability metric sensitive to color, type of card, etc.). Surprisingness is of course a psychological notion, and we do not ordinarily demand that science explain away surprising events. At the other end of the spectrum we have the microcanonical measure used in statistical mechanics, a measure that certainly is well-defined and used in successful predictions. In between we have these measures in cosmology. These measures may or may not be rigorously definable, and it is not clear that there are not rival 'natural' measures delivering very different results (measures that, at the very least, are not absolutely continuous with each other).

How should we react to these rival measures? It seems to me that if we are going to take a probability measure seriously, then it ought to at least be (1) well-defined, (2) 'natural' and (3) such that it 'pulls its weight' in science more than other measures. One can perhaps add other conditions as well, for instance that the measure of any set be invariant as it moves with the dynamical evolution. (1)–(3) are horribly vague, and in Section **6** I will provide a philosophically more satisfying way of thinking about them.

The rationale for (1) is that any probability we are to take seriously should at the very least make sense. Debates about likely versus unlikely initial conditions without a well-defined probability are just intuition-mongering. The rationale for (2) is that due to the infinity of 'gruesome' measures,

measures can in principle be cooked up to make anything unlikely. Not being able to solve the problem of induction, I can not define 'natural'; but I can mention that as cosmologists search for measures according to which inflation is generic, they are indeed looking for measures that are usually considered natural by all involved. Condition (3) arises from the idea that the probability measure should actually be one that is used by science, not one designed merely to create mischief. Given the looseness of 'naturalness', it does not seem implausible that mathematicians could devise all sorts of natural-measure functions over sets of initial conditions. Why treat them seriously? One reason—the only good one—is that they contribute to the explanatory and/or empirical success of the theory in some crucial way. In addition, if there are two or more measures that meet conditions (1) and (2), as we have seen in the cosmology case, then (3) can play the role of tie-breaker. If the measures that claim $\Omega = 1$ is unlikely become useful in testable inflationary scenarios, then they, but not the Evrard-Coles measure, would satisfy (1)–(3).

Even at this level of vagueness, it is interesting to apply our criteria to the present situation. As matters now stand (as I understand them), condition (1) is not satisfied in cosmology. Virtually all of the measures invented by cosmologists are problematic to define (see Earman and Mosterin [1999], Section 9, and Ellis [1999] for discussion). Some of those proposed are fairly natural, so with enough ingenuity natural and well-defined measures might be placed over certain classes of solutions to Einstein's field equations. Will they pull their weight in science? Though vague, I think we can see an important difference between these measures in cosmology and those in statistical mechanics. In the thermodynamical case, the standard probability distribution easily satisfies conditions (1)–(3), plus any number more of plausible conditions. The measure in statistical mechanics essentially grounds an entire science—thermodynamics. In cosmology, the measures are also supposed to ground a science, the science of cosmological inflation. Right now, however, there are many conflicting inflationary mechanisms. If some day a scheme of inflation emerged as empirically successful, then we would have grounds for distinguishing the measure it employs. Until that day, there appears to be a significant difference between the scientific need for the microcanonical measure in thermal physics and various measures used in inflationary cosmology.

5 Brute facts and explanation

Although the Past State is unlikely according to our best science, I urge that one not take this as too serious a mark against it. My argument, as suggested earlier, begins with Hume's reaction to the classic cosmological argument for the existence of God. In that argument, we assume that every effect in the universe must have a cause; otherwise there would be no 'sufficient reason' for the effect. But if every effect must have a cause, we have a dilemma: either there was an infinite chain of causes and effects or there was a first cause, the Uncaused Cause. Not believing an infinite chain of causation would be explanatory (for reasons that are not compelling now), the argument concludes that there was an Uncaused Cause. Similar arguments from motion yield an Unmoved Mover.

One reaction popular among students is to ask, as Hume did, what caused or moved God? This question raises a few more. Should we posit an infinite regress of gods, in keeping with the original explanatory demand? Or should we 'bend' the explanatory demand so that in the case of God he doesn't have to be caused by a separate distinct existence? But then, one thinks, if it is acceptable for something to cause itself or to selectively apply the explanatory demand, we have gone a step too far in positing God as the causer or mover of the universe. Just let the universe itself or the big bang be the 'first' mover or cause and be done with it.

Though the situation with the Past Hypothesis is more complicated, at root my objection begins with a similar point. What would explain the Past State? The most natural answer is an even lower entropy state just before the Past State. The natural 'tendency' of systems is to go to equilibrium, after all. The original low entropy Past State would naturally and probably evolve from an earlier and lower entropy state. But now that lower entropy state is even more unlikely than the original. Either we just keep going, explaining low entropy states in terms of lower ones ad infinitum, or we stop. And when we stop, should we posit a first 'Unlow Low' Entropy State (which is what Price sometimes sounds like he is after; see his [2002], Section 5)? No. We should just posit the original low entropy state and be done with it.

Are there different theoretical explanations of the Past State, ones not appealing to earlier low entropy states? Maybe, but here I am skeptical for reasons again enunciated by Hume. In his *Dialogues Concerning Natural Religion*, Hume has Philo argue:

> [T]he subject in which you [Cleanthes] are engaged exceeds all human reason and inquiry. Can you pretend to show any such similarity between the fabric of a house and the generation of a universe? Have you ever seen Nature in any situation as resembles the first arrangement of the elements? Have worlds ever been formed under your eye [. . .]? If [so] [. . .] then cite your experience and deliver your theory. (Hume [1980], p. 22)

His point is that since the cosmos happens only once, we cannot hope to gain knowledge of any regularities in how it is created. This, I take it, implies that we will not be able to defend any grand principle of how contingent matter-energy sources are distributed at the boundaries of the universe, for what justification would we ever have for such a principle?

There are at least two worries buried in this discussion. One is an empiricist worry about the justification one would have for any grand principle that would explain why the initial conditions are what they are. The second is a more general question about judging when certain basic facts need explanation or not. The Design argument assumes that some purported basic facts, such as the Big Bang, are facts in need of explanation, whereas other purported basic facts, such as God, are not. But what is the difference? Are some basic facts acceptable and others not? Is there a criterion that separates the facts that need explanation from those that do not? What makes the 'new' basic fact better than the old?

The two worries are often linked. Consider an old chestnut in the history and philosophy of science, namely the example of scientists rejecting Newton's gravitational theory because it posited an action-at-a-distance force. Such a force could not be basic because it was judged to be not explanatory. But a priori, why are non-local forces not explanatory and yet contact forces explanatory? This is the second objection above. Furthermore, note that believing Newton's action-at-a-distance problematic stimulated scientists to posit all manner of mechanisms that would restore contact forces. Not only were these efforts ultimately in vain, but many of these posits came at the price of their mechanisms not being independently testable. Thus enters the first objection.

I see the same problem in Price's claim that the Past State needs to be explained. What is it about the Past State that makes it needy of further explanation? Why can't it simply be a brute fact or the Past Hypothesis be a fundamental law? One answer might be to accept that the Past State plus laws are empirically adequate yet find fault with them for lacking some theoretical virtue or other. Empiricists—those who see empirical adequacy as the only criterion that really matters—will not like this, but others will. Which theoretical virtue is the Past State lacking? It is simple, potentially unifying with cosmology, and it has mountains of indirect evidence via our evidence for thermodynamics and whatever mechanics we are considering. But still, it is highly improbable. Though we can reasonably worry about what exactly it means to say that a state of the entire universe is improbable, we can postpone such worries here since that is not the source of Price's problem. Can the improbability of the state mean that it cannot be true or that it is needy of explanation? Well, the Past State can certainly be true; virtually everything that happens is unlikely. What about explanation? I do not think explanation and probability have such a tidy relationship. Lots of low-probability events occur and not all of them demand explanation. Low-probability events can even function as the explananda, not merely the explanans. For example, an asteroid strike in the Yucatan region might explain the death of the dinosaurs, even though (arguably) the prior probability of the asteroid strike is lower than that of the dinosaurs' extinction (see Lange [2002], p. 108). It is far from automatic that low-probability events all deserve explanation. Even if they did, it seems here that the many other theoretical and empirical virtues of the Past State trump any vice due to its improbability. (Furthermore, the sorts of explanations of the Past Hypothesis that Price envisions in his [1996] seem to me to be examples that are wildly speculative and not obviously more probable.)

My own view is that there is not some feature of facts that makes them potentially acceptably brute or self-explanatory, that makes some facts okay as brute and others as not. We instead look at the theoretical system as a whole and see how it fares empirically, and if there are ties between systems then we look to various theoretical virtues to decide (if one is a realist). What we do not want to do is posit substantive truths about the world a priori to meet some unmotivated explanatory demand—as Hegel did when he notoriously said there *must* be six planets in the solar system. In the words of John Worrall ([1996], p. 13):

> the worst of all possible worlds is one in which, by insisting that some feature of the universe cannot just be accepted as 'brute fact', we cover up our inability to achieve any deeper, testable description in some sort of pseudo-explanation—appealing without any independent warrant to alleged a priori considerations or to designers, creators and the rest. That way lies Hegel and therefore perdition.

Price and the scientists he mentions are of course free to devise alternative theoretical systems without the Past Hypothesis. So far there is not much on the table. And what is on the table doesn't look very explanatory, as we will see.

Again, there is a prominent echo of our debate in others areas of science. Consider, for example, the standard model in particle physics. Physicists routinely complain that the model contains too many fundamental parameters. Here is the physicist Sheldon Glashow:

> Although (or perhaps, because) the standard model works so well, today's particle physicists suffer a peculiar malaise. Imagine a television set with lots of knobs; for focus, brightness, tint, contrast, bass, treble, and so on. The show seems much the same whatever the adjustments, within a large range. The standard model is not like that. [. . .] The standard model has about 19 knobs. They are not really adjustable: they have been adjusted at the factory. Why they have their values are 19 of the most baffling meta-questions associated with particle physics. ([1999], p. 80).

Feeling that these 19 'knobs' are too ad hoc, physicists strive in many directions. Some seek to prove the standard model false in an experiment, others search for some 'meaningful pattern' among these parameters, and yet others devise theories such as superstring theory to deal with the problem. But why can't these 19 knobs be brute? Why can't, to take an example, the muon just be 200 times as heavy as the electron and that be that? Glashow even asks why is there a muon at all? But is *everything* to be explained? Will all models of the universe be deficient until physics answers why there is something rather than nothing?

It is perfectly within the physicist's rights to find something ugly about the standard model and to want to devise an alternative without so many knobs. When that alternative exists and is shown to be empirically adequate, we can then compare the two. It may well be that it is superior in various empirical (one would hope) *and* theoretical ways. But to know *beforehand*, as it were, that the existence of muons can not be brute seems to me too strong. Similarly, knowing beforehand that the Past Hypothesis needs explanation seems too strong.

I now want to turn to two tasks. First, I want to show that the Past Hypothesis operates as a fundamental law, at least according to one influential theory of lawhood. If one agrees it is a law, then it is particularly puzzling to me to insist that it demands explanation, as if laws wear on their sleeve their appropriateness as fundamental laws. Second, I then want to sketch why none of the ways I can imagine explaining the Past State really count as explaining the Past State.

6 The best-system analysis

From a certain perspective, the kinds of problems we have discussed in this paper seem almost inevitable—and their solutions do too. Think of the laws of nature the way an advocate of the Ramsey-Lewis 'Best System' does:

> Take all deductive systems whose theorems are true. Some are simpler better systematized than others. Some are stronger, more informative than others. These virtues compete: An uninformative system can be very simple, an unsystematized compendium of miscellaneous information can be very informative. The best system is the one that strikes as good a balance as truth will allow between simplicity and strength. How good a balance that is will depend on how kind nature is. A regularity is a law IFF it is a (contingent) theorem of the best system. (Lewis [1994], p. 478)

Roughly, the laws of nature are the axioms of those true deductive systems with the greatest balance of simplicity and strength. Imagine you are God the Programmer and you want to write a program for the universe. Merely listing every fact would make for a very long program. Simply writing 'anything goes', while making anything that happens compatible with the divine will, does not provide the kind of guidance we would expect from such a being. The laws of nature are roughly those lines you would write in the program. Loewer ([2001]) develops the idea of the Best System further, showing how it can reconcile chance with an underlying determinism. Great gains in simplicity and strength can be achieved by allowing probabilities of world histories given the system of axioms.

While this account of laws has its problems, I do find it attractive because it seems to get scientific practice roughly right: scientists are concerned with finding laws achieving a certain balance of simplicity and strength. It also deals nicely with all the problems we have raised here. First, look at our world with this idea of law in mind. We try to find the simplest most powerful generalizations we can. Dynamical laws like Newton's second law are highly prized: they are remarkably strong and simple. Still, there are many patterns detectable in the world not derivable from such laws, e.g. the entirety of thermodynamics. So we might also introduce some special science laws and probabilistic laws to capture even more generalizations. These moves will allow us to deduce regularities such as entropy increase. However, in a complicated world like ours there will be trade-offs. Making all those thermal phenomena likely according to the fundamental laws will mean making some crucial events unlikely. Once we introduce probabilistic laws like Boltzmann's, it is inevitable that some states get low measure (in all but purely equilibrium worlds, worlds that would get different Ramsey-Lewis laws anyway). Giving them high measure will either ruin the explanation of the phenomena the probabilistic laws were designed to handle—thereby sacrificing strength—or it will cause one to complicate the laws—thereby sacrificing simplicity.

The Best System provides a nice perspective from which to understand the desiderata for measures (1)–(3) in Section **4**. To enter the Best System, concepts would have to be well-defined. The primitive vocabulary used by the Best System just is the natural one. Further, if a measure over boundary conditions makes it into the Best System, then that is just another way of saying that it 'pulls its weight' in science: the initial probability measure is part of the System that maximizes simplicity and strength on balance.

Finally, when it comes to explanation, the Best System theory knows how to take care of itself. First, with Price ([2004]), note that the Past Hypothesis is actually used as a law in our inferences. Your coffee tomorrow relaxes to equilibrium in room temperature, but you are not surprised. We assume the Past State's entropy is low enough to make thermodynamic generalizations correct yesterday, today and for the reasonable future. Second, since the Past Hypothesis is so fantastically powerful and simply state-able, it seems likely that the Best System would include the Past State as one of its axioms. This law would be an unusual, non-dynamical law; but I know of no argument that says that laws cannot be non-dynamical.[5] The Best System theory abruptly

[5] Lewis seems reluctant to call these axioms 'laws' if they are non-dynamical. He writes, 'the ideal system need not consist entirely of regularities; particular facts may gain entry if they contribute enough to collective simplicity and strength. (For instance, certain particular facts about the Big Bang might be strong candidates.) But only the regularities of the system are to count as laws' ([1983], p. 367). Calling non-dynamical axioms 'laws' may cause some discomfort in ordinary language, but I cannot see any compelling reason to deny them the honorific 'law' if they do indeed make the Best System.

ends our debate, for it will declare that the Past Hypothesis does not call for explanation. Why was entropy low in the past?: 'Because it's physically impossible for it not to be.' A real conversation stopper, answers this question.

7 Explaining the past state

Can the Past State be explained? The answer to this question hangs on what we mean by 'explanation' (and even 'we' and 'can'), so there is plenty of room here for people to talk past one another. Not only are there many different theories of what scientific explanation is, but there are many different contexts in which explanation is needed. I cannot hope, nor do I aspire, to show that the Past State cannot be explained according to any conception of explanation. The super-physics of the distant future may well explain, in some sense to somebody, the Past State, if such there be. What I can do is show that none of the ways you might have thought to explain it are so explanatory. In light of the previous discussion, I hope this makes the reader see that being stuck with a Past State is not so bad.

If the Past State is for some reason not the *very* first state, we need to make a quick detour. It may very well be that we can then explain the Past State in some sense. We could show that the Past State followed from an even earlier state. But this kind of explanation is not going to make the Past State's probability high. Fundamentally, such an explanation of the Past State would be of the same kind as one wherein the Past State was 'explained' by backward-evolving present Cauchy data to the Past State. The unlikely state in the past would still remain. So let us suppose the Past State is the very first state of the universe. Then I believe that there are essentially four different ways to explain the Past State. One could (a) re-write the dynamics so that the Past State would be generic in the solution space of the new dynamics, (b) add a new non-dynamical law of nature, (c) eliminate the measure making such states abnormal, or (d) embed the universe in an ensemble of worlds and invoke an anthropic explanation. In other words, we could make the Past State likely (as a result of new laws or new worlds) or make it 'a-likely'. Let us take these options in turn, starting with the first.

(a) Dynamical explanations

Consider again the so-called flatness and horizon problems in standard Big Bang cosmology. The initial conditions for getting Ω and the temperature right are arguably very small according to some natural measure. Physicists treat this as a genuine problem. The counterpart of the Past State for the flatness problem, namely the '*Really* Flat Past State', is so needy of explanation that inflation is posited. Whatever the vices of inflation (see Earman and Mosterin [1999]), consider an important difference between what the physicists want and what Price wants. With inflation, physicists would *explain away* the Really Flat Past State. Price, by contrast, keeps the Past State and wants to explain why *it* obtains. The counterpart of inflation in the thermodynamic case is modifying or replacing classical or quantum dynamics so that the Past State itself emerges as the natural product of the dynamics—not at all what Price is after. He seems to have lost sight of the explanandum. Originally we wanted to explain *thermal phenomena*. But a dynamics making thermal phenomena generic would *remove the need* for a Past Hypothesis.

What novel mechanism would explain the Past State itself? What evidence do we have for such a mechanism? And why should we prefer this mechanism as brute over the Past State? Price's program for explaining the Past State seems to me a recipe for positing some untestable mechanism merely to satisfy one's a priori judgment of what facts can be basic.

Price replies (personal communication) that we know that the Past State, as described, occurred independently of the inference we make from present thermal phenomena, unlike the special state required by non-inflationary cosmology to explain flatness and temperature. I am not convinced. First, do we really have such strong independent evidence that in the distant past entropy was low? Price thinks that we already know the Past State was of low entropy, for we have lots of evidence for a relativistic hot Big Bang model of the universe, e.g. the usual interpretations of cosmic red shifts and the cosmic background radiation. There is no doubt that the model predicts a highly concentrated past state. Going backward in time, we see the density decrease until the Big Bang. Price's claim hangs on this initial concentrated state being rightly described as a state of low thermodynamic entropy. It is, of course, but I am not sure we know this solely from Einstein's equations and the cosmic background radiation. As I understand matters, the expansion of the universe is an adiabatic process, one that is 'isentropic'—i.e., its entropy does not change—and reversible. Sometimes physicists speak of 'gravitational entropy', but these concepts are very speculative and not obviously related to the thermodynamic entropy (see the Appendix). Second, even if we know that the Big Bang state is one of low entropy, there is simply no reason to explain it rather than anything else unless it is low according to a measure actually being used in science. That is, only the Boltzmannian explanation in statistical mechanics gives us reason to think it is improbable in any objective sense. But if the new dynamics makes the Past State likely, then we are not using the Boltzmann explanation anymore.

(b) New non-dynamical law?

In the sense described in Section **5**, I believe this is the right option. A good theory of laws of nature tells us that we ought to count the Past Hypothesis itself as a law. Does this count as an explanation of the Past Hypothesis? Clearly not. This move does not so much explain the Past Hypothesis as state that it does not need explanation because it is nomic.

Aren't laws sometimes explained in terms of other laws? Yes; indeed, we are currently operating under the assumption that the 'laws' of phenomenological thermodynamics follow from the laws at the mechanical level. Laws can help explain other laws. So if we imagined some new more fundamental laws, it might be the case that these explain the Past State in some sense. It is entirely possible that physics be completely 're-packaged' in the distant future.

Before getting carried away with this idea, think carefully about what would actually count as explaining the Past State. If this re-packaging of physics gets rid of the Boltzmann story, then there may be no need to think of the Past State as unlikely; hence there would be no reason to deem it needy of explanation.

Concentrating on new non-dynamical laws, we are imagining a possibility that the new law is non-dynamical, and yet not simply the statement that the Past Hypothesis is lawlike. Price often seems to have such a picture in mind when he considers Penrose's ([1987]) Weyl Curvature Hypothesis (WCH). This hypothesis says, loosely, that a particular measure of spacetime curvature known as the Weyl curvature vanishes near the initial singularity and that this vanishing implies low entropy. I have three points to make about Price's treatment of this proposal. First, Penrose understands the WCH as a time-asymmetric law, a claim about *past* singularities having low entropy. To me it seems a bit of a stretch to say that such a principle explains the Past Hypothesis. *If* the WCH is related to the thermodynamic entropy, then it is simply the Past Hypothesis dressed in fancy clothing. Likewise, when Boltzmann thought of the Past State he knew nothing of quarks. We now can 'translate' the Past State into the language of quarks, but this does not mean that we have explained the Past State. It is every bit as unlikely as before. Second, Price ([1996]) has an a priori dislike of Penrose's time-asymmetric law and proposes instead a time-symmetric version of it—a law to the effect that singularities, wherever and whenever they are found, have low entropy (for criticism, see Callender [1997]). This empirically more risky hypothesis does make different predictions than Penrose's; e.g., it predicts that any singularity we meet in the future will be a source of low entropy. So I grant that it is much stronger than the Past Hypothesis, going as it does well beyond the empirical evidence—we have no idea whether other singularities will be low-entropy sources. If this hunch is correct, I am happy to change the Past Hypothesis to what would essentially be the 'Singularity Hypothesis'. We could then call this an explanation of the Past Hypothesis, and I will not quibble with that. I just want to insist that without any mechanism linking singularities with low entropy, this new hypothesis would be just as mysterious as the old one. Finally, note that

Penrose himself is clear that the WCH is not supposed to have the stipulative status Price gives it; rather he believes it will follow, presumably dynamically, from some new quantum theory of gravity.

(c) Eliminating the initial probability distribution

If there is a problem with the Past State, I take it the problem is that it is so unlikely. So if new physics worked without an initial probability measure, that might explain the Past State by removing its improbability. There have been and continue to be programs that have this effect. Had Boltzmann's original H-theorem worked, every nomically possible initial condition would subsequently have its entropy increase. And recently, Albert ([2000]) has claimed that something nearly like this result obtains if the Ghirardi-Rimini-Weber (GRW) interpretation of quantum mechanics is right. GRW would make entropy increase subsequently likely for every nomically possible initial condition. The statistical mechanical probabilities would emerge as corollaries of the quantum mechanical probabilities. Though a probability measure is used in the stochastic temporal development of quantum states, GRW does not require a measure over initial conditions to account for statistical mechanics. (One can also imagine adding a stochastic 'kick' term to classical mechanics that would have the same results.)

From our point of view, GRW is an interesting case, for it still requires the same past cosmological state the Boltzmannian does—just not the probability measure over initial conditions. That is, since we know entropy has been increasing throughout history, it must have been much lower much earlier. So we still have a Past State, only now we do not need to say that it is unlikely, because the stochastic development of states will make entropy increase likely for all initial conditions. We know, however, that there is a natural well-defined measure over initial conditions, namely the standard one; moreover, we know that according to this measure, this past cosmological state is of extremely small size. Yet since this probability measure now is no longer needed to explain thermal phenomena, we should *not* think of this small size as an epistemic fault with the theory. The Past State, in this context, becomes a problem on the order of receiving surprising hands in card games.

(d) Anthropic explanations

Anthropic explanations notoriously posit an ensemble of real worlds, each one corresponding to a possible initial condition of the universe. The Past State is then explained as follows. Although the Past State by itself is improbable, if we conditionalize on the fact that we exist while assuming that intelligent life can only exist in worlds with Past States, it is instead highly probable. We should be no more surprised at the Past State than should a fish caught by a net with a one-foot hole in it should be surprised at finding himself in a bucket of one-foot or longer fish. This type of explanation has been effectively criticized elsewhere (e.g., Worrall [1996]) and I have nothing new to add. Let me simply remind the reader that it makes precisely the mistake I warned against earlier. It posits a substantive—and enormously extravagant—claim about the world in order to satisfy an explanatory itch that does not demand scratching.

8 Conclusion

Mellor ([2002]) cites the example of John Leslie wherein a firing squad of fifty aims at you and shoots—but luckily for you, they all miss. Notoriously, Leslie insists that you would rightly demand some further reason for your luck. Mellor responds:

> Well, maybe you would; but only because you thought the ability of the firing squad, the accuracy of their weapons, and their intention to kill you made their firing together a mechanism that gave your death a very high physical probability. So now suppose there is no such mechanism. Imagine, as Russell (1927) did, that our universe [. . .] started five minutes ago, with these fifty bullets coming past you, but with no prior mechanism to give their trajectories any physical probability, high or low. Suppose in other words that these trajectories really were among the *initial* conditions of our universe. If you thought that, should you really be baffled and seek some further reason for your luck? (p. 227)

Mellor thinks not, and he (like me) thinks not no matter when the time is, initially, finally, or in between, so long as there is no mechanism. The Past State case is exactly like this, for here, too, there is no mechanism bringing about the low-entropy state. We can introduce a mechanism to explain thermodynamic behavior, but if it is a plausible one, this will only do away with the Past State. If we instead retain the Past State, there is a sense in which we are (according to the standard measure) very lucky. This *is* baffling, frustrating and inexplicable; so it is important to keep in mind that of all the ways Nature could be, 'most' are more baffling, frustrating and inexplicable.

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Appendix: Gravity and the past hypothesis

It is sometimes said that gravity might play a special role with respect to the Past Hypothesis. One famous idea is that of Thomas Gold and his followers, who claimed that gravity caused entropy increase for all classical initial conditions. Albert ([2000]) and Price ([1996]) discuss problems with this view and I shall not repeat them here. Another more modest idea is that the existence of gravity should lead us to expect or at least think 'natural' an initial low-entropy state. (See e.g. Goldstein ([2001])). Consider the pictures shown in Figure 1, which are adapted from Penrose ([1987]), p. 338.

Imagine for a moment that all the particles of a system are at rest, so that we need not worry about the momentum dimensions of phase space. Gravity is an attractive force, and so the natural, 'equilibrium', state of a set of gravitating bodies is for them to be clumped together in the coordinate dimension of position space. It is then pointed out that being clumped together in coordinate space is just the ordinary low-entropy thermal state, so in some sense the state required by the Past Hypothesis is natural after all. Thus Goldstein ([2001]):

> What is important for our purposes here is that what is arguably the most random, typical, natural and least contrived initial state for a system of gravitating particles, one in which they are uniformly distributed over space (with, say, all of them at rest) also happens to be a state of very low entropy, exactly what is needed to complete Boltzmann's account of irreversibility. (p. 51)

What kind of comfort can gravity provide us? Not much, I think. This explanation, on its face, rests an on equivocation on 'equilibrium'. There is gravitational equilibrium, a clumped state, and non-gravitational equilibrium, a non-clumped state. According to one, the Past State is likely; according to the other, unlikely. What are we to say about this situation?

Since the inception of kinetic theory, the relationship between thermodynamics and gravitation has been strained. Maxwell famously argued with Guthrie and Loschmidt over the definition of equilibrium in a gravitational field. And today, applying thermodynamics to self-gravitating fields is still non-trivial. I cannot resolve these details here, though I hope to work through them in future research. For our purposes, there are basically two options. One is that thermodynamics is seen as a science applicable only in the absence of long-range forces. Thermodynamics would be viewed as the macro-science of short-range forces, inapplicable when a significant gravitational coupling is turned on. The other position is that thermodynamics does still apply when gravity is significant. One then needs to do the Boltzmann counting of states, including the gravitational degrees of freedom. It had then better be the case that, when clumping occurs

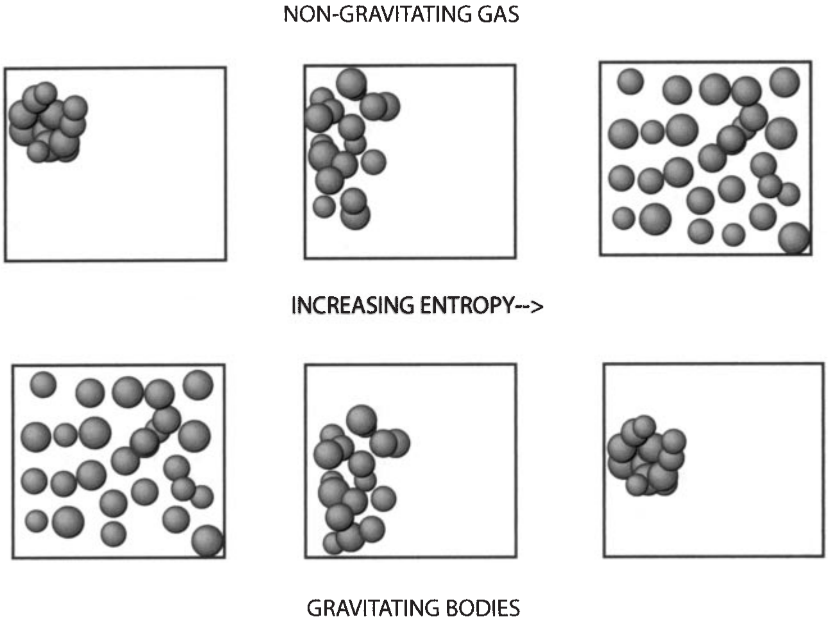

Figure 1. Increasing entropy without gravity and with gravity.

due to gravity, the particles involved pick up a lot of disorganized momentum, making the resulting state one of higher thermal entropy (larger volume in phase space) than before (Albert [2000], p. 90). By moving to more 'likely' regions of momentum space, a system 'ordered' in coordinate space can still be of high entropy, as when a mixture of vegetable oil and water evolves to the more 'orderly' state of separate layers.

In the first case, there will remain two equilibriums, one gravitational and one thermal. The entropy of the thermal equilibrium will be given by the usual Boltzmann formula, where the Hamiltonian used does not include a coupling to the gravitational field. The Boltzmann entropy is $S(E,V) = k \log \Sigma(E)$, where k is Boltzmann's constant and $\Sigma(E)$ is the volume in phase space enclosed by the energy surface of energy E. $\Sigma(E) = \int_{H(p,q) < E} d^{3N}p d^{3N}q$, where the Hamiltonian $H = \Sigma p_i^2/2m$. The entropy of the gravitational equilibrium would be given by some speculative new 'gravitational' entropy, say Penrose's square of the Weyl tensor or some other proposal found in the black hole literature. The question is, which one should govern our degrees of belief about the probability of this Past State? The answer is the usual thermal probability, for it easily meets conditions discussed in Section **4**, whereas the conflicting gravitational probability does not. Gravity's role may lessen our surprise at the state, but the Past State would still be objectively improbable.

In the second case, there will be only one equilibrium state, and it will be calculated including a coupling to the gravitational field. The Hamiltonian would instead be $H = \Sigma p_i^2/2m - \Sigma Gm^2/|x_i - x_j|$. And if a Boltzmannian explanation of entropy increase in terms of the passing of successively more probable states is to work, then the state needed by the Past Hypothesis had better be improbable according to this entropy (assuming it can be calculated to have a non-extreme value—see Kiessling [2001]). Gravity, in this case, will be important in figuring out what the initial state must have looked like, given that its entropy was low even when the gravitation field is considered. However, one will find little comfort from this in positing a low entropy state, for the 'natural' (i.e., probable) state will not be the initial state.

References

Albert, D. [2000]: *Time and Chance*, Cambridge, MA: Harvard University Press.

Brown, H. and Uffink, J. [2001]: 'The Origins of Time-Asymmetry in Thermodynamics: The Minus First Law', *Studies in History and Philosophy of Modern Physics*, **32B**, pp. 525–38.

Callender, C. [1997]: 'Review Symposia: The View From No-When', *Metascience*, **11**, pp. 68–71.

Callender, C. [1998]: 'The View From No-When: Price on Time's Arrow', *British Journal for the Philosophy of Science*, **49**, pp. 135–159.

Callender, C. [1999]: 'Reducing Thermodynamics to Statistical Mechanics: The Case of Entropy', *Journal of Philosophy*, **96**, pp. 348–73.

Callender, C. [2000]: 'Is Time "Handed" in a Quantum World?', *Proceedings of the Aristotelian Society*, **104**, pp. 247–69.

Earman, J. and Mosterin, J. [1999]: 'A Critical Look at Inflationary Cosmology', *Philosophy of Science*, **66**, pp. 1–49.

Ellis, G. F. R. [1999]: '83 Years of General Relativity and Cosmology', *Classical and Quantum Gravity*, **16**, A37–A76.

Evrard, G. and Coles, P. [1995]: 'Getting the Measure of the Flatness Problem', *Classical and Quantum Gravity*, **12**, L93–L97.

Glashow, S. [1999]: 'Does Quantum Field Theory Need a Foundation?' in T.Y. Cao (*ed.*), *Conceptual Foundations of Quantum Field Theory*, Cambridge: Cambridge University Press, pp. 74–88.

Goldstein, S. [2001]: 'Boltzmann's Approach to Statistical Mechanics', in J. Bricmont et al (*eds*), 2001, *Chance in Physics*, Berlin: Springer-Verlag, pp. 39–54.

Hartle, J. [1986]: 'Initial Conditions', in E. W. Kolb et al (*eds*), 1986, *Inner Space/Outer Space: The Interface Between Cosmology and Particle Physics*, Chicago, IL: Chicago University Press, pp. 467–77.

Hume, D. [1980]: *Dialogues Concerning Natural Religion*, ed. R. H. Popkin, Indianapolis, IN: Hackett.

Kiessling, M. [2001]: 'How to Implement Boltzmann's Probabilistic Ideas in a Relativistic World?', in J. Bricmont et al (*eds*), *Chance in Physics*, 2001, Berlin: Springer-Verlag, pp. 83–102.

Lange, M. [2002]: *An Introduction to the Philosophy of Physics*, Oxford: Blackwell Publishers.

Lewis, D. [1983]: 'New Work for a Theory of Universals', *Australasian Journal of Philosophy*, **61**, pp. 343–77.

Lewis, D. [1994]: 'Humean Supervenience Debugged', *Mind*, **103**, pp. 473–490.

Loewer, B. [2001]: 'Determinism and Chance', *Studies in History and Philosophy of Modern Physics*, **32**, pp. 609–20.

Mellor, H. [2002]: 'Too Many Universes', in N. A. Manson (*ed.*), 2002, *God and Design: The Teleological Argument and Modern Science*, London: Routledge, pp. 221–8.

North, J. [2002]: 'What is the Problem about the Time-Asymmetry of Thermodynamics? A Reply to Price', *British Journal for the Philosophy of Science*, **53**, pp. 121–36.

Penrose, R. [1987]: *The Emperor's New Mind*, New York: Oxford University Press.

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

Price, H. [2002]: 'Boltzmann's Time Bomb', *British Journal for the Philosophy of Science*, **53**, pp. 83–119.

Price, H. [2004]: 'On the Origins of the Arrow of Time: Why There is Still a Puzzle about the Low-Entropy Past' in C. Hitchcock (*ed.*), *Contemporary Debates in Philosophy of Science*, Oxford: Blackwell Publishers, pp. 219–239.

Sklar, L. [1993]: *Physics and Chance*, Cambridge: Cambridge University Press.

Worrall, J. [1996]: 'Is the Idea of Scientific Explanation Unduly Anthropocentric? The Lessons of the Anthropic Principle', *LSE Centre for the Philosophy of Natural and Social Sciences Discussion Paper Series*, **25**, pp. 1–20.