CHAPTER TWELVE

There is no Puzzle about the Low-Entropy Past

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12.1 A Story

Suppose that God or a demon informs you of the following future fact: despite recent cosmological evidence, the universe is indeed closed and it will have a “final” instant of time; moreover, at that final moment, all 49 of the world’s Imperial Fabergé eggs will be in your bedroom bureau’s sock drawer. You’re absolutely certain that this information is true. All of your other dealings with supernatural powers have demonstrated that they are a trustworthy lot.

After getting this information, you immediately run up to your bedroom and check the drawer mentioned. Just as you open the drawer, a Fabergé egg flies in through the window, landing in the drawer. A burglar running from the museum up the street slipped on a banana peel, causing him to toss the egg up in the air just as you opened the drawer. After a quick check of the drawer, you close it. Reflecting on what just happened, you push your bed against the drawer.

You quit your job, research Fabergé eggs, and manage to convince each owner to place a transmitter on his egg, so that you can know the egg’s whereabouts from the radar station in your bedroom. Over time you notice that, through an improbable set of coincidences, they’re getting closer to your house. You decide to act, for the eggs are closing in and the news from astronomers about an approaching rapid contraction phase of the universe is gloomy. If – somehow – you can keep the eggs from getting into the drawer, perhaps you can prevent the world’s demise. (Already eight eggs are in the drawer, thanks to your desire to peek and your need for socks.) Looking out your window, you can actually see eggs moving your way: none of them breaking laws of nature, but each exploiting strange coincidences time and again. Going outside, you try to stop them. You grab them and throw them away as far as you can, but always something – a bird, a strange gust of wind – brings the egg back. Breaking the eggs has proved impossible for the same kinds of reasons. You decide to steal all of the eggs, seal them in a titanium box and bury it in Antarctica. That, at least, should buy some time, you think. Gathering all the eggs from outside, you go upstairs to get the ones from the drawer. The phone rings. It's a telemarketer selling life insurance. You decide to tell the telemarketer that their call is particularly ill timed and absurd, given that the universe is about to end. Absent-mindedly, you sit down, start speaking, put the eggs down in the open bureau drawer . . . and the universe ends.

Drop God or the demon from the story. Perhaps science posits this strange future state as a way of explaining the otherwise miraculous coincidences found among the eggs. After all, the hypothesis has some power in explaining the whereabouts of these eggs; it is also very simple. Conditionalizing on it makes all sorts of improbable events probable.

If the standard Boltzmannian explanations of entropy increase and the direction of time are right, then contemporary science posits something *vastly* more surprising than the Fabergé egg hypothesis in the story. It posits what is sometimes called the "Past Hypothesis," the claim that the global entropy at the beginning of the universe is very low. Viewed backward in time, each fundamental physical system in the world is carefully orchestrating its movements to evolve to a low-entropy state roughly 15 billion years ago. The Past Hypothesis demands behavior that is more unlikely than trillions of chicken eggs finally surrounding your bedroom.

Surely this monstrously improbable state – call it the "Past State" – deserves explanation. In his companion chapter (chapter 11), Huw Price argues, in accord with our initial intuitions, that it does. Here, I will argue that when one sees what an explanation of this state involves, it is not at all clear that it can or should be explained. By positing the Past State, the puzzle of the time asymmetry of thermodynamics is solved, to all intents and purposes. (Although I am here claiming that the puzzle of time asymmetry in thermodynamics is effectively solved, there are many other related issues that are still unresolved. For an entry into the foundations of statistical mechanics and time asymmetry literature see, for example, Callender (1999, 2001), Albert (2000), Goldstein (2001), Price (2002), Sklar (1993), and references therein. For a more detailed version of this argument and related issues, see Callender (2003).)

12.2 Thermodynamics and Probability

Classical phenomenological thermodynamics is an amazing science. With simple functional relationships among a few macroscopic predicates, it is able to make successful predictions about all thermal phenomena. Within its domain, there has not been a single exception found to its principal laws. One of these laws, the so-called "second law," has attracted much attention from philosophers. There is, as Price describes in his chapter, a puzzle about how to reconcile the second law with the underlying laws of physics. Gases, for instance, spontaneously relax to equilibrium, always filling their available volumes and never spontaneously returning to nonequilibrium. Why does this happen when it's perfectly possible, according to classical or quantum mechanics, for gases *not* to fill their available volumes? Why, for that matter, does heat always flow from hot to cold and not vice versa, despite cold to hot heat transitions being possible according to classical and quantum mechanics? From the mechanical perspective, these regularities just appear to be coincidences.

The answers to these questions are still controversial. However, there is a majority consensus that Boltzmann's answer is basically right – and right about similar questions regarding any other process governed by the second law of thermodynamics. Roughly, the answer goes as follows. First, separate the macroscopic scale from the microscopic scale. The macroscopic scale will be defined via properties such as pressure, temperature, entropy, volume, and so on. The microscopic scale will be defined via the positions and momenta of all the particles (in classical mechanics) or the quantum wavefunction (in quantum mechanics). Consider the fact that many different microstates can realize the very same macrostate. Slightly changing the position of one proton in your shoe, for instance, will not alter the temperature of the shoe. Boltzmann's insight was to see that those macrostates we call "equilibrium" macrostates are such that they are realized by many more microstates than those we call "nonequilibrium" macrostates. And, in general, higher-entropy macrostates can be realized by more microstates (classical or quantum) than those with lower entropy.

The idea is familiar from playing cards. In a game of poker, with 52 cards and five-card hands, there are over 2.5 million possible hands. Consider the "macrostates" called *royal flush* and *no points*. There are four hands, or "microstates," that can give one a *royal flush*, yet there are over one-and-a-half million hands that correspond to *no points*. Not surprisingly, if the deck is fair, *royal flush* is rare and *no points* is the norm. The explanation of the approach to equilibrium relies on a similar asymmetry: nonequilibrium macrostates are rare, so if a microstate is in a state corresponding to a low-entropy state, it will most likely evolve to a state closer to equilibrium. In this way, one says that it's 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, this Boltzmannian explanation works in both temporal directions. As Price nicely explains, 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* past, contrary to the observed facts. We need to break the symmetry. How? One way is to stipulate a solution to this problem by positing a cosmological hypothesis that states that in the distant past the global macrostate is one of very low entropy. How low? *Really* low: low enough to make thermodynamic generalizations applicable for the roughly 15+ billion years we think these generalizations held or will hold. This hypothesis, suggested by Boltzmann and adopted by Schrödinger, Feynman, and others, is the "Past Hypothesis" mentioned above (coined as such by Albert, 2000) and we'll call the 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 because it started off so low.

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?" I agree with him that we don't really need to ask why it rises, for rising is what is "natural" for entropy to do if a system is out of equilibrium. Should we explain why it was low to begin with? To me, that sounds like asking for an explanation of the initial condition of the uni-

What does "more likely" mean here? Read one way, this question is a tricky issue in philosophy of probability (see, e.g., Sklar, 1993). But the question also arises in a straightforward mathematical sense too. Our coarse measurements of temperature, pressure, and so on are only good up to finite errors. Yet classical or quantum systems take their values – say, for position – from the continuous infinity of values associated with the real number line. Really there are infinitely many microstates that can realize any given actual macrostate. Assuming they are all equally probable, this seems to imply that each state has zero probability. Technically, this is a nonstarter. Yet giving different probabilities as a function of the number of microstates that "look like" a certain macrostate is crucial to statistical mechanics. Enter measure theory, developed originally by the French mathematician H. Lebesque. Measure theory provides a rigorous way of understanding the length or size of a set, even of sets that intuitively have no length (it also is important for the idea of an integral and many concepts in analysis, topology, and more). Importantly, it solves our problem with infinities of microstates. To see how, consider a very simple case, the interval [0, 1]. We can determine the size of the proper subset [0, 0.5] if we define the size as simply 0.5 – 0. We can therefore say the size of [0, 0.5] is greater than the size of the interval between [0.4, 0.6] despite the fact that each contains a continuous infinity of points. We are here just using the simple rule that if the interval is [a,b], then the size is given by [b – a]. Matters become much more complicated with different kinds of sets and a more rigorous and general definition of measure is then needed. But from this example one can see how the concept of measure is important in defining nontrivial probability distributions on continuously infinite sets. Measures (sizes) are defined of sets of microstates and probabilities are a function of these sizes. Note that the rule we used ([b – a]) is a very "natural" one, but in fact, formally speaking, there is an indefinite number of alternative rules. If we had used the bizarre rule [b – a^3], then it would say [0, 0.5] is smaller than [0.4, 0.6].

verse, and we need not explain those for reasons that I'll describe (see also Sklar, 1993, pp. 311–18; Callender, 1998, 2003).

12.3 What We Really Want Explained

Suppose, for simplicity, that the universe is closed and has what we might call a beginning and an end. In fact, recent evidence suggests that it may keep expanding forever, but there are no laws of nature saying that it must. Suppose also that the spacetime is well behaved enough causally that we can foliate it via a succession of distinct spatial hypersurfaces at different times. Then we can say that the universe has initial and final slices of time.

Is there reason to think that the labels "initial" and "final" carry any significance when it comes to explanations? In particular, is there reason to think that final conditions need explanation but initial ones do not?

One can imagine reasons for thinking so. For instance, if the laws of nature were not time-reversal invariant (i.e., if they cared about the direction of time), then only one temporal ordering of slices of time of the universe would be allowed by the laws – the opposite ordering would break the laws. This law-like asymmetry may sanction an explanatory asymmetry. Consider the state of some system S at time t_1 and another state S^* at t_2. If the laws of nature tell us the probability of evolving from S at t_1 to S^* at t_2 *but not vice versa*, then we'll have explanations from t_1 to t_2 but not vice versa. Given t_2, the laws entail *nothing* about the state at t_1. It would therefore be difficult to know what considerations to use in explaining t_1 in terms of what happens at t_2. At any rate, except where noted, the present debate operates under the assumption, for better or worse, that the fundamental laws are time-reversal invariant (on whether they are, see Callender, 2000).

Another reason for treating final and initial conditions differently – one that I very much want to resist – is the thought that it follows from what we mean by "explanation," or more specifically, "causation." Many philosophers are attracted to the idea that good scientific explanations are causal explanations: an event E is explained by citing one or more of its causes C. But causation is typically temporally asymmetric. Usually, if C causes E, then C precedes E. On this view, 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. To do so would be to commit what Price calls a "temporal double standard." Recall the Fabergé egg story. Doesn't knowing the final state of the universe explain why the eggs were moving ever closer to the sock drawer? I am loath not to count this as an explanation merely due to its unusual time direction. Doing so without saying more would clearly beg the question in this context, as Price's chapter shows.

I want to treat initial and final conditions the same way. Hence, in claiming that we shouldn't explain initial conditions of the universe, I should also state that we shouldn't explain final conditions of the universe. But isn't this an odd thing to say? Why shouldn't we try to explain why that state is the way it is?

The key to seeing the answer is to see that the issue is not really about initial conditions versus final conditions, nor even about these versus what lies between. In one sense of "explain," we certainly can explain the initial or final state. Just grab some state before the final state, evolve it in accord with the laws of nature and show what it yields for the final state. In figure 12.1, we might show how the final state arises from S(B) and the laws of nature. Alternatively, we can also explain the initial state of the universe in terms of a state after it, assuming that the laws are time-reversal invariant. For instance, we might show how the initial state arises from S(A) and the laws. The only objection that one might have to this explanation would be one committing Price's temporal double standard.

I trust that Price and others are after something much more than this kind of explanation. When people ask for an explanation of the Past Hypothesis, they are not merely asking whether the Past State is consistent with the laws and conditions on some other time slice. All hands agree that this is at least 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 used in statistical mechanics. Penrose (1989, p. 344) estimates that the probability of this particular type of past state occurring is 1 out of $10^{10^{123}}$ and Kiessling (2001) estimates that it is infinitely improbable!

I have worries about both calculations. But however it is calculated, one must admit that the Past State is going to be monstrously unlikely if the calculation is done with

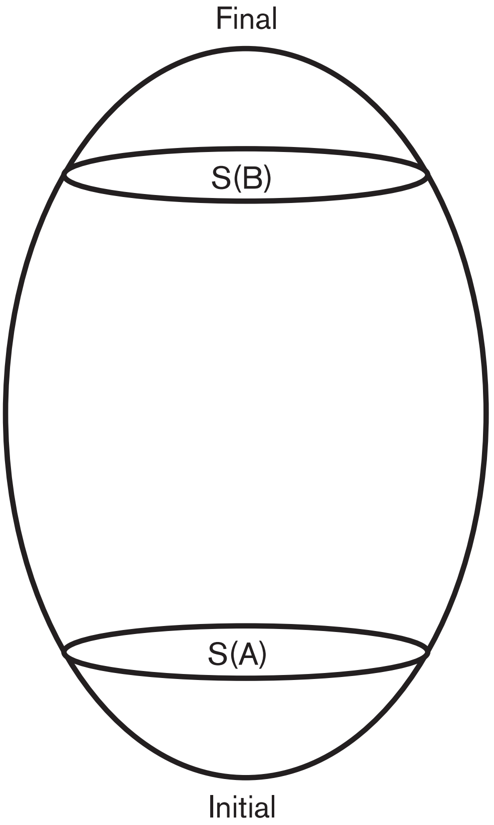

Figure 12.1 A closed time-symmetric universe.

the standard measure. If we want to understand 15 billion years of dynamical evolution as evolution always to more probable states, that first state will need to be very improbable.

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’m skeptical that one might do this while retaining Boltzmann’s story and, more importantly, I’m 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 writes “That in nature the transition from a probable to an improbable state does not take place as often as the converse, can be explained by assuming a very improbable initial state of the entire universe surrounding us. This 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*” (1897; my emphasis). By contrast, Kiessling and others think that it points to a “need for a deeper postulate” (2001, p. 86). As I’ll show, this tension within science and philosophy about explanation has echoes in many other areas as well.

12.4 Brute Facts and Explanation

My objection to the idea of explaining boundary conditions originates in arguments by David Hume. Consider St. Thomas Aquinas's classic cosmological argument for the existence of God. 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, said Aquinas, we find ourselves in a dilemma: either there was an infinite chain of causes and effects or there was a first cause, the Uncaused Cause (God). Not believing that an infinite chain of causation would be explanatory (for reasons that are not entirely compelling now), Aquinas concluded that there was an Uncaused Cause. Similar arguments from motion yielded an Unmoved Mover. There are several objections to these classic arguments. One reaction popular among students is to ask, as Hume did, what caused or moved God? This question raises many 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 something distinct from Himself? But then, one thinks, if it's 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.

Although the situation with the Past Hypothesis is more complicated, at root the above is my criticism of Price. What would explain a low-entropy past state? The most natural thing to say is that an even lower entropy state just before the Past State would explain it. 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 (or in Price's terminology, a Normal Abnormal State)? 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:

> . . . the 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 that 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 and when they don't. 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 nonlocal forces not explanatory and yet contact forces explanatory? This is the second objection above. Furthermore, note that believing Newton's action-at-a-distance to be 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 the posits came at the price of these 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're considering. But still, it is highly improbable. Although 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. The standard probability distribution pulls its weight in science and seems to be a successful theoretical posit in science. Can the improbability of the state mean that it can't 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 don't think that explanation and probability have such a tidy relationship. Lots of low-probability events occur and not all of them demand explanation. Arguably, 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. Furthermore, the sorts of explanations of the Past Hypothesis that Price (1996) envisions seem to me to be examples that are wildly speculative, potentially untestable, 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 acceptable as brute and others not. Instead, we 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 realist). What we don't 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),

> 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. (p. 13)

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, at least according to my (perhaps idiosyncratic) intuitions about explanation. (I try to back up this claim in section 12.6.)

There is an echo of our debate in quantum field theory. Famously, many physicists complain that the standard model in particle physics 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 metaquestions associated with particle physics. (Glashow, 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? Surely that is too strong an explanatory demand to impose on physics.

Again, it seems to me that it's perfectly within the rights of the physicist to find something ugly about the standard model and 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't 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, in agreement with Price, I want to show that the Past Hypothesis operates as a fundamental law. If one agrees that 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. Secondly, I then want to sketch why none of the ways I can imagine of explaining the Past State really count as explaining the Past State.

12.5 The Past Hypothesis as Law

There are many different conceptions of what a law of nature is. See, for example, chapters 7 and 8 of this volume, by John Roberts and Harold Kincaid. According to some, the Past Hypothesis counts as a law and according to others it probably doesn't. Here, I simply want to point out that according to one view of laws of nature, one attractive to some contemporary empiricists, the Past Hypothesis would count as a law. This conception is known as the "Ramsey–Lewis" account of laws, after the philosophers Frank Ramsey and David Lewis. The Ramsey–Lewis theory would seem to abruptly end our debate, for it will declare that the Past Hypothesis doesn't 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.

Lewis describes the theory as follows:

> Take all deductive systems whose theorems are true. Some are simpler and 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 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 that 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. And simply writing "anything goes," while short and sweet, doesn't allow one to infer much in particular. The laws of nature are 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.

Look at our world with this idea of law in mind. We try to find the simplest most powerful generalizations we can. Dynamical laws such as Newton's second law and Schrödinger's equation are great, because they're remarkably strong and simple. But many more patterns are detectable in the world that do not follow from such laws, such as all thermal regularities. So, we might also introduce some special science laws and probabilistic laws to capture these regularities.

Suppose, as seems to be the case, that capturing these thermal regularities implies positing a special state of the universe very early on. Since this special state is so fantastically powerful, allowing us to make predictions in thermodynamics and elsewhere, and yet so simply state-able, it seems very likely that the Past Hypothesis would be among the best system's axioms. Lewis himself seems to be reluctant to call axioms "laws" if they happen only once. 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). Apart from the possible ordinary-language uneasiness of calling an initial state a "law," I can't see any motivation for claiming that some of the axioms of the Best System are laws but not others. In any case, it would have the same kind of status – axiom status – as the laws.

Price is right to point out that we use the Past Hypothesis in a law-like manner. We don't continually rewrite the Past Hypothesis as we notice that entropy keeps increasing. We posit entropy to be low enough for the observed past *and* the inferred future. We don't expect entropy to start decreasing in the next moment. But this just means that the Past Hypothesis is functioning as a brute explainer in the best systematization that we have of the world.

12.6 Explaining the Past State

What would explain the Past State? The answer to this question hangs in large part 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 can't hope to – nor do I aspire to – show that the Past State can't be explained according to any conception of explanation. What I can do, however, is show that none of the ways you might have thought of to explain it are so promising or 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.

Let's suppose the Past State is the very first state of the universe. I've argued elsewhere (Callender, 2003) that it doesn't make a difference if it isn't. Then I believe that there are essentially three different ways to explain the Past State. One could (a) rewrite the dynamics so that the Past State would be generic in the solution space of the new dynamics, (b) add a new nondynamical law of nature, or (c) eliminate the measure that makes such states abnormal. In other words, we could make the Past State likely (as a result of new dynamics or not) or make it "a-likely." Let's take these options in turn.

12.6.1 Dynamical explanations

Consider the so-called "flatness problem" in standard big bang cosmology. It is a problem structurally very similar to the one under consideration. What is the problem? Our universe right now appears to be very nearly flat; that is, spacetime is curved, but only very slightly. Cosmologists are interested in a parameter known as Ω, the so-called critical density. Near-flatness in our universe means that Ω must now be very close to 1. The trouble is that the laws of nature governing the dynamics of spacetime – Einstein's field equations – are such that (using a model known as the Friedman model) departures from "flat" curvature should grow larger with time. If the universe began with even minute curvature irregularities in it, these should be greatly enlarged by now. But the universe presently appears to be roughly flat, so the Friedman equations demand that it must have been even closer to flat much earlier. In terms of Ω, for Ω to now fall anywhere near 1, say even in the range $0.1 \leq \Omega \leq 10$, near the beginning of the universe Ω must have been equal to 1 within 59 decimal places. So-called "inflationary scenarios" are the main response to the flatness problem and other similar problems. Inflation posits a period of very fast universal expansion that would, if correct, reduce the "specialness" of the initial conditions needed for long-lasting near-flatness of the universe. (Note that, unlike in the statistical mechanical case, there is considerable controversy regarding which measure is appropriate – and even whether there exist well-defined measures on the relevant spaces of cosmological histories. For more about this, see Ellis (1999, p. A61), Evrard and Coles (1995), and Callender (2003).)

Note the similarity to our problem. A vast regularity (entropy increase, flatness) is compatible with the laws of nature only if the universe began in an immensely unlikely state. Physicists do seem to think that this is a problem. The counterpart of the Past Hypothesis, namely the "past is *really* flat hypothesis," demands explanation, they think, and so they posit inflation. I'm not so sure that it does (for some of the reasons, see Earman, 1995). Even if I agreed, however, consider an important difference between what the physicists want and what Price wants. With inflation, physicists would *explain away* the "past is really flat hypothesis." Price, by contrast, wants to keep the Past Hypothesis and explain why that Past State obtains. Yet the counterpart of inflation in the present case is modifying or replacing classical or quantum dynamics so that the Past State itself emerges as the natural product of the dynamics. That is not what Price is after. It seems that he has lost sight of the explanandum. Originally, we wanted to explain *thermal phenomena*. But a dynamics that makes thermal phenomena inevitable or generic would *remove the need* for a Past Hypothesis.

If we stick with Price's new explanandum, explaining the Past State itself, and it is the first state, then any kind of novel causal process that brings this state about would seem to require referring to a mechanism outside of spacetime – or adding more spacetime to current models than is warranted by empirical evidence. But that seems to me suspiciously akin to adding some untestable mechanism merely to satisfy one's *a priori* judgment of what facts can be brute. Worse, why should I prefer this brute element over the previous one?

Price would say 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 noninflationary cosmology to explain flatness and temperature.

I have two replies to this response. First, do we really have such strong independent evidence that in the distant past entropy was low? Price is thinking that we already independently know that the Past State was of low entropy, for we have lots of evidence for a relativistic hot big bang model of the universe in 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'm not sure that we know this independently solely from Einstein's equations. As I understand matters, the expansion of the universe is what is called an "adiabatic" process, one that is "isentropic" – that is, its entropy doesn't change – and reversible. Sometimes people talk of "gravitational entropy," but this is very speculative and not obviously related to the thermodynamic entropy. Secondly, and putting details aside, even if we know 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 that is actually being used in science. That is, only the Boltzmannian explanation in statistical mechanics gives us reason to think that it's improbable in any objective sense. But if the new dynamics makes the Past State likely, then we're no longer using the Boltzmann explanation.

12.6.2 New nondynamical law?

In the sense described in section 12.5, I believe that 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 doesn't so much explain the Past Hypothesis as state that it doesn't need explanation because it is nomic.

Aren't laws sometimes explained in terms of other laws? Yes; indeed, we're currently operating under the assumption that the "laws" of thermodynamics follow from the laws of mechanics, the law of low past entropy, and a statistical postulate. 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, however, 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 (if unlikeliness alone even counts as a reason). We will then have "explained" the state only by removing the idea that it is an unlikely state.

Concentrating on new nondynamical laws, we're imagining a possibility that the new law is nondynamical and yet not simply the statement that the Past Hypothesis is law-like. Price often seems to have such a picture in mind when he considers Penrose's (1989) Weyl Curvature Hypothesis. 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 Weyl Curvature Hypothesis 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 Weyl curvature is related to the thermodynamic entropy, then the Weyl Curvature Hypothesis just is the Past Hypothesis, but dressed in fancy clothes. The Past Hypothesis would follow *logically*, not causally, from the Weyl Curvature Hypothesis, so I wouldn't say that the second explains the first in any interesting sense (no disrespect to logical explanations intended – they're great, especially in logic). Secondly, Price (1996) dislikes Penrose's time-asymmetric law and proposes instead a time-symmetric version of the Hypothesis, a law to the effect that singularities, wherever and whenever they are found, have low entropy. (He seems to think, *a priori*, that the laws of nature must be time-symmetric – something for which I've criticized him before: see Callender (1998).) This more risky hypothesis does make different predictions than Penrose's; for example, it predicts that any singularity that we meet in the future will be a source of low entropy. This hypothesis, I grant, is much stronger than the Past Hypothesis, but note that it goes well beyond the empirical evidence. Based on our experience of one singularity, the big bang, we simply have no idea whether or not other singularities will be low-entropy sources. If it turns out that they are, I'm 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 wouldn't 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 does not want the Weyl Curvature Hypothesis to have the kind of stipulative status that it appears to get from Price. Rather, this hypothesis is a conjecture that it will follow, presumably dynamically, from some new quantum theory of gravity for Penrose.

12.6.3 Eliminating the initial probability distribution

There are a variety of programs that would eliminate the need for an initial probability distribution. If there is a problem with the Past State, I take it that the problem is that it is so unlikely. So if new physics could thrive without calling the Past State unlikely, 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 this option follows from the Ghirardi–Rimini–Weber (GRW) interpretation of quantum mechanics. In GRW, the statistical-mechanical probabilities seem to be corollaries of the quantum-mechanical probabilities. Although a probability measure is used in the stochastic temporal development of quantum states, GRW doesn't 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 as GRW classically.)

From our point of view GRW is an interesting case, for it still requires the same past cosmological state that the Boltzmannian does – just not the probability measure over initial conditions. That is, since we know that entropy has been increasing throughout history (history is vastly more interesting than merely equilibrium-to-equilibrium transitions) it must have been much lower much earlier. So we still have a Past State, only now we don't need to say it's 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 (GRW does that instead) 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.

12.7 Conclusion

The only types of explanation that I believe I've left out are so-called "anthropic" explanations. These explanations 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. Sure, we are told, the Past State is unlikely, but if we factor in the (alleged) fact that intelligent life can only exist in worlds with Past States, we should not be surprised to find ourselves in a world with a Past State. We should be no more surprised than 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, which has been criticized elsewhere (see Worrall, 1996), is making precisely the mistake that I warned against earlier. It is positing a substantive – and enormously extravagant – claim about the world in order to satisfy an explanatory itch that does not need to be scratched. Those seeking to explain the Past State who are not motivated by empirical considerations or inconsistencies need a reconsideration of scientific methodology more than they do the ensembles of worlds, imaginary times, and recurring universes to which they appeal.

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