British Journal for the Philosophy of Science, vol. 53, March 2002, pp. 121-36

What is the Problem about the Time-Asymmetry of Thermodynamics?: A Reply to Price

Jill North

Huw Price argues that there are two conceptions of the puzzle of the time-asymmetry of thermodynamics. He thinks this puzzle has remained unsolved for so long partly due to a misunderstanding about which of these conceptions is the right one and what form a solution ought to take. I argue that it is Price's understanding of the problem which is mistaken. Further, it is on the basis of this and other misunderstandings that he disparages a type of account which does, in fact, hold promise of a solution.

The Problem

As Price emphasizes, we must first make sure to understand the problem before we can try to solve it. So what is it that we find puzzling about thermodynamics?

The best way to get at the puzzle, it seems to me, is by means of the following observations. In our experience, systems increase in entropy in the forwards direction of time:[1] ice cubes melt, coffee cools, gases expand to fill their containers. The second law of thermodynamics, which says that the total entropy of the world (or of any isolated sub-system) never decreases, captures the time-directedness of these processes. The question is where the time-asymmetry of this law comes from.

---

[1] As Price notes, the issue of the time-asymmetry of thermodynamics is distinct from the question of whether there is an objective direction to time, and in particular whether one direction is objectively the forwards one. For convenience, I refer to the 'future direction of time' and the 'backwards direction of time', where these should be understood as shorthand for 'what we take to be the future direction of time' and 'what we take to be the backwards direction of time' respectively.

In the nineteenth century, the hope arose that thermodynamics could be reduced to, or in some sense grounded in, statistical mechanics. But the Newtonian dynamical laws, which on this picture underlie thermodynamic behavior, are symmetric in time: according to Newtonian mechanics, whatever can happen forwards can also happen backwards. As far as this time-reversal invariant theory is concerned, there is no law-like difference between the past and the future. The problem is that in our experience, certain processes just do not happen backwards: our experience suggests there *is* a law-like asymmetry between the past and the future. And Newtonian mechanics, it seems, can not account for this asymmetry.

We now know, of course, that Newtonian mechanics is not the fundamental theory of the world. But it seems that no other serious candidate for such a theory will be of more help here, for these theories are similarly invariant under time reversal,[2] and processes like the warming of coffee and the contraction of a gas to one corner of its container are consistent with these theories too. Every one of these theories (with an exception to come) is therefore at odds with the time-asymmetry of our experience, just as Newtonian mechanics is.

So the question remains: what grounds the asymmetry of the second law of thermodynamics? *Why* does coffee cool and ice melt, if the underlying theory allows for the time-reversed processes to occur? The problem, then, is to explain the time-asymmetry of thermodynamics, given that the underlying laws are symmetric in time.

---

[2] The problem arises regardless of one's particular view about what it is for a theory to be time reversal invariant. On the traditional notion, these theories are clearly invariant. On an understanding of time-reversal invariance such as David Albert's, these theories are invariant in at least a partial way—with respect to the positions of particles—and so generate the same difficulty with respect to the asymmetry of thermodynamics. (See Albert [2000], pp. 14-16.)

Price's Approach

Thus, it seems that what we want explained, vis-à-vis the time-asymmetry of thermodynamics, is the fact that all of our experience appears confirmatory of a generalization to the effect that entropy never decreases. Price concludes, however, that "the crux of the observed thermodynamic asymmetry is an existential or particular fact, concerning the nature of our universe early in its history" ([2002], p. 3). On his view, the explanandum is not a generalization about the time-asymmetry of our experience. Instead, the core explanatory issue is why the universe began in an extremely low-entropy state.

Price espouses what he calls an "Acausal-Particular" view of the problem, which he contrasts with the "Causal-General" approach. For the Generalist, the asymmetric explanandum of thermodynamics is a law-like generalization, e.g., that entropy never decreases. A solution, on this picture, would explain the observed thermodynamic regularity, perhaps by locating a causal mechanism responsible for it. The Particularist, on the other hand, takes the explanandum to be the obtaining of special initial conditions of the universe. On this understanding, there is no time-asymmetric generalization to account for; *a fortiori*, there is no underlying causal mechanism which gives rise to it.

Price claims to be advocating a Boltzmannian type of Acausal-Particular account. Let us look more closely at his version of this solution. Consider a vial of pressurized gas which is inside a larger container. When the vial is opened, the gas expands to fill the container. Price's explanation of this process is the following:

> We consider what possible future 'histories' for the system are compatible with the initial set-up. The key to the statistical approach is the idea that, under a plausible way of counting possibilities, almost all the available microstates compatible with the given initial macrostate give rise to future trajectories in which the gas expands. It is *possible*—both physically possible, given the laws of mechanics, and epistemically

> possible, given what we know—that the actual microstate is one of the rare ‘abnormal’ states such that the gas stays confined to the pressurised vial. But in view of the vast numerical imbalance between abnormal and normal states, the behaviour we actually observe is ‘typical’, and therefore calls for no further explanation ([2002], pp. 12-13).

As Price notes, according to Boltzmannian statistical considerations, entropy increase is overwhelmingly likely. The Boltzmannian approach thus yields what Price calls a ‘normalizing explanation’ according to which entropy increase is ‘typical’: no explanation for the observed thermodynamic regularity is needed over and above an appeal to its statistical normality. As he writes in his book, *Time’s Arrow and Archimedes’ Point*, ‘things are more in need of explanation the more they depart from their natural conditions’ ([1996], p. 39); and Boltzmannian statistics have shown entropy increase to be the ‘normal behaviour of matter’ ([2002], p. 15). For Price, then, the fact that the natural measure over microstates deems entropy increase unexceptional—by counting the number of normal microstates as overwhelmingly large—means that this behavior is not in need of explanation beyond that offered by these statistical considerations.

Since Boltzmannian statistical considerations have explained the second law of thermodynamics, this generalization, on Price’s view, is not the explanandum we have in mind when it comes to the time-asymmetry of thermodynamics. The real puzzle, says Price, is why the universe was ever out of equilibrium in the first place, given that equilibrium is the most probable or normal condition for a system to be in. The initial low-entropy condition of the universe, in other words, *is* statistically abnormal and therefore in need of explanation. So the puzzle about the time-asymmetry of thermodynamics is not, ‘Why does entropy increase?’, as we may have thought, but rather, ‘Why isn’t entropy high almost everywhere, almost all the time?’ ([2002], p. 13). Price concludes that all we need in order to explain thermodynamic asymmetry is an explanation of the initial condition of the universe—plus the ‘normalising explanation’ that otherwise, the thermodynamic behavior we observe is unexceptional.

Armed with this understanding of the problem, Price argues that the Causal-General approach is doomed to failure. In positing a causal mechanism responsible for entropy increase, this kind of account is committed to the claim that if it were not for that mechanism, things would not behave as we now observe them to behave thermodynamically. More specifically, the Causal-Generalist is committed to the claim that, ‘If it were not for the mechanism M, the system in question would occupy an “abnormal” (entropy-reducing) microstate’ ([2002], p. 29).

Price thinks that this counterfactual is unjustified. He argues that the Generalist has no reason to assume that if the proposed mechanism suddenly failed, the evolution of thermodynamic features would be any different from what we now observe them to be: just because there is no causal mechanism driving entropy increase does not mean that a system will exhibit abnormal behavior. In fact, Price maintains, we should assume the opposite. For ‘our only guides as to what to expect in the imagined counterfactual situation are our epistemic probabilities’ ([2002], p. 29). And the natural epistemic measure counts normal thermodynamic behavior as overwhelmingly likely. Therefore, once the Boltzmannian statistical considerations are in place, there is no work left for a causal mechanism to do: since the natural measure *always* leads us to expect entropy-increasing behavior, it seems that no account can satisfy the kind of counterfactual it must satisfy in order to succeed in locating a cause of such behavior. Price concludes that the Causal-Generalist misconceives the problem, construing the puzzle as the question of why entropy increases when the real puzzle is why the universe ever began in its low-entropy condition in the first place.

Problems with Price's View

Price, however, reaches these conclusions on the basis of some misunderstandings. First of all, he misconstrues the argument behind the Causal-General approach, and in so doing renders his own objections irrelevant. Price represents the Causal-Generalist as thinking that we need to find a mechanism which forces entropy to increase towards the future in order to be able to explain this behavior. Although this might accurately describe what some General theories look like, it is misleading as a conception of what motivates these views. For the Generalist is not after a causal mechanism per se. The goal, after all, is to *account for thermodynamics*. And since the Boltzmannian story does manage to do this, the Generalist's argument can not be that this solution is deficient in failing to identify some mechanism responsible for normal thermodynamic behavior. Rather, the argument must be that a Causal-General theory can account for thermodynamics *better* than the Boltzmannian one. Price's attack on the causal commitments of General views is therefore beside the point, for it does not address the issue of whether such a theory can account for thermodynamics.

Consider an example of a Causal-General account recently proposed by David Albert in his book *Time and Chance*. Albert suggests that the dynamics of the GRW interpretation of quantum mechanics might be able to account for thermodynamics. The suggestion is that if GRW turns out to be a true theory, then its wave-function collapses might be the underlying cause of the entropy-increasing tendency of thermodynamic systems. The argument in favor of this account, though, is not that it supplies the requisite causal mechanism which is missing from Boltzmann's story. The contention, rather, is that a GRW-based statistical mechanics can provide a better account of our thermodynamic experience.

This is because a Boltzmannian statistical mechanics (on any interpretation of quantum mechanics other than GRW) will require two kinds of fundamental probability laws, the uniform probability distribution over initial wave functions plus the quantum-mechanical probabilities. The former is needed in order to ground thermodynamic asymmetry, since if a system were to begin in an abnormal microstate, the deterministic equations which govern the evolution of its wave function would entail that it would evolve to as to decrease in entropy. Hence the need for a probability distribution over initial wave functions in order to make it overwhelmingly unlikely that a system ever begins in an abnormal microstate in the first place.

A GRW-based statistical mechanics does away with this probability distribution. On this theory, it is the probability per unit time of a wave-function collapse, not an initial distribution over possible microstates, which results in the overwhelmingly high probability of entropy increase with which we are familiar. Here there is no need for the Boltzmannian distribution over phase space since no matter which micrsotate among those compatible with its macrostate a system starts out in, GRW's dynamics entail that it is overwhelmingly likely to evolve in accord with the second law of thermodynamics. The GRW account thus requires only one fundamental probability law: the statistical-mechanical probabilities just are the quantum-mechanical probabilities. This is therefore a simpler, more unified theory of thermodynamics; hence, *ceteris paribus*, it is preferable to the Boltzmannian one.

Of course, the very possibility of such an account depends on the truth of GRW as an interpretation of quantum mechanics. It must also be shown that a GRW-based statistical mechanics is capable of reproducing the (empirically confirmed) probabilistic predictions yielded by the Boltzmannian measure over phase space.[3] One might have legitimate concerns

---

[3] See Albert ([2000], pp. 155-159) for why it seems a GRW-based statistical mechanics should be able to do this.

about whether GRW can satisfy either of these requirements and, so, about whether it is capable of grounding thermodynamics. Price, however, never indicates that he has any doubts about the empirical adequacy of this account; and his attack on the theory's causal claims does not impinge on this assessment. Indeed, as long as we are able to demonstrate that GRW is true of our world and that it yields the overwhelmingly high probability of our entropy-increasing experience, one wonders what more we need in order to reasonably conclude that it is the *cause* of that experience. In any case, since our concern is with finding the right theory of thermodynamics, not with evaluating what such a theory might have to say about the underlying causal structure of the world (which would be an interesting question once we have the correct theory in hand), the objections Price raises are not relevant.

Price thinks his 'counterfactual containment problem' reveals that even if GRW were true of our world, we still would have no reason to believe that it is responsible for the observed thermodynamic regularity. Although we now see that this is a misleading way of framing the debate, it is worth examining why this argument fails.

Price is correct that Causal-General solutions are committed to some kind of counterfactual claim. But he is wrong about which claim they must satisfy; and the counterfactual they *are* committed to *is* justifiable. For suppose there is some causal mechanism underlying entropy increase. These accounts do not have to hold that if it were not for this mechanism—if it were to fail in some system—that system would occupy an abnormal microstate. Rather, they are committed to a claim such as the following: *if* the mechanism they propose *is* what is responsible for the observed entropy-increasing tendency of thermodynamic systems, then if this mechanism were to fail in a given system, we simply would not expect the system to exhibit this tendency. And (as Price himself notes) not expecting a system to behave with an entropy-increasing tendency does not mean we must expect it to behave with an entropy-decreasing tendency.

Return to the GRW proposal. Consider a system for which GRW does not predict a collapse, such as a tiny isolated gas on the order of 10^5 particles. A GRW-based statistical mechanics must contend that this gas will not behave with the law-like increase in entropy we observe of systems in which a collapse does occur.[4] But this does not mean the account is committed to this system's behaving abnormally. The GRW account, remember, does away with the Boltzmannian probability distribution over microstates: there is no statistical claim to tell us that the probability of a system's occupying an abnormal microstate at a time is extremely small. Therefore, if a collapse fails to occur in some system, how that system behaves will depend on which microstate it happens to start out in; and some of these microstates, in the absence of a collapse, will lead to entropy decrease; but some of them will not. And without the natural measure over phase space, there is no way of saying how likely either result is.[5] This may strike us as counter-intuitive: we might assume, as Price does, that *any* gas must expand to fill its container. But it is important to keep in mind that this assumption is based on the experience we have had so far, and that we have not yet had any experience of such tiny systems. Indeed, it is difficult to imagine how we *could* ever experience such a tiny gas: once we interact with it, after all, it will no longer be the tiny isolated system it must be in order for GRW to predict the failure

---

[4] Note that this is on the assumption that the gas has been isolated for a long time; otherwise, we can appeal to collapses which occurred in the system's past in order to conclude that it will behave normally. Here I address the most problematic case for the GRW account.

[5] Price takes this result to be in his favor: 'Without some basis on which to say what would happen in the counterfactual case, the best that can be achieved is a kind of agnosticism about the effects of the mechanism in question—the view that we simply *can't say* whether it makes a difference. Clearly, this agnosticism falls short of a positive commitment to the view that the GRW mechanism is causally responsible for the phenomena in question' ([2002], p. 52 n. 18). But this agnosticism is precisely what the theory should yield, for this is the correct claim that the account is committed to.

of a collapse. So it might just be the case that these systems do not tend to increase in entropy, as a GRW-based thermodynamics would predict.

In short, this theory claims that the GRW jumps are responsible for the thermodynamic regularities we observe. In order to be able to conclude that these jumps are so responsible, then, all the Generalist needs to establish is that in their absence there will be no such reliable thermodynamic regularities—not that there will be an entropy-decreasing regularity. And a GRW-based statistical mechanics, in eliminating the Boltzmann distribution, gives this result. Price, of course, maintains there can be no reason to expect a system to behave any differently without the alleged causal factor. But if GRW turns out to be a true theory, then this is precisely what we should expect—and with *good reason*, namely, that this is how the true theory of our world says such a system will behave. Empirical evidence of the truth GRW, in other words, is what would justify the (correctly formulated) counterfactual this account is committed to. And since the very possibility of a GRW-based statistical mechanics hinges on the truth of the GRW theory, this is not at all problematic for the view.[6]

---

[6] It might be worth working through Price's counterfactual a little more carefully. There are two other situations Price might have in mind: (1) GRW is true of our world, and the collapse mechanism is suddenly turned off in some system; or (2) the collapse mechanism is turned off at the initial state of the universe. Neither case, however, is problematic for the GRW account.

Suppose the GRW mechanism is in place, and that it is now turned off in, say, a cup of ice. According to Price, in order to conclude the collapses are responsible for the entropy-increasing behavior we had observed, this system must now exhibit abnormal behavior: the ice must begin to re-freeze, or melt at a different rate. And since the ice is overwhelmingly likely to continue to melt (and at the rate we expect), Price will conclude that the jumps could not have been responsible for its normal behavior: remove the alleged cause, and we have the same effect. *Contra* Price, however, this system's behavior *is* due to GRW. For the collapses which occurred in the system's past render it overwhelmingly likely to be on a normal trajectory when the mechanism is turned off, and thus overwhelmingly likely at that time to evolve, deterministically, into a macrocondition with the higher entropy we expect.

Now consider turning off the jumps at the initial state of the universe. Suppose we employ a means of evaluating counterfactuals such as that of David Lewis [1986], and suppose the closest possible world is one with the same macrostate. The behavior of this world will depend on which microstate it happens to be in when the GRW mechanism is removed. There are two possibilities. It might occupy an abnormal microstate, and so exhibit abnormal behavior. Or it might be in a normal microstate, and thus exhibit a law-like tendency to increase in entropy: thermodynamics will be true of this world and, moreover, may be accounted for by a Boltzmannian theory. But there is no way to determine, a priori, the likelihood of either result, for the Boltzmannian distribution over initial microstates can not be assumed to hold in this world—not until we have empirical confirmation that it does.

In arguing that nothing, not even the truth of GRW, could allow us to expect different thermodynamic behavior in the absence of an alleged cause, Price is supposing that we can never have reason to believe a system will behave other than how Boltzmannian statistical considerations tell us it will behave. This view, however, relies on a mistaken conception of the status of the probabilities which factor into the Boltzmannian account, for it assumes that the Boltzmannian measure over microstates always holds—regardless of how the world turns out to be in the counterfactual case in which there is a causal mechanism for entropy increase. Price, in other words, is assuming that the uniform distribution over the region of phase space corresponding to all the microconditions compatible with a system's macrocondition holds a priori.[7] But the uniform distribution over microstates, if it holds, is an *empirical fact* about the way our world happens to be; it is not an a priori truth which can be assumed to hold in any imagined case. And so it must be *empirically confirmed* in order for us to be justified in imposing it on a system's phase space. After all, there is no unique way of placing measures on continuously infinite sets like the set of microconditions compatible with a given macrocondition: there are (infinitely) many ways of assigning sizes to continuously infinite sets of points. It just so happens that one of these measures, the one where the size of such a set is

---

Whether thermodynamics is true in this case thus depends in the world's initial state. Price will conclude that positing GRW as the cause of thermodynamics in our world is unnecessary: all our world needs is the right kind of initial state. However, even the case in which a Boltzmannian world results when the collapse mechanism fails does not prove that GRW is irrelevant thermodynamics. All this shows is that if GRW actually causes such behavior, then in its absence something else might cause it instead; i.e., this is simply a case of causal overdetermination, and in such cases, a failure of counterfactual dependence does not mean a lack of causal relation.

(We might suppose instead that the closest world is one with the same laws. If the GRW jumps are removed at the beginning, then the law of this world will be the Schrödinger equation [plus, perhaps, the initial low-entropy macrocondition]. But this world must display abnormal thermodynamic behavior; otherwise, it would have other laws [at least on an account such as Lewis'] which, by stipulation, it does not have. Thus this case satisfies Price's counterfactual.)

My thanks to David Albert for pointing out these extra details.

[7] Price seems to want to avoid this reproach, insisting that, 'I am taking no particular stand on the nature and origins of the probabilities involved in Boltzmann's account' ([2002], p. 15). But he needs to treat the Boltzmannian

determined by the standardly-calculated volume of the region of phase space it occupies, yields the right empirical predictions. This empirical confirmation is what justifies our using this measure.

Price, we have seen, thinks we must rely on 'our epistemic probabilities' ([2002], p. 29) when evaluating the counterfactual case. So it seems that he would respond to the above criticism by saying that we should assume a uniform distribution because we do not know which microstate actually obtains and, all things being equal, we ought to assign equal probability to each possible microstate. But this response fails, for it relies on a principle of indifference according to which equipossible cases have the same probability, where equipossibility is determined via symmetry considerations based on our epistemic situation. And the principle of indifference can not be used to determine the probabilities of empirical outcomes. This is because, firstly, it assigns different probabilities to outcomes depending on the parameters with which we describe a situation. Moreover, it is entirely contingent whether the probabilities we assign on the basis of a priori symmetry considerations will match the actual frequencies with which outcomes occur. It is true that the uniform distribution seems remarkably simple or 'natural'; we might suspect that these features are what justify our imposing it on a system's phase space. But this simply is not the case. As Bas Van Fraassen has put it, 'there is no a priori reason why all [natural] phenomena should fit models with such "nice" properties only' ([1989], p. 317). Therefore, whenever the uniform distiribution does accord with the empirical phenomena, this is an a posteriori fact about the way our world happens to be.

The Boltzmannian measure over phase space, in other words, is a contingent, scientific fact which yields the right empirical predictions; the Boltzmannian probabilities are neither

---

measure as a priori in order to argue that the Generalist has no reason to expect different behavior in the absence of epistemic nor a priori. This is why, *pace* Price, it is not pointless to posit an underlying cause of entropy increase even if statistical reasoning leads us to expect such behavior: we can not rely on statistical reasoning alone in order to rule out a causal explanation of thermodynamic behavior, because a causal mechanism might turn out to be the reason *why* the behavior we observe happens to conform to such reasoning. Thus, Price is correct that there is a distinction among solutions based on whether they employ one or two time-asymmetric elements in order to account for thermodynamics; and in the end we may need only one asymmetry in order to do so. Nevertheless, whatever antecedent preference we might have for a one-asymmetry view can not on its own decide in favor of this solution; for empirical evidence might reveal a two-asymmetry account to be the correct one.

This also suggests why Price's discussion about the contrast class to thermodynamic asymmetry is misleading. It may be that the Generalist and Particularist differ as to what a world without thermodynamic asymmetry would look like. Nonetheless, antecedent considerations of what the proper contrast class is—whether it is the existence of entropy gradients which slope in both directions or no entropy gradient at all—can not motivate one kind of solution over the other without begging the question at issue. For what a world would look like if there were not the thermodynamic asymmetry we observe depends on what in fact accounts for entropy increase in our world. If, *contra* Price, there is a mechanism underlying this behavior in our world, then what would happen in its absence depends on the nature of this mechanism. Until we have a solution to the problem, that is, all we know about a thermodynamically symmetric world is that things would not behave with the law-like increase in entropy of our world; whether this

---

the proposed mechanism.

amounts to no entropy gradient or to a mixture of gradients depends on what turns out to be responsible for the asymmetry of our world.

Price acknowledges the complaint that he treats the statistical-mechanical probabilities as epistemic, thereby allowing the state of our knowledge to factor into what, it seems, should be a wholly objective scientific explanation. Against this charge, he argues that the probabilities in his account are *properly* epistemic, for they do not do any causal or explanatory work; rather, they serve to *alter what needs explaining*. He writes: '[T]here is a quite different role that probabilities can play in explanatory contexts, other than that of providing causes: viz., that of guiding our judgments as to what is "anomalous"—what calls for causal explanation, and what merely needs to be "normalised" (or given a normalising explanation […])' ([2002], p. 28). Price concludes that we do not need objective probabilities in an explanation of thermodynamic asymmetry. What is more, he says, any argument to the contrary will presuppose that we want a causal rather than a normalizing explanation, thus begging the very question at issue.

Now we can understand why Price's claim that the Boltzmannian account offers a "normalising explanation" for entropy increase seems confused. According to Price, the second law of thermodynamics is explained by noting that the natural measure, in counting the number of normal microstates as overwhelmingly large, renders entropy-increasing behavior overwhelmingly statistically likely. Price thus argues *from* the 'natural epistemic measure' *to* the conclusion that entropy-increasing behavior is 'typical' and so not in need of explanation. But the reason we use this 'natural measure' in the first place is *precisely that* it counts the behavior we observe as overwhelmingly likely or 'typical.' As we've seen, these statistical considerations are not justified on a priori or epistemic grounds. We impose the natural distribution, then, simply *because* it renders the thermodynamic features of our experience overwhelmingly probable; hence our experience of macroscopic systems suggests that this is how microconditions are actually distributed in nature. Thus, we *begin with* the empirical data of entropy increase and then use this to justify our imposing the uniform distribution on a system's phase space. Price's claim that we have a normalizing explanation for entropy increase thus seems to amount to no more than the claim that the natural measure over microstates is empirically confirmed by our entropy-increasing experience.

More generally, Price suggests that the only phenomena science needs to explain are the statistically anomalous ones, since statistically normal phenomena are explained via statistical considerations alone. On his view, therefore, it does not make sense to ask, 'Why does a gas expand to fill its container?', given that (allegedly) a priori statistical reasoning has shown us to expect such behavior. *Contra* Price, however, *all* empirical phenomena—even the statistically normal ones—call for explanation, as far as science is concerned. Surely science aims—and surely it *should* aim—to explain the frequently occurring phenomena just as much as it tries to explain the seemingly anomalous ones. The fact that entropy increase isn't statistically extraordinary, e.g., is an *empirical fact* about the world and, accordingly, is something which science should try to explain—and *does* try to explain, via the Boltzmannian account. Price's suggestion that things require explanation only if they fail to exhibit statistically probable behavior is therefore misleading, for we can not determine what something's natural condition is in the absence of an empirical theory which, in turn, serves as a scientific explanation for that condition's being considered the natural one. Thus, a gas' expanding to fill its container is to be expected only given the empirically confirmed theory according to which the microstates of thermodynamic systems are distributed in the way the Boltzmannian account says. Of course, in any account there must be some brute facts which do not require explanation in the way that other facts which depend on the primitive ones do. But the fact that thermodynamic systems behave with a law-like entropy-increasing tendency despite the symmetry of the underlying laws is surely something calling for explanation—as is *any* process or behavior we observe. Indeed, the only thing for which science may not offer some sort of explanation, I would suggest, is the fact that certain initial conditions obtain or that certain fundamental laws happen to hold in our world. (Price disagrees; I will return to this shortly.)

Price will think I am begging the question in assuming there is a law-like generalization to account for in the first place. For him, the law-like character of what we observe is in dispute. Indeed, the distinction he draws between one- and two-asymmetry views lies precisely in whether the view says there is a law-like asymmetry which needs explaining. (Hence his suggestion that the requisite counterfactual is not whether there would be an entropy-increasing *tendency* in the absence of the proposed cause, but whether a system *would* increase in entropy.)

But this seems to misunderstand the puzzle which the Boltzmannian account is trying to solve. This solution does not deny that there is a law-like asymmetry to our experience; it simply denies that there is any asymmetric dynamical mechanism which gives rise to that experience. The problem, after all, is how to account for the widespread asymmetry of our experience in a world apparently governed by time-reversal invariant laws. So we begin with the observed tendency of entropy to increase as an empirical feature of our experience, and thus as something which a scientific account should try to explain. The Boltzmannian solution then says that the observed asymmetry results from asymmetric boundary conditions—not that there is no apparent asymmetry which needs explaining.

Price's reason for thinking the entropy-increasing generalization is not law-like is that it is not projectible. As he notes, the Boltzmannian account can succeed only on the assumption that entropy was extremely low at the initial state of the universe. This, plus the fact that the Boltzmannian probabilities don't preclude a low-entropy future boundary condition—since they do not preclude such a past condition—leads Price to conclude that we do not have reason to be confident that entropy will continue to increase until we understand more about the low-entropy past. In Price's terms, the probabilities of the Boltzmannian account have a 'deferential status' towards the past, leaving open the possibility that they might be similarly 'trumped' in the future. Therefore, the most we can say in forming a generalization about our thermodynamic experience is that entropy is likely to be high, *ceteris paribus*.

Yet there seems to me to be pretty good reason to retain confidence in the belief that entropy will continue to increase. First of all, a time-asymmetric theory such as GRW *does* rule out a future low-entropy boundary condition. If GRW turns out to be the true theory of our world, then, we *would* have good reason to expect entropy to continue to increase. What is more, this reason is independent of our understanding anything about the past. (Indeed, GRW by itself does not tell us *anything* about the past.)[8]

Even if GRW turns out not to be a true theory, Boltzmannian statistical considerations plus symmetric dynamical laws give us good reason to expect that entropy will increase in the future, for they tell us that this is what will happen with overwhelmingly high probability. Indeed, when combined with time-reversal invariant laws, Boltzmannian statistical considerations *also* render it overwhelmingly probable that entropy has increased towards the *past*: hence the need for a low-entropy initial boundary condition. Thus, it is not as though the past low-entropy condition overrides the claims these considerations yield about the past, so that

---

[8] Price maintains that 'we are not justified in postulating such a [time-asymmetric] law, unless we have independent reason for excluding the possibility of a low entropy future' ([2002], p. 53 n. 21). But surely experimental evidence

we can not trust their predictions about the future until we know whether they are similarly overridden in that direction.[9] Rather, the past low-entropy condition serves to *correct* the empirically *disconfirmed* claims they make about the past. Therefore, in the absence of any empirical evidence to suggest that the Boltzmannian statistical claims about the future are incorrect—and given the strong empirical evidence in favor this account—we have good reason to rely on its predictions. It is true that the Boltzmannian story does not preclude the possibility of a low-entropy future boundary condition, and if we ever do obtain evidence of such a condition, we would have to alter our theory accordingly. But this does not disrupt our overwhelmingly good grounds for thinking that entropy will continue to increase given the confirmation of those accounts which tell us that this is what will happen. And these grounds are independent of what we know about the low-entropy past.

Lastly, I want to briefly comment on Price's view that the low-entropy initial state of the universe requires explanation because it is so statistically improbable. Such an explanation, he says, would yield "'some sort of law-like narrowing of the space of possibilities, so that such a universe no longer counts as abnormal' ([2002], p. 41). Price thinks this is all we need in order to explain the asymmetry of thermodynamics in general.

This understanding of the puzzle, however, stems from Price's misconception of the status of the Boltzmannian probability measure: for him, the reason the second law of

---

of the truth of GRW would justify our postulating it as the theory of our world, in which case we would be justified in trusting its predictions.

[9] Indeed, the entire notion of probabilities which are trumped' in one direction of time is strange. Price seems to think these probabilities are still out there in the world in the backwards direction of time even though they are only ever manifested in observable frequencies in the forwards direction. But surely any probabilities which appear in the fundamental laws must have *something* to do with the actual frequencies with which outcomes are observed to occur: if certain probabilities do not have anything to do with how things actually happen in the world, then they can not play any role in explaining its behavior, and so ought not to appear in its fundamental laws. Thus, if we do not see any law-like entropy-increasing behavior in the backwards direction of time, we should conclude that there are

thermodynamics does not require explanation is that supposedly a priori statistical considerations provide all the explanation we need. This alone suffices to show that the puzzle about the time-asymmetry of thermodynamics doesn't reduce to that of explaining the initial condition of the universe. Yet there are a few more considerations which reinforce the suggestion that Price's aim is off the mark. Firstly, it is not as clear as Price seems to think that global initial conditions can be explained in the way that he wants. For Price, such an explanation would render initial smoothness statistically unexceptional or 'normal.' Given the aforementioned difficulties with the entire notion of a "'normalizing explanation' of empirical phenomena, however, it remains unclear whether Price can achieve the explanation he seeks. Indeed, according to some views, the initial low-entropy state should be regarded as a law of nature. If correct, this would counter Price's understanding of the time-asymmetric explanandum of thermodynamics, since granting the initial condition law-like status is tantamount to saying that it can not be further explained.

Second, Price has not said enough in arguing against those who insist that global initial conditions can not be explained. He writes that,

> [T]he proponent of [the] '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. 39).

It is unclear, however, that there is an unjustified temporal bias at work here. Craig Callender, for one, has suggested that the prevalence of successful explanations of events by appeal to laws plus earlier conditions might justify this temporal aspect of our explanations ([1998], pp. 151-152). If so, then the difference in temporal orientation could be a *legitimate* reason to conclude that initial conditions are special when it comes to scientific explanation. Indeed, a general

---

no chances governing that behavior in this direction, not that they are still present but overridden. If we *only* ever see the effect of such chances in one time direction, what reason could we have for concluding they exist in the other?

difference in explanatory standards with respect to the past and the future could be justified by the scientific theory of our world. GRW, for example, yields probabilistic predictions towards the future but no such claims about the past; so that if GRW is true, scientific explanations towards the future would differ from those towards the past in a way which does not rely on the kind of anthropocentric temporal bias Price criticizes. (More generally, this kind of difference might be justified by the fact that our world has a low-entropy initial condition but no similar future condition.)

Finally, even if we grant Price the prospect of explaining initial conditions in the way that he wants, it strikes me that his demand ought to lead him to prefer a proposal such as Albert's. On the GRW theory, entropy increase is overwhelmingly likely for *any* microstate compatible with the universe's initial low-entropy macrostate. Albert's account thus yields the observed entropy-increasing tendency for a wider range of initial conditions than the traditional Boltzmannian account does. Thus, on the GRW proposal, it becomes less unlikely—less seemingly accidental—that the initial conditions of the universe were such as to generate behavior in accord with the second law of thermodynamics.

Conclusion

Let us recall the problem we started with. We began by wondering why it is the case that, in a world supposedly governed by time-reversal invariant laws, there is such a widespread time-asymmetry to our experience. The traditional approach assumes asymmetric boundary conditions. The Causal-General approach, on the other hand, says that entropy never decreases because of some underlying causal mechanism.

Price thinks both these accounts miss the point, insofar as the time-asymmetry of thermodyanmics is concerned, since what needs to be explained is not why entropy increases but why entropy was low to begin with. As we have seen, however, Price reaches this understanding of the problem on the basis of a misconception of the nature of the statistical-mechanical probabilities and of what needs to be explained in any scientific account of the world. Moreover, his puzzle is different from the one we started out with—a puzzle which is answered quite nicely by a Causal-General solution such as Albert's on which thermodynamics results from a fundamental dynamical law. As they stand, therefore, both the (correctly understood) Boltzmannian solution and the Causal-General approach fail to answer Price's problem. But explaining the initial condition of the universe in the way that Price wants is neither necessary nor sufficient for explaining the time-asymmetry of thermodynamics. And since entropy increase *is* something that calls for explanation, it seems that Price is the one who misconceives the problem.

Acknowledgements

I am very grateful to David Albert for helpful discussion and comments on earlier versions of this paper.

References

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

Callender, C. [1998]: "The View from No-When", *The British Journal for the Philosophy of Science*, 49, pp. 135-159.

Lewis, D. [1986]: "Counterfactual Dependence and Time's Arrow", in his *Philosophical Papers: Volume II*, Oxford: Oxford University Press, pp. 32-52. Originally printed in *Noûs*, 13, pp. 455-76.

Price, H. [2002]: “Boltzmann’s Time Bomb”, *The British Journal for the Philosophy of Science*, 53 [pp.].

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

Van Fraassen, B. C. [1989]: *Laws and Symmetry*, New York: Oxford University Press.