

Time's Arrow, Time's Fly-Bottle

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For more than a century, physics has known of a puzzling conflict between the time-asymmetry of thermodynamic phenomena and the time-symmetry of the underlying microphysics on which these phenomena depend. In the spirit of “philosophy as therapy”, this paper examines the current status of this puzzle, distinguishing the central issue from various issues with which it is commonly confused. In particular, I argue that there are two competing conceptions of what is needed to resolve the puzzle of the thermodynamic asymmetry, which differ with respect to the number of distinct T-asymmetries they take to be manifest in the physical world. On the preferable one-asymmetry conception, the remaining puzzle concerns the ordered distribution of matter in the early universe. The puzzle of the thermodynamic arrow thus becomes a puzzle for cosmology.

1. The puzzle of temporal bias

Late in the nineteenth century, on the shoulders of Maxwell, Boltzmann and many lesser giants, physicists saw that there is a deep puzzle behind the familiar phenomena described by the new science of thermodynamics. On the one hand, many such phenomena show a striking temporal bias. They are common in one temporal orientation, but rare or non-existent in reverse. On the other hand, the underlying laws of mechanics show no such temporal preference. If they allow a process in one direction, they also allow its temporal mirror image. Hence the puzzle: if the laws are so even-handed, why are the phenomena themselves so one-sided?

What has happened to this puzzle since the 1890s? Many contemporary physicists appear to regard it as a dead issue, long since laid to rest. Didn't it turn out to be just a matter of statistics, after all? However, while there are certainly would-be solutions on offer—if anything, too many of them—it is far from clear that the puzzle has actually been solved. Late in the twentieth century, in fact, one of the most authoritative writers on the conceptual foundations of statistical mechanics could still refer to an understanding of

the time-asymmetry of thermodynamics as ‘that obscure object of desire’. (Sklar 1995)

One of the obstacles to declaring the problem solved is that there are several distinct approaches, not obviously compatible with one another. Which of these, if any, is supposed to be *the* solution, now in our grasp? Even more interestingly, it turns out that not all these would-be solutions are answers to the same question. There are different and incompatible conceptions in the literature of what the puzzle of the thermodynamic asymmetry actually is—about what *exactly* we should be trying explain, when we try to explain the thermodynamic arrow of time.

What the problem needs is what philosophers do for a living: drawing fine distinctions, sorting out ambiguities, and clarifying the logical structure of difficult and subtle issues. In this paper, my aim here is to bring these methods to bear on the puzzle of the time-asymmetry of thermodynamics. In particular, I want to distinguish the true puzzle from some of the appealing false trails, and hence to make it clear where physics stands in its attempt to solve it.¹

What does this have to do with Wittgenstein? Mainly that it provides an example of “philosophy as therapy”. To put it in terms of Wittgenstein’s famous metaphor, the goal is to free a fly who has been trapped for more than a century, in a fly-bottle constituted by the strange time-asymmetry of the second law of thermodynamics. True, Wittgenstein had in mind the fly-bottles in which philosophers entrap themselves, whereas our present prisoners are physicists, as much as philosophers. And the result of the exercise does not seem to be that the “problems ... *completely* disappear” (*PI*, §133)—some genuine puzzles remain, in my view. Nevertheless, as we’ll see, this is certainly a case in which philosophy is therapeutic, helping to purge us of pseudo-puzzles that arise, at least in part, from bad habits of thought.

2. The true puzzle—a first approximation and a popular challenge

Everyone agrees, I think, that the puzzle of the thermodynamic arrow stems from the conjunction of two facts (or apparent facts—one way to dissolve

1 The present treatment is brief, but I discuss these topics at greater length elsewhere—see Price (1996, 2002a, 2002b, 2004).

the puzzle would be to show that one or other of the following claims isn't actually true):

1. There are many common and familiar physical processes, collectively describable as cases in which entropy is increasing, whose corresponding time-reversed processes are unknown or at least very rare.
2. The dynamical laws governing such processes show no such T-asymmetry—if they permit a process to occur with one temporal orientation, they permit it to occur with the reverse orientation.

As noted, some people will be inclined to object at this point that the conjunction is *merely* apparent. In particular, it may be objected that we now know that the dynamical laws are not time-symmetric. Famously, T-symmetry is violated in weak interactions, by the neutral *K* meson. Doesn't this eliminate the puzzle?

No. If the time-asymmetry of thermodynamics were associated with the T-symmetry violation displayed by the neutral *K* meson, then anti-matter would show the reverse of the normal thermodynamic asymmetry. Why? Because PCT-symmetry guarantees that if we replace matter by anti-matter (i.e., reverse P and C) and then view the result in reverse time (i.e., reverse T), physics remains the same. So if we replaced matter by anti-matter but didn't reverse time, any intrinsic temporal arrow or T-symmetry violation would reverse its apparent direction. In other words, physicists in anti-matter galaxies find the opposite violations of T-symmetry in weak interactions to those found in our galaxy. So if the thermodynamic arrow were tied to the T-symmetry violation, it too would have to reverse under such a transformation.

But now we have both an apparent falsehood, and a paradox. There's an apparent falsehood because (of course) we don't think that anti-matter behaves anti-thermodynamically. We expect stars in anti-matter galaxies to radiate just like our own sun (as the very idea of an anti-matter galaxy requires, in fact). And there's a paradox, because if this were the right story, what would happen to particles which are their own anti-particles, such as photons? They would have to behave both thermodynamically and anti-thermodynamically!

Here's another way to put the point. The thermodynamic arrow isn't just a T-asymmetry, it is a PCT-asymmetry as well. There are many familiar processes whose PCT-reversed processes are equally compatible with the underlying

ing laws, but which never happen, in our experience. We might be tempted to explain this asymmetry as due to the imbalance between matter and anti-matter, but the above reflections show that this is not so. So instead of the puzzle of the T-asymmetry of thermodynamics, we could speak of the puzzle of the PCT-asymmetry of thermodynamics. Then it would be clear to all that the strange behaviour of the neutral K meson isn't relevant. Knowing that we could if necessary rephrase the problem in this way, we can safely rely on the simpler formulation, and return to our original version of the puzzle.

3. Four things the puzzle is not

Some of the confusions common in debates about the origins of the thermodynamic asymmetry can be avoided distinguishing the genuine puzzle from various pseudo-puzzles with which it is liable to be confused. In this section I'll draw four distinctions of this kind.

The meaning of irreversibility

The thermodynamic arrow is often described in terms of the 'irreversibility' of many common processes—e.g., of what happens when a gas disperses from a pressurised bottle. This makes it sound as if the problem is that we can't make the gas behave in the opposite way—we can't make it put itself back into the bottle. Famously, Loschmidt's reversibility objection rested on pointing out that the reverse motion is equally compatible with the laws of mechanics. Some responses to this problem (e.g., Ridderbos and Redhead 1998) concentrate on the issue as to why we can't actually reverse the motions (at least in most cases).

This response misses the interesting point, however. The interesting issue turns on a numerical imbalance in nature between 'forward' and 'reverse' processes, not case-by-case irreversibility of individual processes. Consider a parity analogy. Imagine a world containing many left hands but few right hands. Such a world shows an interesting parity asymmetry, even if any individual left hand can easily be transformed into a right hand. Conversely, a world with equal numbers of left and right hands is not interestingly P-asymmetric, even if any individual left or right hand cannot be reversed. Thus the interesting issue concerns the numerical asymmetry between the two kinds of structures—here, left hands and right hands—not the question whether one can be transformed into the other.

Similarly in the thermodynamic case, in my view. The important thing to explain is the numerical imbalance in nature between entropy-increasing processes and their T-reversed counterparts, not the practical irreversibility of individual processes.

Asymmetry in time versus asymmetry of time

Writers on the thermodynamic asymmetry often write as if the problem of explaining this asymmetry is the problem of explaining 'the direction of time'. This may be a harmless way of speaking, but we should keep in mind that the real puzzle concerns the asymmetry of physical processes *in* time, not an asymmetry *of time itself*. By analogy, imagine a long narrow room, architecturally symmetrical end-to-end. Now suppose all the chairs in the room are facing the same end. Then there's a puzzle about the asymmetry in the arrangement of the chairs, but not a puzzle about the asymmetry of the room. Similarly, the thermodynamic asymmetry is an asymmetry of the 'contents' of time, not an asymmetry of the container itself.

It may be helpful to make a few remarks about the phrase 'direction of time'. Although this expression is in common use, it isn't at all clear what it could actually mean, if we try to take it literally. Often the thought seems to be that there is an objective sense in which one time direction is future (or 'positive'), and the other past (or 'negative'). But what could this distinction amount to? It's easy enough to make sense of the idea that time is *anisotropic*—i.e., different in one direction than in the other. For example, time might be finite in one direction but infinite in the other. But this isn't enough to give a *direction* to time, in the above sense. After all, if one direction were objectively the future or positive direction, then in the case of a universe finite at one end, there would be two possibilities. Time might be finite in the past, and or finite in the future. So anisotropy alone doesn't give us *direction*.

Similarly, it seems, for any other physical time-asymmetry to which we might appeal. If time did have a direction—an objective basis for a privileged notion of positive or future time—then for any physical arrow or asymmetry in time, there would always be a question as to whether that arrow pointed forwards or backwards. And so no physical fact could answer this question, because for any candidate, the same issue arises all over again. Thus the idea that time has a real direction seems without any physical meaning. (Of course, we can use any asymmetry we like as a basis for a conventional labelling—saying, for example, that we'll regard the

direction in which entropy is increasing as the positive direction of time. But this is different from discovering some intrinsic directionality to time itself.)

For present purposes, then, I'll assume that it is a conventional matter which direction we treat as positive or future time. Moreover, although it makes sense to ask whether time is anisotropic, it seems clear that this is a different issue from that of the thermodynamic asymmetry. As noted, the thermodynamic asymmetry is an asymmetry of physical processes *in* time, not an asymmetry of time itself.

Entropy gradient not entropy increase

If it is conventional which direction counts as positive time, then it is also conventional whether entropy increases or decreases. It increases by the lights of the usual convention, but decreases if we reverse the labelling. But this may seem ridiculous. Doesn't it imply, absurdly, that the thermodynamic asymmetry is merely conventional?

No. The crucial point is that while it's a conventional matter whether the entropy gradient slopes up or down, the gradient itself is objective. The puzzling asymmetry is that the gradient is monotonic—it slopes in the same direction everywhere (so far as we know).

It is worth noting that in principle there are two possible ways of contrasting this monotonic gradient with a symmetric world. One contrast would be with a world in which there are entropy gradients, but sometimes in one direction and sometimes in the other—i.e., worlds in which entropy sometimes goes up and sometimes goes down. The other contrast would be with worlds in which there are no significant gradients, because entropy is always high. If we manage to explain the asymmetric gradient we find in our world, we'll be explaining why the world isn't symmetric in one of these ways—but which one? The answer isn't obvious in advance, but hopefully will fall out of a deeper understanding of the nature of the problem.

The term 'entropy' is inessential

A lot of time and ink has been devoted to the question how entropy should be defined, or whether it can be defined at all in certain cases (e.g., for the universe as a whole). It would be easy to get the impression that the puzzle of the thermodynamic asymmetry depends on all this discussion—that whether there's really a puzzle depends on how, and whether, entropy can be defined, perhaps.

But in one important sense, these issues are beside the point. We can see that there's a puzzle, and go a long way towards saying what it is, without ever mentioning entropy. We simply need to describe in other terms some of the many processes which show the asymmetry—which occur with one temporal orientation but not the other. For example, we can point out that there are lots of cases of big difference in temperatures spontaneously equalising, but none of big differences in temperature spontaneously arising. Or we can point out that there are lots of cases of pressurised gas spontaneously leaving a bottle, but none of gas spontaneously pressurising by entering a bottle. And so on.

In the end, we may need the notion of entropy to generalise properly over these cases. However, we don't need it to see that there's a puzzle—to see that there's a striking imbalance in nature between systems with one orientation and systems with the reverse orientation. For present purposes, then, we can ignore objections based on problems in defining entropy. (Having said that, of course, we can go on using the term entropy with a clear conscience, without worrying about how it's defined. In what follows, talk of entropy increase is just a placeholder for a list of the actual phenomena that display the asymmetry we're interested in.)

Summary

For the remainder of the paper, then, I take it (i) that the asymmetry in nature is a matter of numerical imbalance between temporal mirror images, not of literal reversibility; (ii) that we are concerned with an asymmetry of physical processes in time, not with an asymmetry in time itself; (iii) that the objective asymmetry concerned is a monotonic *gradient*, rather than an increase or a decrease; and (iv) that if need be the term 'entropy' is to be thought of as a placeholder for the relevant properties of a list of actual physical asymmetries.

4. What would a solution look like? Two models

With our target more clearly in view, I now want to call attention to what may be the most useful distinction of all, in making sense of the many things that physicists and philosophers say about the thermodynamic asymmetry. This is a distinction between two very different conceptions of *what it would take* to explain the asymmetry—so different, in fact, that they disagree on *how many* distinct violations of T-symmetry it takes to explain the observed

asymmetry. On one conception, an explanation needs two T-asymmetries. On the other conception, it needs only one.

Despite this deep difference of opinion about what a solution would look like, the distinction between these two approaches is hardly ever noted in the literature—even by philosophers, who are supposed to have a nose for these things. So it is easy for advocates of the different approaches to fail to see that they are talking at cross-purposes—that in one important sense, they disagree about what the problem is.

The two-asymmetry approach

Many approaches to the thermodynamic asymmetry look for a dynamical explanation of the second law—a dynamical cause or factor, responsible for entropy increase. Here are some examples, old and new:

1. *The H-theorem*. Oldest and most famous of all, this is Boltzmann's development of Maxwell's idea that intermolecular collisions drive gases towards equilibrium.
2. *Interventionism*. This alternative to the *H*-theorem, apparently first proposed by S. H. Burbury in the 1890s (Burbury 1894, 1895), attributes entropy increase to the effects of random and uncontrollable influences from a system's external environment.
3. *Indeterministic dynamics*. There are various attempts to show how an indeterministic dynamics might account for the second law. A recent example (Albert 1994, 2000) is a proposal that the stochastic collapse mechanism of the GRW approach to quantum theory might also explain entropy increase.

I stress two points about these approaches. First, if there is something dynamical which makes entropy increase, then it needs to be time-asymmetric. Why? Because otherwise it would force entropy to increase (or at least not to decrease) in both directions—in other words, entropy would be constant. In the *H*-theorem, for example, this asymmetry resides in the assumption of molecular chaos. In interventionism, it is provided by the assumption that incoming influences from the environment are 'random', or uncorrelated with the system's internal dynamical variables.

The second point to be stressed is that this asymmetry alone isn't sufficient to produce the observed thermodynamic phenomena. Something which forces entropy to be non-decreasing won't produce an entropy gra-

dient unless entropy starts low. To give us the observed gradient, in other words, this approach also needs a low entropy boundary condition—entropy has to be low in the past. This condition, too, is time-asymmetric, and it's a separate condition from the dynamical asymmetry. (It is not guaranteed by the assumption of molecular chaos, for example.)

So this approach is committed to the claim that it takes *two* T-asymmetries—one in the dynamics, and one in the boundary conditions—to explain the observed asymmetry of thermodynamic phenomena. If this model is correct, explanation of the observed asymmetry needs an explanation of both contributing asymmetries, and the puzzle of the thermodynamic arrow has become a double puzzle.

The one-asymmetry model

The two-asymmetry model isn't the only model on offer, however. The main alternative was first proposed by Boltzmann in the 1870s (Boltzmann 1877), in response to Loschmidt's famous criticism of the *H*-theorem. To illustrate the new approach, think of a large collection of gas molecules, isolated in a box with elastic walls. If the motion of the molecules is governed by deterministic laws, such as Newtonian mechanics, a specification of the microstate of the system at any one time uniquely determines its entire trajectory. The key idea of Boltzmann's new approach is that in the overwhelming majority of possible trajectories, the system spends the overwhelming majority of the time in a high entropy macrostate—among other things, a state in which the gas is dispersed throughout the container. (Part of Boltzmann's achievement was to find the appropriate way of counting possibilities, which we can call the *Boltzmann measure*.)

Importantly, there is no temporal bias in this set of possible trajectories. Each possible trajectory is matched by its time-reversed twin, just as Loschmidt had pointed out, and the Boltzmann measure respects this symmetry. Asymmetry arises only when we apply a low entropy condition at one end. For example, suppose we stipulate that the gas is confined to some small region at the initial time t_0 . Restricted to the remaining trajectories, the Boltzmann measure now provides a measure of the likelihood of the various possibilities consistent with this boundary condition. Almost all trajectories in this remaining set will be such that the gas disperses after t_0 . The observed behaviour is thus predicted by the time-symmetric measure, once we conditionalise on the low entropy condition at t_0 .

On this view, then, there's no time-asymmetric factor which causes

entropy to increase. This is simply the most likely thing to happen, given the combination of the time-symmetric Boltzmann probabilities and the single low entropy restriction in the past. More below on the nature and origins of this low entropy boundary condition. For the moment, the important thing is that although it is time-asymmetric, so far as we know, this is the only time-asymmetry in play, according to Boltzmann's statistical approach. There's no need for a second asymmetry in the dynamics.

5. Which is the right model?

It is important to distinguish these two models, but it would be even more useful to know which of them is right. How many time-asymmetries should we be looking for, in trying to account for the thermodynamic asymmetry? This is a big topic, but I'll mention two factors, both of which seem to me to count in favour of the one-asymmetry model.

The first factor is simplicity, or theoretical economy. If the one-asymmetry approach works, it simply does more with less. In particular, it leaves us with only one time-asymmetry to explain. True, this would not be persuasive if the two-asymmetry approach actually achieved more than the one-asymmetry approach—if the former had some big theoretical advantage that the latter lacked. But the second argument I want to mention suggests that this can't be the case. On the contrary, the second asymmetry seems redundant.

Redundancy is a strong charge, but consider the facts. The two-asymmetry approach tries to identify some dynamical factor (collisions, or external influences, or whatever) that causes entropy to increase—that makes a pressurised gas leave a bottle, for example. However, to claim that one of these factors *causes* the gas to disperse is to make the following 'counterfactual' claim: *If the factor were absent, the gas would not disperse* (or would do so at a different rate, perhaps). But how could the absence of collisions or external influences *prevent* the gas molecules from leaving the bottle?

Here's a way to make this more precise. In the terminology of Boltzmann's statistical approach, we can distinguish between *normal* initial microstates (for a system, or for the universe as a whole), which lead to entropy increases much as we observe, and *abnormal* microstates, which are such that something else happens. The statistical approach rests on the fact that normal microstates are vastly more likely than abnormal microstates, according to the Boltzmann measure.

In these terms, the above point goes as follows. The two-asymmetry approach is committed to the claim that the universe begins in an abnormal microstate. Why? Because in the case of normal initial microstates, entropy increases anyway, without the mechanism in question—so the required counterfactual claim isn't true.

It is hard to see what could justify this claim about the initial microstate. At a more local level, why should we think that the initial microstate of a gas sample in an open bottle is normally such that if it weren't for collisions (or external influences, or whatever), the molecules simply wouldn't encounter the open top of the bottle, and hence disperse?

Thus it is doubtful whether there is really any need for a dynamical asymmetry, and the one-asymmetry model seems to offer the better conception of what it would take to solve the puzzle of the thermodynamic asymmetry. But if so, then the various two-asymmetry approaches—including Boltzmann's own *H*-theorem, which he himself defended in the 1890s, long after he first proposed the statistical approach—are looking for a solution to the puzzle in the wrong place, at least in part.

For present purposes, the main conclusion I want to emphasise is that we need to make a choice. The one-asymmetry model and the two-asymmetry model represent two very different views of *what it would take* to explain the thermodynamic arrow—of what the problem is, in effect. Unless we notice that they are different approaches, and proceed to agree on which of them we ought to adopt, we can't possibly agree on whether the old puzzle has been laid to rest.

6. The Boltzmann-Schuetz hypothesis—a no-asymmetry solution?

If the one-asymmetry view is correct, the puzzle of the thermodynamic arrow is really the puzzle of the low entropy boundary condition. Why is entropy so low in the past? After all, in making it unmysterious why entropy doesn't decrease in one direction, the Boltzmann measure equally makes it mysterious why it does decrease in the other—for the statistics themselves are time-symmetric.

Boltzmann himself was one of the first to see the importance of this issue. In a letter to *Nature* in 1895, he suggests an explanation, based on an idea he attributes to 'my old assistant, Dr Schuetz' (Boltzmann 1895). He notes that although low entropy states are very unlikely, they are very likely to

occur eventually, given enough time. If the universe is very old, it will have had time to produce the kind of low entropy region we find ourselves inhabiting simply by accident. “Assuming the universe great enough, the probability that such a small part of it as our world should be in its present state, is no longer small”, as Boltzmann puts it.

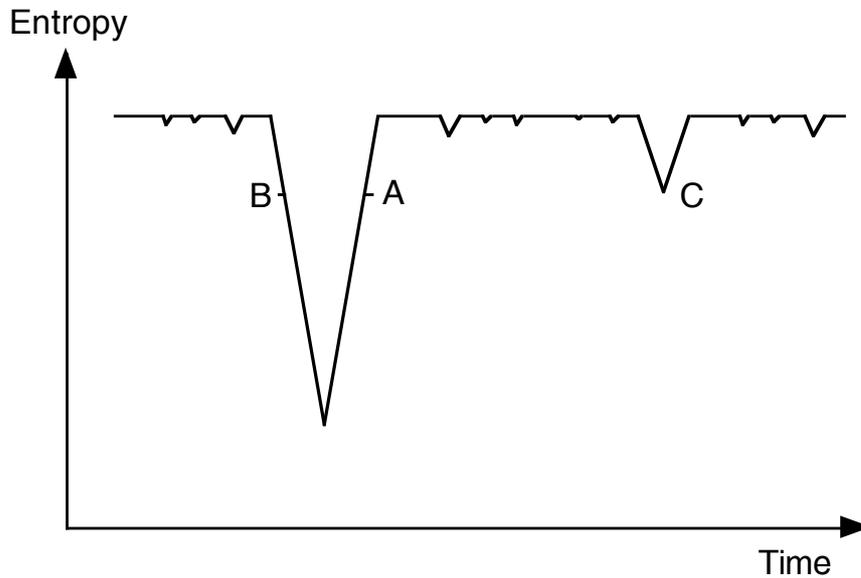


Figure 1: *Boltzmann's entropy curve*

It is one thing to explain why the universe contains regions like ours, another to explain why we find ourselves in such a region. If they are so rare, isn't it more likely that we'd find ourselves somewhere else? But Boltzmann suggests an answer to this, too. Suppose, as seems plausible, that creatures like us couldn't exist in the vast regions of near-equilibrium between such regions of low entropy. Then it's no surprise that we find ourselves in such an unlikely place. As Boltzmann himself puts it, “the ... H curve would form a representation of what takes place in the universe. The summits of the curve would represent the worlds where visible motion and life exist”.

Figure 1 shows what Boltzmann calls the H curve, except that this diagram plots entropy rather than Boltzmann's quantity H . Entropy is low when H is high, so the summits of Boltzmann's H curve are the troughs of the entropy curve. The universe spends most of its time very close to equilibrium. But occasionally—much more rarely than this diagram actually suggests—a random re-arrangement of matter produces a state of low entropy. As the resulting state returns to equilibrium, there's an entropy slope, such as the one on which we (apparently) find ourselves, at a point such as A.

Why do we find ourselves on an uphill rather than a downhill slope, as at B? In another paper (Boltzmann 1897), Boltzmann offers a remarkable proposal to explain this, too. Perhaps our perception of past and future depends on the entropy gradient, in such a way that we are bound to regard the future as lying 'uphill'. Thus the perceived direction of time would not be objective, but a product of our own orientation in time. Creatures at point B would see the future as lying in the other direction, and there's no objective sense in which they are wrong and we are right, or vice versa. Boltzmann compares this to the discovery that spatial up and down are not absolute directions, the same for all observers everywhere.

For present purposes, what matters about the Boltzmann-Schuetz hypothesis is that it offers an explanation of the local asymmetry of thermodynamics in terms which are symmetric on a larger scale. So it is a no-asymmetry solution—the puzzle of the thermodynamic asymmetry simply vanishes on the large scale.

7. The big problem

Unfortunately, however, this clever proposal has a sting in its tail, a sting so serious that it now seems almost impossible to take the hypothesis seriously. The problem flows directly from Boltzmann's own link between entropy and probability. In Figure 1, the vertical axis is a logarithmic probability scale. For every downward increment, dips in the curve of the corresponding depth are exponentially more improbable. So a dip of the depth of point A or point B is much more likely to occur in the form shown at point C—where the given depth is very close to the minimum of the fluctuation—than in association with a much bigger dip, as at A and B. Hence if our own region has a past of even lower entropy, it is much more improbable than it needs to be, given its present entropy. So far, this point seems to have been appreciated already in the 1890s, in exchanges between Boltzmann and Zermelo. What doesn't seem to have been appreciated is its devastating consequence, namely, that according to the Boltzmann measure it is much easier to produce fake records and memories, than to produce the real events of which they purport to be records.

Why does this consequence follow? Well, imagine that the universe is vast enough to contain many separate fluctuations, each containing everything that we see around us, including the complete works of Shakespeare, in all their twenty-first century editions. Now imagine choosing one of these fluc-

tuations at random. It is vastly more likely that we'll select a case in which the Shakespearean texts are a product of a spontaneous recent fluctuation, than one in which they were really written four hundred years earlier by a poet called William Shakespeare. Why? Simply because entropy is much higher now than it was in the sixteenth century (as we normally assume that century to have been). Recall that according to Boltzmann, probability increases exponentially with entropy. Fluctuations like our twenty-first century—'Shakespearean' texts and all—thus occur much more often in typical world-histories than fluctuations like the lower-entropy sixteenth century. So almost all fluctuations including the former don't include the latter. The same goes for the rest of history—all our 'records' and 'memories' are almost certainly misleading.

To make this conclusion vivid we can take advantage of the fact that in the Boltzmann picture, there isn't an objective direction of time. So we can equally well think about the question of 'what it takes' to produce what we see around us from the reverse of the normal temporal perspective. Think of starting in what we call the future, and moving in the direction we call towards the past. Think of all the apparently miraculous accidents it takes to produce the kind of world we see around us. Among other things, our bodies themselves, and our editions of Shakespeare, have to 'undecompose', at random, from (what we normally think of as) their future decay products. That's obviously extremely unlikely, but the fact that we're here shows that it happens. But now think of what it takes to get even further back, to a sixteenth century containing Shakespeare himself. The same kind of near-miracle needs to happen many more times. Among other things, there are several billion intervening humans to 'undecompose' spontaneously from dust.

So the Boltzmann-Schuetz hypothesis implies that our apparent historical evidence is almost certainly unreliable. So far as I know, this point was first made in print by von Weizsäcker (1939). Von Weizsäcker notes that "improbable states can count as documents [i.e., records of the past] only if we presuppose that still less probable states preceded them". He concludes that "the most probable situation by far would be that the present moment represents the entropy minimum, while the past, which we infer from the available documents, is an illusion".

Von Weizsäcker also notes that there's another problem of a similar kind. The Boltzmann-Schuetz hypothesis implies that as we look further out into space, we should expect to find no more order than we already have reason to

believe in. But we can now observe vastly more of the universe than was possible in Boltzmann's day, and there seems to be low entropy all the way out.

So the Boltzmann-Schuetz hypothesis faces some profound objections. Fortunately, as we're about to see, modern cosmology goes at least some way to providing us with an alternative.

8. Initial smoothness

We have seen that the observed thermodynamic asymmetry requires that entropy was low in the past. Low entropy requires concentrations of energy in useable forms, and presumably there are many ways such concentrations could exist in the universe. On the face of it, we seem to have no reason to expect any particularly neat or simple story about how it works in the real world—about where the particular concentrations of energy we depend on happen to originate. Remarkably, however, modern cosmology suggests that all the observed low entropy is associated with a single characteristic of the early universe, soon after the big bang. The crucial thing is that matter is distributed extremely smoothly in the early universe. This provides a vast reservoir of low entropy, on which everything else depends. In particular, smoothness is necessary for galaxy and star formation, and most familiar irreversible phenomena depend on the sun.

Why does a smooth arrangement of matter amount to a low entropy state? Because in a system dominated by an attractive force such as gravity, a uniform distribution of matter is highly unstable (and provides a highly useable supply of potential energy). However, about 10^5 years after the big bang, matter seems to have been distributed smoothly to very high accuracy.

One way to get a sense how surprising this is, is to recall that we've found no reason to disagree with Boltzmann's suggestion that there's no objective distinction between past and future—no sense in which things really happen in the direction we think of as past-to-future. Without such a distinction, there's no objective sense in which the big bang is not equally the end point of a gravitational collapse. Somehow that collapse is coordinated with astounding accuracy, so that the matter involved manages to avoid forming large agglomerations (in fact, black holes), and instead spreads itself out very evenly across the universe.

In my view, this discovery about the cosmological origins of low entropy is one of the great achievements of late twentieth century physics. It is a remarkable discovery in two quite distinct ways, in fact. First, it is the only

anomaly necessary to account for the low entropy we find in the universe, at least so far as we know. So it is a remarkable theoretical achievement—it wraps up the entire puzzle of the thermodynamic asymmetry into a single package, in effect. Second, it is astounding that it happens at all, according to existing theories of how gravitating matter should behave—which suggests, surely, that there is something very important missing from those theories. (True, it is easy to fail to see how astounding the smooth early universe is, by failing to see that the big bang can quite properly be regarded as the end point of a gravitational collapse. But anyone inclined to deny the validity of this way of viewing the big bang faces a perhaps even more daunting challenge: to explain what is meant by, and what is the evidence for, the claim that time has an objective direction.)

9. Open questions

Why is the universe smooth soon after the big bang? This is a major puzzle, but—if we accept that the one-asymmetry model—it is the only question we need to answer, to solve the puzzle of the thermodynamic arrow. So we have an answer to the question with which we began. What has happened to the puzzle noticed by those nineteenth century giants? It has been transformed by some of their twentieth century successors into a puzzle for cosmology, a puzzle about the early universe.

It is far from clear how this remaining cosmological puzzle is to be explained. Indeed, there are some authors (e.g., Callender 1997, 1998, 2004; Sklar 1993) who doubt whether it needs explaining. If so, then the upshot of our therapy is truly Wittgensteinian: “the philosophical problems” do indeed “completely disappear.” (PI, §133); but there are grounds for scepticism, in my view (Price 2004). However, these issues are beyond the scope of this paper. I want to close by calling attention to some open questions associated with this understanding of the origins of the thermodynamic asymmetry, and by making a case for an unusually sceptical attitude to the second law.

One fascinating question is whether whatever explains why the universe is smooth after the big bang would also imply that the universe would be smooth before the big crunch, if the universe eventually recollapses. In other words, would entropy would eventually decrease, in a recollapsing universe?²

2 The recognition of this possibility is commonly attributed to Thomas Gold (1962). However, as I am grateful to Larry Schulman for pointing out to me, the attribution may well depend on an extrapolation beyond anything explicit in Gold’s own work.

This possibility is often been dismissed on the grounds that a smooth recollapse would require an incredibly unlikely 'conspiracy' among the components parts of the universe, to ensure that the recollapsing matter did not clump into black holes. However, as we have already noted, this incredible conspiracy is precisely what happens towards (what we usually term) the big bang, if we regard that end of the universe as a product of a gravitational collapse. The statistics themselves are time-symmetric. If something overrides them at one end of the universe, what right do we have to assume that the same does not happen at the other? Until we understand more about the origins of the smooth early universe, then, it seems best to keep an open mind about a smooth late universe.

Some people dismiss the question whether entropy would reverse in a recollapsing universe on the grounds that the current evidence suggests that the universe will not recollapse. However, it seems reasonable to expect that when we find out why the universe is smooth near the big bang, we'll be able to ask a theoretical question about what that reason would imply in the case of universe which did recollapse. Moreover, as a number of writers have pointed out (see Hawking 1985; Penrose 1979), much the same question arises if just a bit of the universe recollapses—e.g., a galaxy, collapsing into a black hole. This process seems to be a miniature version of the gravitational collapse of a whole universe, and so it makes sense to ask whether whatever constrains the big bang also constrains such partial collapses.

10. Scepticism about the second law

In my view, the moral of these considerations is that until we know more about why entropy is low in the past, it is sensible to keep an open mind about whether it might be low in the future. The appropriate attitude is a kind of healthy scepticism about the universality of the second law of thermodynamics.

The case for scepticism goes like this. What we've learnt about why entropy increases in our region is that it does so because it is very low in the past (for some reason we don't yet know), and the increase we observe is the most likely outcome consistent with that restriction. As noted, however, the statistics underpinning this reasoning are time-symmetric, and hence the predictions we make about the future depend implicitly on the assumption that there is no corresponding low entropy boundary condition in that direction. Thus the Boltzmann probabilities don't enable us to predict without quali-

fication that entropy is unlikely to decrease, but only that it is unlikely to decrease, *unless there is the kind of boundary condition in the future that makes entropy low in the past*. In other words, the second law is likely to continue to hold so long as there isn't a low entropy boundary condition in the future. But it can't be used to exclude this possibility—even probabilistically!

Sceptics about the second law are unusual in the history of thermodynamics, and I would like to finish by giving some long-overdue credit to one of the rare exceptions. Samuel Hawksley Burbury (1831–1911) was not one of the true giants of thermodynamics. However, he made an important contribution to the identification of the puzzle of the time-asymmetry of thermodynamic phenomena. And he was more insightful than any of his contemporaries—and most writers since, for that matter—in being commendably cautious about declaring the puzzle solved.

Burbury was an English barrister. He read mathematics at Cambridge as an undergraduate, but his major work in mathematical physics came late in life, when deafness curtailed his career at the Bar. In his sixties and seventies, he thus played an important role in discussions about the nature and origins of the second law. In a review of Burbury's monograph *The Kinetic Theory of Gases for Science* in 1899, the reviewer describes his contribution as follows:

[I]n that very interesting discussion of the Kinetic Theory which was begun at the Oxford meeting of the British Association in 1894 and continued for months afterwards in *Nature*, Mr. Burbury took a conspicuous part, appearing as the expounder and defender of Boltzmann's H-theorem in answer to the question which so many [had] asked in secret, and which Mr. Culverwell asked in print, '*What is the H-theorem and what does it prove?*' Thanks to this discussion, and to the more recent publication of Boltzmann's *Vorlesungen über Gastheorie*, and finally to this treatise by Burbury, the question is not so difficult to answer as it was a few years ago. (Hall 1899)

It is a little misleading to call Burbury a defender of the *H*-theorem. The crucial issue in the debate referred to here was the source of the time-asymmetry of the *H*-theorem, and while Burbury was the first to put his finger on the role of assumption of molecular chaos, he himself regarded this assumption with considerable suspicion. Here's how he puts it in 1904:

Does not the theory of a general tendency of entropy to diminish [*sic*]³ take too much for granted? To a certain extent it is supported by experimental evidence. We must accept such evidence as far as it goes and no further. We have no right to supplement it by a large draft of the scientific imagination. (Burbury 1904)

Burbury's reasons for scepticism are not precisely those which seem appropriate today. Burbury's concern might be put like this. To see that the dynamical processes routinely fail to produce entropy increases towards the past is to see that it takes an extra ingredient to ensure that they do so towards the future. We're then surely right to wonder whether that extra ingredient is sufficiently universal, even towards the future, to guarantee that the second law will always hold. As the first clearly to identify the source of the time-asymmetry in the *H*-theorem, Burbury was perhaps more sensitive to this concern than any of his contemporaries.

At the same time, however, Burbury seems never to have distanced himself sufficiently from the *H*-theorem to see that the real puzzle of the thermodynamic asymmetry lies elsewhere. The interesting question is not whether there is a good dynamical argument to show that entropy will always increase towards the future. It is why entropy steadily *decreases* towards the past—in the face, note, of such things as the effects of collisions and external influences, which are 'happening' in that direction as much as in the other! As we've seen, this re-orientation provides a new reason for being cautious about proclaiming the universal validity of the second law. Once we regard the fact that entropy decreases towards the past as itself a puzzle, as something in need of explanation, then it ought to occur to us that whatever explains it might be non-unique—and thus that in principle, there might be a low entropy boundary condition in the future, as well as in the past.⁴

3 Burbury is apparently referring to Boltzmann's quantity *H*, which does decrease as entropy increases.

4 An earlier version of this paper is due to appear as (Price 2006). I am grateful to the editor of the volume concerned for permission to reprint much of that material here.

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