Forthcoming in *The British Journal for the Philosophy of Science.*

Boltzmann's Time Bomb

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ABSTRACT: Since the late nineteenth century, physics has been puzzled by the time-asymmetry of the thermodynamic phenomena in the light of the apparent T-symmetry of the underlying laws of mechanics. However, a compelling solution to this puzzle has proved elusive. In part, I argue, this can be attributed to a failure to distinguish two conceptions of the problem. According to one, the main focus of our attention is a time-asymmetric law-like generalisation. According to the other, it is a particular fact about the early universe. This paper aims (i) to distinguish these two different conceptions of the time-asymmetric explanandum in thermodynamics; (ii) to argue in favour of the latter; and (iii) to show that whichever we choose, our rational expectations about the thermodynamic behaviour of the future must depend on what we know about the past—contrary to the common view, statistical arguments alone do not give us good reason to expect that entropy will always continue to increase.

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1 Introduction

Late in the nineteenth century, leading physicists were puzzled by the temporal asymmetry of the Second Law of Thermodynamics, in the light of the apparent time-symmetry of the underlying laws of mechanics. Late in the twentieth century, however, a leading authority on the subject could still refer to an understanding of this asymmetry as 'the elusive object of desire' (Sklar, [1995], p. 191). To be sure, not all of Professor Sklar's contemporaries would have agreed with his pessimistic assessment of the state of the subject. But equally surely, the optimists could not have agreed amongst themselves as to what solution had been found. In the preceding century, then, progress had been slow—a convincing solution had indeed proved elusive.

In my view, one of the things which has made an explanation of the thermodynamic asymmetry so elusive is that it is difficult to be clear about the precise nature of the problem—about what *exactly* it is that needs to be explained. Indeed, with respect to this meta-issue about the nature of the explanandum, Sklar's cinematic allusion seems especially apt. In Buñuel's *Cet Obscur Objet du Désir,* the obsessed hero seems oblivious to the fact that the object of his desire is played by two different actresses. I don't think that Sklar had this ambiguity in mind, but it seems beautifully analogous to the present case. There are two distinct conceptions of what needs to be explained about the asymmetry of thermodynamics, and it is easy to fail to notice that they are distinct, easy to bark up the wrong tree.

In this paper I try to tease these two conceptions apart, and to identify one of them as the problem's *vera crux*. My aim is thus to clarify the issue of the proper explanandum in thermodynamics, under the assumption that it is the time-asymmetry which excites our interest, and which drives the enquiry. To a large extent, this project is a matter of 'framing' views which are already well-known, and simply of calling attention to crucial respects in which they differ from one another (in particular, respects to do with their conceptions of the nature of the problem). The most crucial point of difference concerns the logical form of the asymmetric explanandum in thermodynamics. I argue that not only is the crucial explanandum not law-like, but that it is not even a generalisation. On the contrary, I maintain, the crux of the observed thermodynamic asymmetry is an existential or particular fact, concerning the nature of our universe early in its history.

In one sense, the view itself is certainly not new. As I shall explain, it originates in Ludwig Boltzmann's later statistical approach to the Second Law.[1] The implication

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[1] Boltzmann's earlier proposal is the so-called H-Theorem. The statistical approach begins in the late 1870s with Boltzmann's response to Loschmidt's 'reversibility objections', and is developed further in the 1890s, especially in response to a debate initiated by Culverwell about the source of the time-asymmetry in

concerning the nature of the asymmetric explanandum has been drawn explicitly by a number of later writers (e.g., Penrose [1989], p. 315). But what has not been well appreciated, I think, is the extent to which this view is simply an answer to a different question from the one asked by many other writers about the thermodynamic asymmetry. Hence it hasn't been understood how the debate proceeds at cross purposes—how the fact that these latter writers find the statistical approach incomplete rests on the fact that they have a different conception of what needs to be done. So while the views themselves are well-known, I think that the taxonomy I offer is novel, and helpful in clarifying the difference between Boltzmann's approach and others.

More importantly, however, the taxonomy helps to bring to light a surprising and little-recognised corollary of Boltzmann's view. It turns out that despite the truly massive numbers involved, the Boltzmann statistical considerations in themselves do not give us grounds for confidence that entropy will not decrease in the future. In so far as we presently have such grounds, they rest on what has been learnt quite recently about why entropy was low in the past, and on the implications of this new knowledge concerning the future. (Hence, it turns out, they rest on cosmology.)

This corollary of Boltzmann's view is certainly counterintuitive, and might be thought to be a good reason for preferring the alternative view of the explanandum. But in fact, I argue, the alternative view leads by a different path to the same conclusion. The conclusion thus turns out to be inevitable, whichever of the two views of the explanandum we prefer. Either way, the Second Law is in a sense exploded—contrary to the received view, statistical mechanics alone does not give us grounds for confidence that entropy will continue to increase. Until we know more about relevant aspects of past, rational epistemic practice demands that we keep an open mind about the thermodynamic characteristics of future.

This radical threat to the authority of the Second Law has been ticking away for more than a century, implicit but largely unrecognised in Boltzmann's response to the reversibility objections. It is true, as we shall see, that Boltzmann himself suggested a way in which it might be disarmed. But this strategy, never entirely satisfactory, seems to have been decisively overturned by recent work in cosmology. Boltzmann's conceptual bomb has been detonated, in my view, and it is time to assess the damage it has done.

Boltzmann's derivation of the Second Law; see Sklar [1993], ch. 2, Price [1996], ch. 2. The exact relationship between Boltzmann's early and late views is a matter for considerable debate, in advance of which it would unwise to claim that what I am here calling the statistical approach is exactly Boltzmann's view. But the key themes of the approach do seem to originate largely with Boltzmann, and it is therefore appropriate, as well as conventional, to associate the view with his name.

1.1 Outline of the paper

As noted, my main aim is to distinguish two different views of the core explanatory issue concerning the time-asymmetry of thermodynamics. I begin (1.2) by setting out some presuppositions which provide some of the ground rules for the debate, as I see it. These assumptions avoid much irrelevancy, in my view. While it would be unrealistic to expect that they will be uncontroversial, I hope that even those who are inclined to reject them will find them useful reference points for a more comprehensive taxonomy of possible views of the time-asymmetry of thermodynamics (and be prepared to say how rejecting one or other of these assumptions gives us a better account of this explanandum).

With these presuppositions in place, Section 2, the main body of the paper, distinguishes two conceptions of the time-asymmetric explanandum in thermodynamics. 2.1 begins with two views of the relevant 'contrast class'—that is, two conceptions of how nature would be if the observed thermodynamic asymmetry did not obtain. These two conceptions of the contrast class correspond to two views of the explanandum. On one view, the primary locus of the asymmetry is a law-like or quasi–law-like *generalisation* about the behaviour of matter. On the other, it is a *particular* fact about the history of our universe—a single 'anomaly' in want of explanation, in the light of a *time-symmetric* understanding of the typical character of physical processes. I suggest that Boltzmann's statistical approach is best seen in the latter light.

Sections 2.2–2.3 defend and clarify this 'Particularist' understanding of the statistical approach, and its points of contrast with 'Generalist' approaches to the thermodynamic asymmetry. One major contrast, and one of Particularism's major advantages, turns on its theoretical economy: it calls for (at most) one temporal asymmetry (a low entropy boundary condition in the past), where its Generalist rival requires two (the same asymmetric boundary condition, *and* an asymmetric principle guaranteeing that entropy does not decrease).

Section 2.4 sets outs what is perhaps the most surprising and least understood consequence of the Particularist view, and, apparently, its starkest point of contrast with the Generalist alternative: its contention that our reasons for thinking that entropy will not decrease in the future depend on our knowledge of why it was low in the past. This conclusion is an immediate consequence of the time-symmetric feature of the probabilities invoked by the Particularist view, and the fact that they do not exclude a low entropy boundary condition in the past.

It might seem a major advantage of the Generalist view that its time-asymmetric dynamical probabilities avoid this counterintuitive conclusion. Sections 2.5–2.8 respond to this suggestion, arguing not only that the same conclusion re-emerges in new epistemological form—our case for accepting Generalism must defer to informed judgements about the relevant boundary conditions—but also that Generalism fails a major test. Roughly, it fails to establish that there is work for the dynamical asymmetry to do.

Finally, Section 3 turns to the big cosmological 'anomaly', which Particularism takes to be the true crux of the problem of thermodynamic asymmetry. 3.1 describes the anomaly itself, as we currently understand it. 3.2 responds to an argument from Sklar, to the effect that an appeal to cosmology cannot solve the puzzle of thermodynamic asymmetry. (The issue concerns the so-called 'branch systems' approach. While I endorse Sklar's criticisms of versions of that approach which presuppose a Generalist understanding of the explanandum, I argue that a Particularist version fares better.) And 3.3 concerns the project of explaining the anomaly. *Inter alia*, I respond to the common suggestion that it is inappropriate to seek an explanation of such an 'initial condition'.

1.2. Four preliminary clarifications

First, then, to mark off some boundaries and ground rules for the present discussion—in other words, to set out briefly some presuppositions or assumptions I shall take for granted in the remainder of the paper. My aim is to investigate the nature of the time-asymmetric explanandum in thermodynamics, in the light of these presuppositions. I make these assumptions in order to avoid issues which seem to me irrelevant to the main game, and hence to try to minimise the talking at cross purposes which tends to characterise debate on these matters. Some people will feel that in drawing the boundaries as I do, I fence myself off from the interesting part of the subject. But all the more reason to mark the boundaries of my own discussion explicitly, in order to bring these disagreements into the open.

I make four main presuppositions. First, I take it that the issue is not about the direction, asymmetry or anisotropy *of time itself*. It is about the asymmetry of certain physical processes *in* time, not about the asymmetry *of* time. For my own part, I am not convinced that it is possible to make sense of the possibility that time itself might have a direction.[2] And it is simply not necessary to make sense of this possibility to characterise the asymmetry of thermodynamic phenomena. This asymmetry consists in the fact that many thermodynamic phenomena are asymmetric *in* time, occurring in nature with only one of the two temporal orientations apparently permitted by the underlying laws.

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[2] The best candidate is the view that time might be anisotropic, and that this would be evidenced by time-asymmetry in the laws of physics. (See Horwich ([1987]), ch. 3, for example.) However, this view is at best of secondary relevance to the issue of the nature of the time-asymmetric explanandum in thermodynamics. True, some writers maintain that the latter explanandum comprises, or includes, a time-asymmetric physical law; and might propose to link this law, by the view in question, to an anisotropy of time itself. But our present interest lies at the first stage, in the issue of the nature of the explanandum. At that stage, what we are dealing with is certainly an issue about the asymmetry of physical phenomena in time, rather than an issue about time itself.

It is true that the Second Law is commonly formulated in a way which presuppose a temporal orientation. If we say 'Entropy always increases', or 'Entropy never decreases', we are taking for granted a view as to which counts as the 'positive' direction of time—reverse the sign on the temporal axis, and an increase becomes a decrease, and vice versa. However, this does not imply that the existence of the thermodynamic asymmetry shows or requires that the choice of sign is anything more than conventional. After all, there is an objective asymmetry which consists in the matter on which the two labelling schemes agree, viz., that there is a monotonic entropy *gradient*.

Some people may feel that they can make sense of the possibility that one labelling scheme or other is objectively correct, and hence that there is an objective fact in nature about the slope of this entropy gradient—whether it is positive or negative. On this view, there is an additional fact to be explained, in addition to the existence of the gradient itself. For my part, I don't understand what that additional fact could be, or what could count as evidence about it, one way or the other.[3] For the purposes of this paper, then, I take for granted that the objective asymmetry consists simply in the existence of the monotonic gradient in the observed region of our universe, not in any further fact about the gradient's objective orientation. This is my second presupposition.

Thirdly, I assume that the asymmetry to be explained turns on the vast numerical imbalance between 'entropy-increasing' processes and their 'entropy-decreasing' temporal mirror images, and not on the issue of whether an individual process of the former kind can be 'reversed' into a process of the latter kind. The latter issue is logically independent of the former, as is easily seen by considering a spatial analogy. The issue as to whether nature exhibits a numerical imbalance between left-handed and right-handed versions of structures of a certain kind (hands, for example) is independent of the issue as to whether it is possible to convert a specimen of one parity into a specimen of the other. In particular, therefore, the issue of the temporal asymmetry of thermodynamics does not turn on the practical difficulties of 'reversing the motions' in real systems.[4]

Finally, and perhaps most surprisingly, I assume that the term 'entropy' is actually inessential. We can characterise our explanandum as a long list of the actual kinds of physical phenomena which exhibit a temporal preference; which occur in nature with one temporal orientation but not the other. Warm beer cools down rather than

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[3] It is not enough that time be anisotropic, in the sense mentioned in the previous footnote. For example, the fact (if it is a fact) that the universe is closed by the Big Bang at one end and open at the other, does not make one end 'objectively' the future, or 'positive', direction of time. Indeed, if there were an objective fact of this kind, then presumably there would be two kinds of possible such universes, those with the Big Bang at the negative end and those with it at the positive end. But what would distinguish such worlds, and how could we tell which we lived in?

[4] In making this assumption I part company, for example, with Ridderbos and Redhead ([1998]), who take the central explanandum to be the issue as to why, in contrast to the spin-echo experiments, most real systems are not practically reversible.

heats up in a tub of ice, pressurised gas flows out from but not into a bottle, and so on. (In all these cases the description presupposes the ordinary temporal labelling, but not that the sign of that labelling is anything more than conventional.) The notion of entropy *may* turn out to provide a useful way of generalising over this class of phenomena, but doing without it wouldn't deprive us of the means to characterise our real object of desire, which is an understanding of the temporal bias displayed by real systems.[5]

In principle, we could do without the notion of entropy altogether, in other words, and hence by-pass a century of discussions about how it should be defined. In practice, knowing that this option is open to us in principle, we can afford to be less strict. I shall continue to use the term in this paper, but I presuppose that in the last analysis, if need be, it may be regarded as merely a convenient and telegraphic label for aspects of physical systems which could be characterised in other ways.

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

2 Two conceptions of the thermodynamic asymmetry

2.1. What would symmetry look like?

Explanatory questions normally presuppose a contrast class. When we ask 'Why E?', we mean 'Why E rather than F?', even if F is not explicitly mentioned. If E is the time-asymmetry of thermodynamics, what is F? In other words, what would time *symmetry* look like, with respect to the phenomena in question? There are two quite distinct possibilities:

(a) *Non-monotonic entropy gradients*. The world might exhibit entropic gradients in both temporal directions, without a global temporal preference, at least on the large scale. For example, there might be a single long period of entropy 'increase', 'followed' by a matching period of entropy 'decrease'.

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[5] At worst, in my view, we lose the ability to specify which cases of temporal asymmetry count as thermodynamic. The problem might be thought more serious. After all, isn't the crucial explanandum the entropy-related generalisation itself? If this were so, then arguably the term 'entropy' would be essential, on the grounds that the generalisation could not be formulated without it. The argument would not be irresistible. After all, there are certainly ways of formulating important sub-generalisations without using the notion of entropy, such as by noting that heat flows from warmer bodies to cooler bodies. But in any case, as I argue below, the real time-asymmetric explanandum is not a generalisation.

(b) *No entropy gradient.* Entropy might be approximately constant everywhere, and at all times.

So the question 'Why not symmetry?' could be taken to mean 'Why not (a)?', or 'Why not (b)?', or perhaps 'Why not either (a) or (b)?'—and these questions correspond to different understandings of the problem.

One difference in particular seems to me to be both important and under-appreciated. In asking 'Why not (a)?' we are asking for an explanation of an apparently *general* or *universal* feature of entropy gradients, viz., that they all slope upwards in the future direction. In asking 'Why not (b)?', on the other hand, we are asking for an explanation of an *existential* feature of entropy gradients, viz., that there are any. This difference illustrates the way in which the issue of the contrast class allows us to tease apart two quite different approaches to the thermodynamic time-asymmetry.

On one side are what I shall call *Causal-General* theories. These approaches take the explanandum to be, at least in part, a time-asymmetric *generalisation*—the general fact that entropy never decreases, or some such. Broadly speaking—perhaps taking some liberties with the terms *causal* and *dynamical*—they seek a causal explanation of this general fact in dynamical terms. Approaches I take to fall under this heading include 'interventionism' and certain appeals to asymmetric initial microscopic independence conditions, as well as to suggestions grounded on law-like asymmetries in the dynamical laws themselves.[6] What unifies these diverse approaches, in my view, is their sense of the nature of the project. All of them seek a *causal-explanatory* account of a *time-asymmetric generalisation* about the physical world as we find it. One of my main claims in this paper will be that neither aspect of this characterisation of the project is compulsory, if our task is to understand the temporal asymmetry of thermodynamic phenomena. There is an alternative and in my view preferable characterisation of the project, which rejects both aspects of the Causal-General characterisation of what needs to be done.

At this stage, however, I shall not be alarmed if neither aspect of label 'Causal-General' seems entirely compelling. In what follows, I hope to earn the right to apply the label to a range of familiar approaches by a taxonomist's usual techniques—by calling attention to their commonalities, and especially to the respects in which they differ from alternative approaches.

One crucial commonality is the attempt to identify a dynamical 'cause', 'engine' or *sine qua non* of entropy increase—that is, crucially, of some factor *in the absence of which entropy might decrease*. Whether this factor is an environmental influence, or some

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[6] Interventionism is the view that it is random environmental influences which force entropy to increase. The appeal to initial independence is famously embodied in Boltzmann's H-Theorem—on the issue as to which such appeals are relevant here, see fn. 19. For a recent version of the asymmetric dynamical law approach, see Albert ([1994]).

principled lack of initial correlations, or something in the dynamical laws themselves, it does need to be time-asymmetric. Why? Simply because otherwise it would 'push' equally well in either direction. With a symmetric dynamical engine, then, entropy would be constrained not to decrease in both temporal directions. In other words, it would have to be constant—there would be no entropy gradient.

Absence of an entropy gradient is option (b) above, which hence corresponds to one way in which the Causal-General theory might answer the question 'What would symmetry look like?' At first sight, however, this approach also seems to allow for symmetry via option (a). If entropy is prevented from decreasing by the action of an asymmetric dynamical engine, then if there were no such engine or factor, asymmetric or otherwise, entropy might decrease. In other words, we might find entropy gradients inclined in both temporal directions.[7]

But there is a tension here. A gradient which goes down in one direction goes up in the other. So this possibility would amount to a world in which there were two kinds of entropy increases, those which increased to the future and those which increased to the past. By hypothesis, these increases occur without the help of a dynamical engine. Wouldn't this undermine the case for thinking that such an engine is necessary to explain the increase in the actual world? (True, there is still the problem of explaining the asymmetry of increases over decreases in the actual world, an asymmetry which by hypothesis would be absent in the imagined world. But as we shall see, it is far from clear that this asymmetry is evidence of an asymmetric *cause* or *law*.)

I'll return to this tension below (in 2.7), when I mention some objections to the Causal-General theory. As we'll see, there are general grounds for scepticism about the efficacy of the Causal-General theory's asymmetric engine. Leaving this point to one side, it seems that according to a Causal-General theory, there are two relevant contrasts—two ways in which the universe might have been time-symmetric, had things been otherwise. An understanding of the supposed role of the dynamical engine calls for an appreciation of both contrasts.

Things are more straightforward for the rival to the Causal-General theory. According this *Acausal-Particular* view, as I shall call it, there is no *time-asymmetric* generalisation in need of explanation in thermodynamics. All the time-asymmetry of observed thermodynamic phenomena resides in an existential or particular fact—roughly, the fact that physical processes in the known universe are constrained by a low entropy 'boundary condition' in one temporal direction.[8] Against the background of a

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[7] This conception of the time-symmetric contrast class is implicit in standard versions of the branch systems approach (see 3.2, below), for example, by which the crucial issue is taken to be that as to why all low-entropy branch systems show the same temporal orientation, not that as to why there are low-entropy branch systems in the first place.

[8] My use of the term 'boundary condition' is intended simply to reflect the role the proposition in question plays in the explanation of the thermodynamic asymmetry, relative to 'local' dynamical constraints on the

time-symmetric understanding of the normal behaviour of matter, this particular fact alone is sufficient to account for the observed asymmetry in thermodynamic phenomena. The task of explaining the observed asymmetry is thus the task of explaining a particular violation of contrast class (b)—a particular huge entropy gradient, in a world in which (roughly[9]) none are to be expected.

The Acausal-Particular view is a familiar approach under an unfamiliar name. It is essentially Boltzmann's statistical approach to the explanation of the thermodynamic asymmetry. My purpose in giving it a new name is to call attention to the two inadequately-understood respects in which it differs from the approaches I characterise as Causal-General theories. I suspect that not all who regard themselves as followers of Boltzmann will agree with me about what his approach entails. In imposing an illuminating descriptive taxonomy, I hope to bring such differences into the open, to clarify the options, and to show how radical a reorientation is involved in Boltzmann's later view.

Labels aside, the basic character of Boltzmann's statistical approach is well known. Consider a system not currently in equilibrium, such as a vial of pressurised gas within a larger evacuated container. We want to know why the gas expands into the larger container when the vial is opened. 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. There is no need for an asymmetric causal constraint to 'force' the gas to leave the bottle—this is simply what we should expect it to do anyway, if our expectations about its initial state are guided by Boltzmann's probabilities.[10]

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behaviour of matter. In other words, it reflects the view that the condition does not flow from those constraints, but needs to be given independently. It is not intended to exclude the possibility that the condition might turn out to be a law-like consequence of some broader (say, cosmological) theory.

[9] The qualification is needed because the expectation is probabilistic. Accordingly, entropy gradients are *sometimes* to be expected, the more frequently the smaller they are. Large entropy gradients, such as those observed in the real world, are to be expected approximately never (at least in the absence of some sort of anthropic selection mechanism).

[10] This is not to deny (of course) that there are causal constraints which will contribute to determining whether the gas will leave the bottle. The function from present microstates to future trajectories obviously depends on many such constraints—the presence or absence of a cap on the bottle, to mention just one example! In real systems it is by no means a trivial matter what future or past macrostates are 'accessible' to systems in a given initial or present macrostate. However, the essence of the statistical view is simply that with these constraints—whatever they are—held fixed, the tendency of the gas to 'equilibrate' reflects nothing

Why does this approach makes option (b) the important contrast class, for an understanding of the observed thermodynamic asymmetry? Essentially, because Boltzmann's way of counting possible microstates is time-symmetric. As a result, it implies that equilibration is equally likely 'towards the past' as towards the future. Most microstates compatible with the present macrostate correspond to histories in which the gas was more dispersed in the past, and in general, in almost all possible histories, the gas spends almost all of its time very uniformly distributed. The present low entropy state, and the apparent decrease of entropy towards the past, thus appear very atypical, or exceptional, by the same measure which renders what happens towards the future so unexceptional. Moreover—by symmetry, again—such entropy-decreasing behaviour appears exceptional, *regardless of its temporal orientation*. Hence an option (a) universe, with a spatiotemporally symmetric arrangement of entropy gradients, would be no less in need of explanation than a universe with an asymmetric gradient. In both cases the basic question is the same. Why isn't entropy high almost everywhere, almost all the time?

2.2. Boltzmann's about-turn

Let's formulate the basic principles of this statistical approach a little more precisely. First of all, let's assume that basic explanatory questions are of the form: 'Given that C, why E rather than F?' The thought here is that we never explain things in isolation. We always take something as already given, and seek to explain the target phenomena in the light of that. C represents this background, and E the target phenomenon. (C might comprise accepted laws, as well as 'boundary conditions' being treated as 'given' and unproblematic for the purposes at hand.) The key requirement of the statistical approach is then that we have some principled basis for delineating a space of possibilities, and for a measure on that space, such that we can ask whether E is 'common', or 'probable', or 'normal', given C. If and only if this holds—E is normal (and F abnormal), given C —we may take it that the explanatory question has been adequately answered. There is no longer any explanatory puzzle, *at the level of the original question*. There may well be issues at other levels, of course. Why C, for example? In the thermodynamic case, as we shall see, this turns out to be the really interesting question.

Glossed in these terms, the application of the statistical approach goes something like this. As before, we consider a system not currently in equilibrium, such as a newly-opened vial of pressurised gas within a larger evacuated container—let C stand for a description of this set up. We want to know why, given C, the gas expands (E) into the larger container. We consider what possible future 'histories' for the system are compatible with C. Boltzmann proposes that according to a natural measure, almost all

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more than the numerical imbalance between the initial microstates which are such that it does so, and those which are such that it does not; and in particular, is not a sign of a time-asymmetry in these causal constraints.

the available future trajectories are such that E. It is possible that the actual microstate of the system 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 (in the measure in question) between abnormal and normal states, the behaviour we actually observe is unexceptional, and calls for no further explanation.

The statistical approach thus relies on what might be termed a *normalising* pattern of explanation. Normalising explanation seems significantly different from causal explanation. In the former case, the explanation identifies no responsible factor, no *sine qua non*, for the explanandum in question. On the contrary, it relies on pointing out that in an important sense, no such factor is needed—there is no relevant 'less constrained' state, which the systems in question are being prevented from reaching.

This distinction between normalising and causal explanation seems to me to be useful, and helpful in particular in conveying a sense of the difference between the statistical approach and others. However, the nature of explanation is not my present topic, and the distinction bears no essential weight for the purposes of this paper. In labelling the statistical approach 'acausal' I have a more specific point in mind. This approach turns its back on the idea that the explanation of the asymmetry of thermodynamic phenomena requires identification of an *asymmetric* cause or engine for entropy increase. (In its place, as we are about to see, it offers us a time-symmetric conception of the normal behaviour of matter.) Thus its view of the location of the time-asymmetry is appropriately called 'acausal', whatever our views about the nature of explanation.

I should also emphasise that I am taking no particular stand on the nature and origins of the probabilities involved in Boltzmann's account. It is therefore compatible with the claims of this paper that these are problematic matters for the statistical approach. What matters for present purposes is simply that whatever their nature and origin, these probabilities are *time-symmetric.* As a result, that the entire burden of explaining the time-asymmetry of thermodynamic phenomena needs to be born elsewhere (viz., in an asymmetric boundary condition). My interest is in contrasting this one-asymmetry approach with approaches which invoke time-asymmetry in two places: not only in the same asymmetric boundary condition, but also (and more significantly, by their own lights) in an asymmetric nomological constraint on the behaviour of matter. For present purposes, therefore, the crucial feature of the statistical approach is simply that it involves no such second asymmetry.

However, Boltzmann's approach avoids the second asymmetry at the cost of making the first very much more puzzling than it might otherwise seem. Consider again the case of our gas, for example. If in a time-symmetric measure on the space of possible histories for the gas, the vast majority of histories compatible with the state C are such that the gas disperses in the future, then the vast majority of trajectories compatible with

C are also such that the gas was dispersed in the past. In other words, the more we make it seem *unexceptionable* that entropy increases in one direction, the more we make it seem *exceptionable* that the same does not happen in the other direction. We make the future seem *normal* at the cost of making the past seem *abnormal*.

Even more importantly, perhaps, the cost of making E seem normal and unproblematic, given C, is to make C itself seem very abnormal and problematic. Boltzmann offers us an account of why E, given C, which makes it puzzling why C obtains in the first place. The *normal* thing would be for a system not to be in disequilibrium at all—for the gas to be dispersed throughout the container from the beginning.[11]

Understood in these statistical terms, then, Boltzmann's legacy is in two parts. First, he gives us a kind of statistical 'Master Principle', a train of thought going something like this:

> *Master Principle*: There is microscopic variation in nature, such that physical systems similar in gross or macroscopic respects may nevertheless differ in detailed or microscopic respects. This variation occurs within a space of possibilities, to which a probability measure may be applied, yielding empirical predictions in cases in which the statistical distribution of microscopic variations has macroscopic consequences. The general principle that entropy increases then reflects nothing more profound than that highly probable variations are very common. (The interesting physical issues have already been subsumed into the question as to what the range of variation and measure actually are, for systems of any particular kind.)

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[11] In a reply to an ancestor of this paper, David Albert ([1998]) has suggested that there is something odd in using Boltzmann's probabilities in this way, so that 'the world's having started out in [a low entropy state] amounts ... to some sort of *puzzle*.' However, it is clear that Boltzmann himself did not find this odd. In responding to Loschmidt's famous 'reversibility objection' he adds the following note:

> I will mention here a peculiar consequence of Loschmidt's theorem, namely that when we follow the state of the world into the infinitely distant past, we are actually just as correct in taking it to be very probable that we would reach a state in which all temperature differences have disappeared, as we would be in following the state of the world into the distant future. (Boltzmann [1877], p. 193)

In fact, of course, entropy seems to have been even lower in the past than it is now. On Boltzmann's own admission, then, his statistical treatment makes it puzzling why entropy is *not* higher in the past. And he himself appears later to have in mind a solution to the puzzle, in terms of the anthropic idea that intelligent life can only exist in the rare but inevitable periods of fluctuation to low entropy in an infinite universe—see Boltzmann ([1895], p. 415). Neither the fact that this solution is problematic (see Price [1996], pp. 33–37, and fn. 14 below) nor the fact that if it were not problematic it would show that there is a sense in which what we observe is not, after all, of very low probability, detracts from the fact that it shows that Boltzmann took seriously the implications of his own statistics for the 'initial conditions'. (Brown and Uffinck [forthcoming] refer to this issue as 'the second problem of Boltzmann'.)

Secondly, Boltzmann shows us that there is a major anomaly, consisting in the fact that under plausible accounts of variation and measure for real systems, which reflect the time-symmetry of the dynamics of those systems, such statistical predictions are often disastrously inaccurate, when applied towards (what we call) the past—and the fact that the world as we find it is of extremely low probability, under this natural measure.

A major consequence of Boltzmann's statistical approach is thus to problematise something which we might otherwise take for granted, namely, the low entropy present and past. This state of affairs is not necessarily problematic in the same way for other approaches, not guided by a time-symmetric conception of the possibilities open to matter. However, this is not to say that other approaches can do without such a low-entropy 'boundary condition' altogether. On the contrary, if entropy were not low initially then even if there were an asymmetric dynamical engine which made entropy increase, this engine would produce no observable asymmetry. So all parties must agree that this boundary condition is a crucial aspect of the problem of the time-asymmetry of thermodynamics. The differences are first, that the statistical approach renders this boundary condition at least *prima facie* exceptional, in a way in which other approaches need not; and second, and most crucially, that this is the *only* locus of time-asymmetry, according to the statistical view.

The essence of the Acausal-Particular approach is thus that if our focus is strictly on time-asymmetry in thermodynamics, the *whole* riddle is that of the low entropy past. In particular, we don't need a time-asymmetric version of the Master Principle. The trick to making a time-symmetric version compatible with the anomaly of the low entropy past is to accord a somewhat deferential status to the probabilities of the Boltzmann measure—to allow that they guide rational expectation *only in the absence of certain sorts of constraints.* The Master Principle thus tells us in a time-symmetric way what is probable, *other things being equal.* As far as we know, there is just one significant respect in which things are not equal, associated with the existence of the low entropy past.

As far as we know. However, to take the Master Principle to be time-symmetric is to hold open the possibility that there might be other circumstances of this kind—other contexts in which the Boltzmann probabilities are 'trumped' by other factors. This consequence of Boltzmann's view has not been widely appreciated, I think. As we shall see (in 2.4), it entails a conclusion which many people—including, I suspect, many who regard themselves as followers of Boltzmann—will find difficult to accept. Our justification for believing that entropy will continue to increase towards the future cannot rest on statistical grounds alone, but must rely on what we know about why entropy does not increase towards the past, and on the implications of that knowledge for the future.

In two ways, then, an understanding of the low entropy past thus becomes crucial. This past condition is both the crux of the puzzle of the observed thermodynamic asymmetry in our past light cone, and phenomenon we need to understand to predict the state of related matters on our future light cone.

This much is implicit in Boltzmann's statistical approach, I think, but we now know very much more about the past condition in question than was known in Boltzmann's time. One of the remarkable achievements of late twentieth century physics has been to provide a plausible cosmological characterisation of the condition in question. More on this condition, and the issue of its possible explanation, in Section 3. For the moment, the point I wish to emphasise is that the most revolutionary aspect of Boltzmann's statistical approach is to direct our attention 'behind us' in this way—to make the low-entropy past the focus of our enquiry into the time-asymmetry of thermodynamic phenomena. An interest in the *destination* of thermodynamic processes is replaced with an interest in their *origins,* or starting point. This dramatic theoretical and temporal re-orientation—'Boltzmann's about-turn', as we might call it—is the key to the difference between the Causal-General and Acausal-Particular approaches, and one of the main ambitions of my taxonomy is to give it the visibility that it deserves.

2.3. Two ways of parsing nature

One way to make Boltzmann's about-turn visible is to reflect on its historical antecedents. In the second half of the nineteenth century what seemed to call for explanation was a law-like regularity, related to the possibility of extracting work from warm bodies. Nineteenth-century physics sought first to characterise this regularity, and then to account for it in terms of the dynamical properties of matter and a statistical understanding of heat. But it was the striking uniformity and ubiquity of the regularity, not its time-asymmetry, which initially were seen as the target of explanation. Time-asymmetry came into the picture only gradually, as people began to see that because the generality they sought to explain was time-asymmetric, it was unclear, to say the least, that it could be explained on the basis of time-symmetric dynamics.

Once in play, however, the time-asymmetry tended to be seen as part and parcel of the relevant generalisation. This view is still widely held, and comprises the core of what I am calling the Causal-General approach. Proponents of this approach often concede that there is an additional issue—Why is entropy low in the past?—which is independent of the generality on which they take themselves to be focussing. But the original issue seems to survive, as a puzzle about the origins of an asymmetric generalisation. This is then a matter of thinking that there are *two* time asymmetries in play—not only the asymmetry of the low entropy initial condition, but also a law-like asymmetric principle which ensures that, from such conditions, entropy increases monotonically. As I have said, I think that this is a mistake. There is a generality in play, but it is not time-asymmetric (and apparently not law-like, in the full sense, in that it is overridden by the low entropy boundary condition). And there is a time-asymmetry in play, but it is not a generalisation.

The issue between the Causal-General and Acausal-Particular approaches may thus be characterised as a dispute about the proper way to ‘parse’, or construe the origins, of a range of natural phenomena. According to the Causal-General approach, the relevant thermodynamic phenomena arise as follows:

Asymmetric boundary condition—entropy low in the past[12]
+ Asymmetric law-like tendency—entropy constrained to increase

Observed asymmetry.

Given that the boundary condition is itself time-asymmetric, this means that for the Causal-General approach, as I noted above, there are two asymmetries in play—the asymmetry of the boundary condition, and that of the law-like generalisation itself.

For the Acausal-Particular approach, on the other hand, the observed phenomena are parsed as follows:

Asymmetric boundary condition—entropy low in the past
+ Symmetric default condition—entropy likely to be high, *ceteris paribus*

Observed asymmetry.

Thus there is only one asymmetry in play, in this version.

It is surprising that such a stark contrast—the invocation of one temporal asymmetry in the latter approach, as against two in the former—seems to have received little explicit attention in the literature. The contrast suggests that *prima facie*, at least, the Acausal-Particular approach has a considerable theoretical advantage. To the extent that asymmetry is a theoretical ‘cost’, the Causal-General approach is a great deal less economical than its rival. And the extra expenditure doesn’t seem to buy us anything. After all, if the universe began in thermodynamic equilibrium and simply stayed there, there would be no temporal asymmetry to explain. How is that adding *one* asymmetry—a low entropy condition at one end—creates a need for *two* asymmetries to explain the respect in which the resulting universe differs from the symmetrical universe?

I’ll return to the issue of economy below—in 2.5–2.8, I’ll consider a possible response on the part of the Causal-General approach. For the moment, our interest is in the two different conceptions embodied by these models of what needs to be explained about the time-asymmetry of thermodynamic phenomena. For the Causal-General

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[12] To repeat the caution of fn. 8, note that my use of the term ‘boundary condition’ here is not intended to exclude the possibility that the condition in question might turn out to be a law-like.

approach, asymmetry enters at two points, but it is the apparent law-like generalisation rather than the boundary condition which has been the focus of most attention. This generalisation, after all, is the Second Law itself.

Historically, then, the primary explanandum associated with the temporal asymmetry of thermodynamics was seen as a law-like generalisation—'In all physical interactions, net entropy change is non-negative', 'Heat always flows from hot bodies to cold', or something of the kind. In some people's view, the effect of an appreciation that the Second Law is statistical in origin, and therefore doesn't have the truly universal character of other physical laws, is simply to dilute the universal quantifier—to replace 'all' with 'almost all'—while otherwise retaining the principle's asymmetric and law-like character.[13] But I am maintaining that the true impact of the statistical approach goes much further than this. What remains of the relevant generalisation is neither time-asymmetric nor law-like. And all the time-asymmetry resides in the boundary condition, previously seen as of secondary importance.

Indeed, at this point the issue of asymmetry has in one sense slipped into the background. According to the Acausal-Particular approach, as I noted at the end of 2.1, it is no longer the time-asymmetry *per se* which constitutes the crucial anomaly. What now seems most anomalous is not the time-asymmetry as such, but the fact that the observed trajectory of the universe is 'abnormal' to such a striking degree. The fact that this anomaly is time-asymmetric has become secondary. Exactly the same issue would arise if it were not—if low-entropy regions were found to occur in a time-symmetric pattern. On the other hand, time asymmetry certainly adds spice to the puzzle—it makes prospects for an explanation of the anomaly seem bleaker, given the apparent time-symmetry of the underlying laws. In that sense, the time asymmetry itself remains puzzling.

2.4 Could entropy decrease, and if not, why not?

Are we entitled to believe that entropy will continue to increase—that gas will not pressurise spontaneously, that heat will not flow from cold to hot bodies, and so on? If so, on what grounds?

It is widely assumed that we do have very good grounds to expect that entropy will continue to increase. According to Causal-General theories, our reason for confidence on this matter may be held to be that we have grounds to accept a law-like (albeit probabilistic) generalisation, a consequence of which is that entropy is very unlikely to decrease. However, the common confidence is not confined to supporters of

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[13] The principle is thought to retain its law-like character in the sense that it is still thought to support reliable, albeit probabilistic, projections concerning the future. As I emphasise below, the time-symmetric probabilities of the statistical approach do not have this projective character—they cannot provide reasonable grounds for excluding the possibility of a low entropy future boundary condition.

Causal-General theories. For example, it is expressed explicitly in a recent exposition of Boltzmann's statistical approach by the physicist Joel Lebowitz. After noting the need to assume a low-entropy initial macrostate, Lebowitz says that 'the initial microstate can be assumed to be typical' of the set of microstates compatible with that low-entropy initial macrostate. He continues:

> We can then apply our statistical reasoning to the further evolution of this initial state, ... and use phase-space-volume arguments to predict the future behaviour of macroscopic systems—but not to determine the past.
>
> The behaviour of all macroscopic systems ... can therefore be confidently predicted to be in accordance with the second law. (Lebowitz [1993], p. 37)

But is this confidence justified? In my view, claims of this kind lack an essential qualification. This use of statistical reasoning is legitimate only on the assumption that the macroscopic systems in question are *not* subject to a future low entropy condition, in addition to the past low entropy condition. To treat Boltzmann's probabilities in a properly time-symmetric way is to concede that because they do not exclude this possibility towards the past, they do not exclude it towards the future. In other words, the absence of a future low entropy condition cannot reasonably be inferred from the probabilities alone (vast though the numbers are).

According to the Acausal-Particular approach, in other words, we have no law-like assurance concerning the future direction of the entropy gradient. There is a generalisation in play, but it is time-symmetric, and in both directions it comes with a qualification: we should expect entropy to increase, *in the absence of specific reasons to think otherwise*. Increase is merely the default option. Since we know that that default option is not always exercised in nature, our view of the future needs to be a cautious one. We should simply suspend judgement, I think, until we know more about the origins of the low entropy condition in our past.

Interestingly, however, different explanations of the low entropy past have quite different implications for the future. Boltzmann's own suggestion ([1895], p. 415, [1964], p. 446; cf. fn. 11) was that the low entropy past in our region of the universe is merely the kind of rare fluctuation which is to be expected occasionally in an infinitely old universe. On this view, then, the explanation of the low entropy past requires nothing additional to the Boltzmann probabilities themselves. As a result, it gives us no reason not to take these probabilities to be predictive—no reason not to be confident that entropy will not decrease, at least in any future period of relevance to us, in view of the extremely high probability of extremely long intervals between fluctuations.[14]

Things are quite different by the lights of the leading contemporary proposal. As we shall see in Section 3, this proposal is that the source of the observed low entropy in our region is the very low gravitational entropy at the time of the Big Bang. On this view, it is conceivable that we might have reason to expect entropy to decrease in the relevant future, if we had theoretical reasons to expect such a low gravitational entropy to be associated with other cosmological events, and to expect such events in our own future, at sufficient proximity for us to feel their effects. (For more on this sort of possibility, see Price [1996], ch. 4; see also fn. 20.)

Once again, we must resist the temptation to think that any such reason would be swamped by the massive unlikeliness of entropy-reducing behaviour. The probabilities involved in the Boltzmann model are time-symmetric. In terms of these probabilities, entropy-reducing behaviour is *exactly* as unlikely towards the past as it is towards the future. Nevertheless, entropy does decrease towards the past. Unless we are prepared to allow that the low entropy past is merely a fluke—as, in effect, on Boltzmann's own view—then we have accepted that these probabilities are not the last word. As useful as they may be in other circumstances, they are 'trumped' by whatever explains the fact that entropy is low in the past. Symmetry and logical consistency then require that we accept that the same might happen towards the future. The most we can say is that entropy is very likely to increase in the future, *in the absence of the kind of constraints which make it decrease towards the past*.

The unpalatibility of this conclusion should not blind us to faults of the alternative. In effect, the conclusion is one horn of a fundamental dilemma. This dilemma offers us a choice between time-symmetric and time-asymmetric probabilities in the statistical foundations of thermodynamics. If we choose time-symmetric probabilities, then the fact that these probabilities do not always yield reliable predictions towards the past requires us to admit the possibility that they might fail in a similar way towards the future. In other words, we are led to the above conclusion.

The other horn of the dilemma consists in the choice of time-asymmetric probabilities. The immediate advantage of time-asymmetry might seem to be that it allows probabilities of a 'non-deferential' nature—probabilities which, unlike Boltzmann's

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[14] This is what I meant at the beginning, when I said that Boltzmann himself provides us with a way of defusing his own implicit challenge to the Second Law. It is worth noting that if Boltzmann's proposal were successful, it would amount to a 'no-asymmetry' explanation of the observed asymmetry of thermodynamic phenomena—it explains these phenomena within a world-view which is time-symmetric on the large scale. Unfortunately, the proposal faces major problems, the worst of which is that it is much easier for a fluctuation to 'fake' historical records, by producing them from scratch, than by producing the real state of affairs (of even lower entropy) of which they purport to be records. On this view, then, historical records such as memories are almost certainly misleading. See Price ([1996], pp. 33–37), Albert ([2000], ch. 4).

rather anaemic alternative, actually give us a rational basis for a very high degree of confidence that entropy will not decrease. On this view, the probabilities themselves are trumps, in the sense that they provide legitimate grounds for a prediction about the absence of a low-entropy future.

What are the disadvantages of this position? One, perhaps, is that a century and a quarter of investigation has failed to find a plausible source of the required asymmetry.[15] But let us set this difficulty aside, and approach the issue from the other end. Should a proper understanding of the explanandum concerning the asymmetry of thermodynamics lead us to search for an explanans of this kind, or does such a search rest on a mistaken conception of the nature of the problem? Are there objections of principle to this horn of the dilemma?

2.5 A case for time-asymmetric probabilities?

Earlier (2.3) we touched on what seemed to be one such objection. The Causal-General approach requires *two* temporal asymmetries to account for the observed asymmetry. It requires a low entropy initial condition, in addition to the asymmetry embodied in its probability measure. Without the former asymmetry, the latter produces no observable asymmetry: the universe simply begins and remains in a state of maximum disorder.

I noted that the Acausal-Particular approach thus had a *prima facie* advantage in theoretical economy, and appealed to the following intuition. In a universe which began and remained in a state of maximum entropy, there would be no asymmetry to be explained; and yet the respect in which our universe differs from such a symmetric universe seems characterisable simply by the addition of a single low-entropy boundary condition. How is it that the addition of such a boundary condition could create the need for a second asymmetric principle, to explain the asymmetry of the resulting universe? The second asymmetry thus seems redundant.

A proponent of the Causal-General view might respond as follows:

> The imagined symmetric universe fails to distinguish between two possibilities. In one possible world there is no constraint in either direction which ensures that entropy does not decrease. In the other possible world there is such a constraint, in both directions. In the latter case, one cannot simply add a low entropy boundary condition to one end (unless we're allowed to produce very unlikely worlds): one also needs to relax the constraint in the relevant direction—i.e., to make it asymmetric. In this latter case, then, it is only by making two asymmetric changes that one ends up with a world of the kind the Causal-

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[15] Though see Albert ([1994] and [2000]. ch. 7) for a view of this kind which is certainly better motivated than most.

> General view takes ours to be—a world with an asymmetric low entropy boundary condition, *and* an asymmetric law-like constraint. This leaves the former possibility, that we begin with a symmetric world with no constraint in either direction. In this case, adding an asymmetric boundary condition gives us an asymmetric world, but not the kind of world the Causal-General view takes ours to be. It gives a world in which entropy does increase monotonically, but not a world in which it is 'constrained' to do so—not a world is which the fact that it does so is a law-like matter. In other words, adding a low entropy boundary to the former kind of possible world does not yield a world like our world. So in neither case do we get a world like ours by adding one asymmetry to a symmetric world. The economy argument fails.

This response brings us to a crucial issue. Do we have good grounds for thinking that our world is one in which entropy is 'constrained' in this way, 'prevented' from decreasing by some law-like or causal mechanism? Should we expect things to be different if there were no such factor? Is there really work for such a mechanism to do ?

In my view, the right answer to all these questions is 'No'. More precisely, but just as damagingly, I think that the Causal-General view gives us no reason to think that the answer is not 'No'. To explain this conclusion, I want to begin with some arguments for the opposite view.

2.6 The objectivity of the phenomena

At several points in debates about the Second Law, it has been objected that certain proposals are insufficiently objective for the causal–explanatory task supposedly at hand. For example, Popper criticises Pauli's view that entropy is merely a 'measure of our ignorance', on the grounds that it cannot account for the objective facts of irreversibility. Popper uses the example of a small bottle of air inside an evacuated vacuum flask. The air escapes irreversibly into the larger flask when the smaller bottle is opened. Popper says that 'since the fact to be explained—the irreversibility of the process—is an objective experimental fact, the probabilities and improbabilities in question must be objective also.' (Popper [1982], p. 107) He says that Pauli's view 'leads to the absurd result that the molecules escape from our bottle because we do not know all about them, and because our ignorance is bound to increase unless our knowledge was perfect to begin with.' ([1982], p. 109)

There is more than one point in the offing here. One legitimate concern—not exactly Popper's, I think—might be that if we combined the doctrine that entropy is a measure of ignorance with the view that it is the increase in entropy which needs explaining in the case in question, we would have lost sight of the project of explaining the behaviour of the gas altogether. Our focus would be on our knowledge of the gas, not on the gas itself. In the present context, however, this concern is decisively sidestepped by my fourth presupposition at the beginning, that the term 'entropy' plays no essential role in characterising the asymmetric phenomena of thermodynamics. We are thus free to concentrate on what seems to be Popper's main claim, that epistemic probabilities cannot do causal work.

More recently, but in a similar vein, David Albert has described a common misgiving about Boltzmann's approach. Everything seems to hang on the fact that we are ignorant of the system's exact microstate, for it is in virtue of this that we distribute our epistemic probability equally over a class of possibilities, almost all of which lead toward equilibrium. But, as Albert puts it, 'Nothing, *surely*, about what anybody may or may not have *known* about those two bodies ... can have played any role in bringing it *about* (that is: in causing it to *happen*) that the *temperatures* of those two bodies subsequently *approached* each other!' (Albert [1994], p. 670, emphasis in the original).

In order to prevent confusion with issues concerning the low entropy past, it is helpful to reformulate these objections in terms of systems initially in equilibrium. Imagine the gas is initially distributed throughout the larger flask, for example. Popper's and Albert's argument would then be that ignorance alone cannot account for the objective fact that the gas does not accumulate in the smaller bottle. Since the explanandum—now, by stipulation, the behaviour of the gas—is an objective, observer-independent matter, its causal explanation cannot turn on an observer's state of knowledge.

This is correct as far as it goes, I think, but it doesn't establish that we need objective probabilities. After all, an opponent might agree that *if there were to be* a causal explanation of the fact that the gas stays dispersed, any probabilities it invoked would need to be objective; but go on to deny that such an explanation is needed. Perhaps this is where explanation stops, with brute Humean regularities. Or, more plausibly, perhaps the only *causal* explanation is the microphysical one, which, if the dynamics is deterministic, would not involve probabilities at all. In the deterministic case, the fact that the gas does not enter the container is ensured by the fact that its microstate (at some suitable earlier time) is simply not one of those which would lead to its entering the container. Should we *expect* a non-trivial causal explanation of that fact? It is worth noting that there is a quite different role that probabilities can play in explanatory contexts, other than that of providing causes: viz., that of guiding our judgements as to what is 'anomalous'—what calls for causal explanation, and what merely needs to be 'normalised' (or given a normalising explanation, in my terminology of 2.2).

Thus arguments such as Popper's and Albert's, based on an appeal to the objectivity of thermodynamic phenomena, needed to be treated with caution. One cannot move from the objectivity of the phenomena to the objectivity of their assumed probabilistic causes, without a supporting argument that the phenomena actually call for causal explanation, in the assumed sense. And yet this is the very point at issue between Causal-General views and their Acausal-Particularist opponents. Appeals to the objectivity of the phenomena simply do not decide the matter. In particular, the phenomena themselves are 'nomically neutral', and to read a regularity as a law is simply to beg the relevant question.[16]

So far, then, we do not have an argument for the Causal-Generalist's asymmetric causal engine. I now want to turn to a very general reason for doubting that there is work for such an engine to do.

2.7 The counterfactual containment problem

Suppose that it is argued that we need an objective causal mechanism to explain the universal tendency of physical systems to reach and maintain thermodynamic equilibrium. Causal claims are required to support counterfactuals. If events of type A cause events of type B, then—in general, at least—it ought to be true of particular cause–effect pairs of these kinds that if the A-type event hadn't happened, the B-type event wouldn't have happened. (Sometimes the B-type event might happen anyway, for other reasons, but if this were always true, what causal–explanatory work would the A-type events be doing?)

In the present case, then, someone who thinks that some causal mechanism M underlies the fact that entropy does not decrease should claim that if weren't for this mechanism, things would not be as we observe them to be: if it weren't for M, entropy would decrease, at least in some of the cases we observe.[17] If it weren't for M, our pressurised gas would be confined to its open container, or at least diffuse at a different rate. However, this counterfactual claim seems unjustified. Assuming the mechanism M in place, we seem to have no reason to expect that if it suddenly failed, things would be other than we observe them to be, in thermodynamic respects.

The argument is simple. Anti-thermodynamic behaviour requires an abnormal microstate, in the terminology with which we began. So the counterfactual in question amounts to the following:

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[16] There is an analogy here with the structure of certain arguments against Humean antirealism about physical necessity. Realist opponents sometimes accuse Humeans of making physical regularities 'mere' uncaused coincidences. But having rejected the view of causation which gives the notion of mere coincidence its opposition, Humeans are entitled to point out that the realists are presupposing the very point at issue.

[17] Albert has proposed that such a mechanism might be provided by the GRW interpretation of quantum mechanics. In Albert ([1994], p. 677) he accepts the relevant counterfactual, at least implicitly, in taking seriously the objection that if his suggestion were correct, entropy would not increase in systems containing too few constituents to allow the GRW mechanism to have its effect. And in Albert ([1998], p. 16) he accepts it explicitly, saying that it is 'perfectly right ... that anybody who claims that one or another causal mechanism called M is what actually underlies the tendencies of the entropies of thermodynamic systems to increase must also be claiming that if that mechanism were not *operating* then would *be* no such tendencies.' See also Albert ([2000]), ch. 7.

(1) If it were not for the mechanism M, the system in question would occupy an 'abnormal' (entropy-reducing) microstate.

But why should we accept this? The supposition that we do not have a causal mechanism to ensure that the system will *not* be in an abnormal state, does not give us any reason to think that the system *will* occupy an abnormal state. (It is not as though physical systems are like adolescents, choosing an outlandish course unless explicitly prevented from doing so.) Our only guides as to what to expect in the imagined counterfactual situation are our epistemic probabilities. If these derive from a version of Boltzmann's statistical method, epistemically construed, then with overwhelming epistemic probability, we should expect the behaviour of the system to be 'normal'. We should think this:

(2) If it were not for the mechanism M, the system in question would behave just as it actually does behave.

Even if we don't have Boltzmann's probabilities to guide us, we still don't have reason to accept (1), unless we have some *other* basis for epistemic probabilities—a basis which assigns high probability to an entropy-reducing states in the imagined counterfactual circumstances. And it is hard to think of what such a basis could be, short of a question-begging appeal to the desired conclusion itself—i.e., to the assumption that M actually is a cause of 'normal' behaviour.

Note the burden of proof here. *It is the proponents of Causal-General views who need a justification for these counterfactual claims*. Their Particularist opponents need only say 'We see no reason to accept such a counterfactual claim.' Hence it is not sufficient for the Causal-General view to object that its opponents have no basis for a contrary counterfactual claim.[18]

I conclude that claims that the explanation of thermodynamic behaviour requires a causal mechanism should be treated with a great deal of suspicion. So far as I can see, the Causal-Generalist has no basis for the claim that without such a mechanism, the

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[18] Albert ([1998]) seems to me to miss this point, in failing to notice that the following supposed response to the above objection is in fact a serious problem for his own view:

> If the GRW-theory is right, then there simply *are* no [Boltzmann-like probability distributions over initial conditions]; and so (of course) there would be no such distributions to fall back on in the event that one were to entertain a counterfactual sort of GRW-theory with the spontaneous localizations removed.

It is Albert who needs to justify the counterfactual (1), not his opponents who need to refute it. 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—that is, of a positive commitment to (1).

behaviour of matter would be any different than it actually is. I call this the 'counterfactual containment problem'.[19]

2.8 The rule of law?

A Causal-Generalist might concede that there are no grounds to affirm that entropy *would* decrease, in the absence of the causal factor she takes to ensure that entropy does not decrease. Nevertheless, she might say, there would still be a difference. What would be missing would be a *guarantee* that entropy would not decrease. In other words, it would no longer be a *law* that entropy does not decrease.

Thus the claim is that it is the causal factor which makes non-decrease a *law-like* matter. Without that factor it might obtain in any case, but it wouldn't be *guaranteed. Inter alia*, then, the causal factor removes the need to qualify our expectation that entropy will not decrease with a *ceteris paribus* condition. The Causal-General theorist thus concedes that what we observe is not unexpected *given lack of low entropy future boundary conditions*, but says that we need asymmetric objective probabilities to exclude that possibility—to make it nomologically 'illegal'.

However, this response takes for granted that have good reason to think the observed monotonic increase in entropy is law-like. In particular, it assumes that we have a good reason to accept a proposition (a statement of a law) from which it follows that we are entitled to assign negligible credence to the possibility that entropy will eventually decrease towards the future, as it actually decreases towards the past. In other words, it assumes that we are already entitled to rule out (to all intents and purposes) the possibility of a low entropy future boundary condition (at least within what might loosely be termed 'cosmological time', rather than the vastly longer periods required for a mere fluctuation to produce low entropy at the current level). If this were not so, we would not be justified in accepting the law-like proposition of which it is a consequence.

But whence could derive this confidence about the future? If there were an argument for asymmetric dynamical probabilities with this consequence, it would rest on what we observe in the region of the universe to which we have access. But do we have good reason to think that this region is typical? In particular, can we legitimately rule out the possibility that the universe is time-symmetric on a larger scale—that the same asymmetries occur with the opposite orientation elsewhere? It would be question-begging

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[19] It might be objected that the above argument does not touch one of the kinds of the view I counted in 2.1 as Causal-Universal, namely the approach which relies on 'absence of initial correlations'. After all, isn't it true that if entropy-decreasing correlations were not absent initially, entropy would decrease? Indeed, but the triviality of the point reflects the question-begging nature of some such approaches. Approaches of this kind appear to face a dilemma. If lack of initial correlations means nothing stronger than lack of exactly what it would take for entropy to decrease in the future, then the condition is trivial—it has no explanatory force, and cannot constitute a nomological 'second asymmetry'. If on the other hand it does amount to a stronger condition, then it is not trivial, but is subject to the counterfactual containment problem. I discuss these issues in more detail in Price ([1996]), ch. 2, and especially Price ([forthcoming]).

to rule out this possibility on the basis of probabilities whose own justification lay in the local asymmetry. The point is an old one. Because induction assumes that the future is like the past, it is powerless in the face of a theoretically well-motivated hypothesis to the contrary.[20]

In other words, the case for objective asymmetry in dynamics faces a dilemma. On one hand, if the probabilities it advocates are law-like—that is, if they are such that in the light of a belief that they obtained, it would be reasonable for us to be almost certain that reversal of the thermodynamic arrow never occurs—then we should not adopt such a belief. For we have good reason to keep an open mind on the latter issue, at least until the relevant cosmology is better understood. On the other hand, if the probabilities it advocates are not such as to make it unreasonable to believe that entropy might decrease in the distant future, then they do not need to be asymmetric—matching future-to-past probabilities would not be incompatible with the fact that entropy decreases towards the past.

Where does this leave us? First, it undermines the proposed response to the counterfactual containment problem. The response conceded that there were no grounds for the claim that without an asymmetric causal factor, entropy would not increase; but suggested that the difference would be that this increase would no longer be a law-like matter. We have seen that in assuming that it is a law-like matter, the Causal-Generalist simply begs the question against her opponent.

Secondly, however, our discussion reveals that the second horn of the our fundamental dilemma from 2.4 offers no escape from the conclusion which made the first horn seem unacceptable—the conclusion that our confidence that entropy will not decrease towards the future must rest on what we know about why it does decrease towards the past. Embracing the second horn—adding a second temporal asymmetry to our explanatory model—seemed to offer a means of avoiding that conclusion, in the form of law-like probabilities capable of 'trumping' future boundary conditions. We now see that the apparent advantage was illusory. The law-like model remains epistemologically subordinate to our best independent assessment about the possibility likelihood of the relevant boundary conditions. In this sense, too, then, the second asymmetry buys us nothing new.[21]

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[20] This is not to deny that there could be present observational evidence of relevance to the issue as to whether there is a future low entropy boundary condition, analogous to that in the past. On the contrary, the existence of such evidence seems a live and fascinating possibility. (See Price [1996], pp. 99–111 and Callender [1998], pp. 144–46, for an introduction.) However, this is evidence of a different kind than our 'local' evidence for dynamical probabilities. My point is that the latter evidence cannot be projected without question into the distant future, in the face of a well-motivated hypothesis to the effect that other factors might then be in play.

[21] Note the difference in the two routes to same endpoint. If we choose the first horn of the fundamental dilemma, we take it that we have grounds for accepting a generalisation which—in virtue of being time-symmetric and not excluding a low entropy past—cannot exclude the possibility of a low entropy future. If we choose the second horn, however, then our goal is an asymmetric generalisation which would be sufficiently

2.9 Summary

We began (2.1) with two conceptions of the 'contrast class' for the explanation of the asymmetry of thermodynamic phenomena. This led us to two views of the time-asymmetric explanandum itself. I have argued that one of these, the Causal-General approach, has the disadvantage of requiring two time-asymmetries in nature, with no compensating benefit—on the contrary, the causal work claimed for the second asymmetry seems not to require doing.

According to the alternative Acausal-Particular view, the single time-asymmetric explanandum is the low entropy past 'boundary condition', responsible for the many individual processes whose time-asymmetry in nature comprises the observed thermodynamic asymmetry. I associated this view with Boltzmann's statistical approach, and argued that it has the surprising consequence that any prediction about the future course of thermodynamic behaviour needs to pay attention to our understanding of the low entropy past. The hope that the Causal-General approach could avoid this conclusion turned out to be illusory. Until we know more about the reasons for the 'anomaly' of the low entropy past, we are simply not justified in accepting a dynamical model which rules out such a future.

In the remainder of the paper I turn to the 'big anomaly' itself. For one thing, I want to respond briefly to the common suggestion that, as an initial condition, this feature of the universe does not call for explanation. This suggestion is not incompatible with the conclusion that there is no other time-asymmetric explanandum in thermodynamics—taken together with that conclusion, it merely implies that there is no further explanatory work to be done. However, it seems to me that the suggestion is mistaken, and rests in part on a temporal double standard—on a failure to adopt consistent explanatory standards with respect to the past and future. I also want to respond briefly to an argument by Larry Sklar, who has claimed that an appeal to cosmological initial conditions does not provide the 'elusive object of desire' concerning the asymmetry of thermodynamics. I want to suggest that Sklar's argument is evaded by a Particularist conception of the explanatory task.

3 The big anomaly

3.1 Initial smoothness

What kind of boundary conditions, at what times, are needed to account for the time-asymmetry of observed thermodynamic phenomena? What 'anomaly' do we need to

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law-like to exclude the latter possibility, but the problem is that we are not justified in postulating such law, unless we have independent reason for excluding the possibility of a low entropy future.

understand, if we are to predict the future with confidence? There seems no *a priori* reason to expect any particularly coherent answer to these questions. Remarkably, however, it seems that a single simply-characterizable condition may do the trick. Leaving aside the possibility of subtle yet-to-be-discovered effects of a future boundary condition, it seems likely that *all* the observed 'disequilibrium' can be attributed to a single characteristic of the universe soon after the Big Bang. If we let the laws fix the space of possibilities, and then impose this condition, there are no obvious anomalies left unaccounted for. *All* the anomalous order in the present universe seems unexceptional, once we conditionalise on this one unlikely condition in the early universe.[22]

What is this condition? It is that the matter in the universe be distributed extremely evenly, immediately after the Big Bang. It may seem puzzling that this should be described as a state of low entropy, but the key lies in the properties of gravity. In a system dominated by a universal attractive force, a uniform distribution of matter is highly unstable—the 'natural' thing for such matter to do is to clump together. (Think of it by analogy with a huge collection of sticky polystyrene foam pellets—their natural tendency is to stick together in large agglomerations.) To get a sense of how extraordinary it is that matter should be distributed uniformly near the Big Bang, keep in mind that in the absence of any objective basis for a distinction, the Big Bang may equally be regarded as the end point of a gravitational collapse. For such a collapse to produce a very smooth distribution of matter is, to put it mildly, quite surprising.[23]

What is remarkable about this feature of the universe is not merely that it is so staggeringly anomalous, in terms of ordinary conceptions of how gravitating matter might be expected to behave; but also that, so far as we know, it is the *only* anomaly necessary to account for the vast range of low entropy systems we find in the universe. In effect, initial smoothness seems to provide a vast 'reservoir' of low entropy, that everything else draws from. The most important mechanism is the formation of stars and galaxies. Smoothness is necessary for galaxy and star formation, and most irreversible phenomena with which we are familiar owe their existence to the sun. (The best account of these ideas I know is that by Penrose [1989], ch. 7.)

In my view, this plausible hypothesis about the origins of low entropy is perhaps the most remarkable result of late twentieth century physics. It is under appreciated, I think, for three main reasons. First, it hasn't been properly appreciated that the real puzzle of time-asymmetry in thermodynamics *is* the question why entropy is low in the

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[22] Of course, it is trivial that there is *some* condition G such that what we observe has high probability, conditional on G—after all, we could conditionalise on the actual history of the universe. What is significant is that there seems to be a condition which will do the trick which is defined in terms of macroscopic parameters at a single stage in the universe's history.

[23] American 'quite'. Imagine throwing billions of sticky foam pellets into a tornado, and having them land in a perfect sheet, one pellet thick, over every square inch of Kansas—that's an easy trick, by comparison.

first place—so it hasn't been appreciated to what extent this cosmological hypothesis gives concrete form to an issue which has plagued physics for 125 years. Second, it hasn't been realised that an understanding of the low entropy past is essential to any well-informed judgement about the future. And third, an anthropocentric temporal bias, according to which we are inclined to take initial conditions as simply given, makes it difficult for us to appreciate what an extraordinary discovery it is—at least according to all existing conceptions of how matter should behave—that the universe should be in this state at this time.

The puzzle of initial smoothness thus gives concrete form to the explanatory project which begins with the time-asymmetry of thermodynamics. If cosmology could explain initial smoothness, the project would be substantially complete. I'll say more below about the task of explaining initial smoothness, especially in the light of the apparent time-symmetry of the available laws. In particular, I'll discuss the common suggestion that it doesn't need explaining—that it is somehow inappropriate to ask for an explanation of an initial condition. Before that, however, I want to respond to a prominent recent argument that such an appeal to cosmology cannot, after all, provide the elusive understanding of the temporal asymmetry of thermodynamic phenomena.

3.2 Cosmology and branch systems

Larry Sklar asks whether an appeal to cosmological features, including initial smoothness, can 'account for the most problematic fact of the world we want to account for in statistical mechanics, the Second Law behaviour of "small", individual systems?' ([1993], p. 318) He argues that cosmological initial conditions cannot explain one of the most striking features of the thermodynamic asymmetry, namely, that it has the same orientation wherever we look—all 'branch systems' are aligned the same way. He concludes that '[t]hat elusive object of desire, the Second Law, has once again eluded our search for its explanatory ground.' ([1993], p. 329)

Sklar's criticism of the traditional branch systems approach seems to me entirely correct, so far as it goes. However, I think the basic problem lies in the fact that that traditional approach asks the wrong question about branch systems. In effect, it asks for an explanation of a time-asymmetric generalisation—a time-asymmetric constraint on branch systems *in general*—and tries to extract an answer from raw statistical considerations alone. Because the underlying probabilities are time-symmetric, there is no acceptable answer to the question posed in this form. (Sklar argues convincingly that the tradition misses this, by failing to consider the 'final' end of a branch system.)

But suppose that we abandon the search for a general law-like constraint, and begin instead with the particular puzzle of the sheer *existence* of low entropy branch systems (in vastly greater numbers than predicted by the Boltzmann statistics). As we have seen, the answer to this puzzle seems to lie in an understanding of a condition of the universe in the distant past. Without initial smoothness, there simply wouldn't be such low entropy branch systems.

Against this background, we now turn to a 'local' generalisation. Among those low entropy branch systems that there are (at least in our region) all show the same temporal orientation—in all of them, the entropy curve slopes 'downwards' towards the past. It seems that this fact may be explained, not by any asymmetric factor which 'forces' the entropy of a branch system to increase towards the future, but simply by the time-symmetric Boltzmann probabilities, in conjunction with the hypothesis that there is a low entropy boundary condition in the past. The crucial point is that in terms of the Boltzmann probabilities, the 'cheapest' (most probable) way to reach a specified low entropy boundary is to get there monotonically (or almost monotonically—the expected fluctuations from monotonicity are very small, compared to the overall gradient). The most likely universe with a single low entropy boundary will be one in which all entropy gradients in quasi-isolated subsystems slope down towards that boundary. In other words, the observed behaviour is unexceptional, in the restricted probability measure which results by conditionalising on the boundary condition. In this measure, in general, a single oppositely-oriented branch system will be as unlikely as it would be in the absence of any boundary condition. We should expect reverse-oriented branch systems—branch systems exhibiting anti-thermodynamic behaviour, by ordinary standards—no more often than we should expect them occur by random fluctuations in a universe in equilibrium. Imposing the condition that a gas be in pressurised bottles 'initially' does not make it any more likely that one of those bottles will spontaneously repressurise, at some 'later' time.

Of course, all of this needs to be back up by some numerical arguments. A good place to start would be with elaborations of the Ehrenfest Urn models, which have sometimes been used to model the effects of boundary conditions on thermodynamic systems (see, for example, Cocke [1967] and the references in Schulman [1997], p. 156). Using sub-urns to represent branch systems, the result to be expected is that in almost all trajectories compatible with a single ordered boundary condition on the system as a whole, the accumulation of order in the sub-urns is parallel and monotonic. My point here has been a conceptual one: the possibility of an explanation of this kind depends on the shift from a conception of the explanandum as a law-like generalisation, to a conception of it as a 'local' or particular matter.

By way of analogy, consider the task of explaining fact that we terrestrial organisms exhibit biochemical 'parity violations' in certain ways—we use say right-handed versions of organic molecules such as DNA, when the mirror-image biochemistry would work just as well. (This example is used with a different point by Sklar himself [1993], p. 383.) Does this indicate the need for some law-like parity violation? No—the imbalance may simply stem from a single ancestral accident, early in the history of life on this planet. Similarly, the Acausal-Particular view is that the parallelism of the entropy gradient of low entropy branch systems, far from indicating some law-like time-asymmetry, is simply a manifestation of a single historical event. Whether that event should be said to be accidental, as in the biological case, remains to be seen. The crucial point is that it is sufficient to explain the regularity we observe, *so long as we don't take ourselves to be looking for something stronger*—so long as we don't take ourselves to be looking for the origins of a *law*, or a generalisation of greater scope.[24]

3.3 Do the initial conditions need explanation?

According to the Acausal-Particular view, then, all the explanatory burden of the time-asymmetry of thermodynamics ends up in the puzzle of the low entropy initial conditions. In the light of late twentieth-century cosmology, this puzzle now takes concrete form. Why is the universe smooth, soon after the Big Bang?

Does this need explanation? Some people say not. For example, Craig Callender suggests that 'the whole enterprise of explaining global boundary conditions is suspect, for precisely the reasons Hume and Kant taught us, namely, that we can't obtain causal or probabilistic explanations of why the boundary conditions are what they are.' (Callender [1997], p. 69. See also Callender [1998], pp. 149–50, and Sklar [1993], pp. 311–312, for similar concerns.) However, it seems to me that this attitude to the explanation of initial conditions is on shaky ground. Would it take the same view of the need to explain an equivalent condition at any other time? If so, it is perilously close to a kind of global explanatory nihilism, which answers every 'Why?' question with the answer that things had to be some way, so why not this way? If not, on the other hand, then the proponent of this 'no need to explain initial conditions' view needs to tell us what is special about (what we call) *initial* conditions.

The threat here is a temporal double standard—an unjustified discrimination on the basis of temporal location or orientation. Suppose we found that 'miraculous' anti-thermodynamic behaviour occurs as we approach black holes. Presumably we would take that to call for explanation. Indeed, we would take it to do so even if it were found to occur only towards a single black hole. In that case, we would want to know what distinguished that singularity from others—what made that one so abnormal. But except for the difference of temporal direction, that is *exactly* what find to be the case towards the Big Bang—unless we count the fact that the Big Bang is not just any old singularity,

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[24] Another conceptual aid at this point is the temporal reorientation I called Boltzmann's about-turn. If instead of asking ourselves why entropy in each branch system goes up towards the 'future', we ask why it goes down towards the 'past', I think it immediately seems much more plausible that the solution lies in something common in that direction, some single condition to which all non-equilibrium branch systems (in our experience) owe their low entropy.

which surely ought to make us more rather than less inclined to seek an explanation for its strange condition.

I am not claiming that boundary conditions *always* call for explanation, but only that at boundaries, as elsewhere, our explanatory appetites are properly guided by the principles we already accept. On this basis, boundaries conditions which appear abnormal in the light of what we take to be the laws do call for further explanation—and this principle should be applied in a temporally unbiased way. Initial smoothness calls for explanation because—just as much as would 'final' smoothness—it conflicts so radically with our ordinary expectations as to how gravitating matter should behave.

Let me put this more explicitly. Suppose, for a moment, that in the past thirty years or so, physics had discovered that the matter in the universe is collapsing towards a Big Crunch, fifteen billion years or so in our future—and that as it does so, something very, very extraordinary is happening. The motions of the individual pieces of matter in the universe are somehow conspiring to defeat gravity's overwhelming tendency to pull things together. Somehow, by some unimaginably intricate balancing act, the various forces are balancing out, so that by the time of the Big Crunch, matter will have spread itself out with great uniformity. A butterfly—nay, a molecule—out of place, and the whole house of cards would surely collapse![25]

As a combination of significance and sheer improbability—the latter judged by well-grounded existing conceptions of how matter might be expected to behave—this discovery would surely trump anything else ever discovered by physics. Would it really be plausible to suggest that physicists should sit on their hands, and not even *try* to explain it? (They might fail, of course, but that's always on the cards—the issue is whether it is appropriate to *try*.) If this discovery didn't call for explanation, then what conceivable discovery ever would?

In my view, however, this state of affairs is *exactly* what physics has discovered! I have merely described it in unusual language, reversing the usual temporal conventions. By my second presupposition at the beginning of the paper, this redescription has no objective significance. If it is a proper matter for explanation described one way, it is a proper matter for explanation described the other.

If this weren't reason enough, a further motivation for seeking an explanation of initial smoothness emerges from the argument of this paper. On the explanation of this puzzling past condition depends no less a matter than our best-grounded view of the ultimate fate of the universe, at least in thermodynamic respects. To make a well-informed judgement about the future, we need to understand the past. In these

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[25] Why? Because, as Albert ([2000], p. 151) puts it, 'the property of being an *abnormal* microstate is extraordinarily *unstable* under small perturbations'. By the lights of the Boltzmann measure, then, the tiniest disturbance to our imagined entropy-reducing universe would be expected to yield an entropy-increasing universe.

circumstances, to take the view that the past was what it was, and that to ask for explanation is misguided, is to condemn ourselves to the same view of the future—*que sera, sera*. Perhaps we can't do any better than this, but again, what could justify the view that we shouldn't even try?

This is not to claim that the nature of the explanatory task is unproblematic, of course. There are certainly conceptual problems in this area, as there are in cosmology in general—not least of them the fact (if it is a fact) that the universe is a 'one-off'. I want to finish by addressing just one of these problems, which might seem to lead to a particular difficulty for the Acausal-Particularist approach to the thermodynamic asymmetry.

If we ask what would count as an explanation of initial smoothness, the natural thought is to seek some further theoretical constraints on how the universe might be, in the light of which the smooth early universe no longer seems abnormal. There seem to be two main possibilities: (i) some sort of law-like narrowing of the space of possibilities, so that such a universe no longer counts as abnormal; or (ii) some sort of incorporation of 'our' universe into a larger ensemble of equally real universes, together with an explanation for the fact that we find ourselves in such an abnormal one (e.g., because intelligent life could not exist in most of the others). For present purposes, I'll ignore the latter ('anthropic') strategy.

It might seem that the first strategy reveals a deep inconsistency in the general approach I have advocated. Wouldn't it amount to a revision of our background probability measure on the space of possible histories for the universe, so that this measure would no longer be time-symmetric? In other words, doesn't it conflict with the attempt to retain time-symmetric probabilities, and hence with the basic explanatory structure of the statistical approach, as I have characterised it?

No, for two reasons. First, it need not make the global probability measure time-asymmetric. It might turn out to have the consequence that entropy would decrease towards a Big Crunch, as well as towards the Big Bang. In this case, the asymmetry of the measure in our vicinity would be balanced by a reverse asymmetry elsewhere.

More importantly, however, there might still be a good case for 'factoring' the probability measure into two components: a background time-symmetric component, which accurately predicts the behaviour of matter in either temporal sense, wherever it is not subject to the constraints associated with cosmological boundaries; and an additional component, which reflects the influence of such boundaries. A loose analogy here might be the Newtonian distinction between inertial and non-inertial motion. This distinction is useful and theoretically important, even though true inertial motion is rare or even nonexistent. Many systems can usefully be treated as inertial, for particular experimental purposes. Similarly, it might well be useful to distinguish the 'local', symmetric dynamical behaviour of matter from its behaviour under the influence of cosmological low entropy boundary conditions. Even if 'pure' cases of the former kind are unknown in our region, there are many instances—long-isolated systems in equilibrium—in which symmetric probabilities give the right results, to high accuracy, over limited times. Moreover, as long as the underlying dynamics remain time-symmetric, this too will favour a two-level theoretical structure, in which probabilities at the first level—those taken to associated simply with local dynamics—remain time-symmetric. So there may be very good theoretical grounds for the kind of model I've proposed—a two-level account of the observed asymmetry, with time-symmetric probabilities at the first level.

Are there precedents for the use of probabilities which factor in this way? Perhaps—think of the familiar example of a minting machine which randomly produces coins of variable bias. The net objective probability is a resultant of two factors. We could easily vary this example to bring it closer to the present case. Imagine a society who make coins from magnetic material. Their coins behave like ours in the absence of magnetic fields, but differently on the surface of the Earth. In the northern hemisphere, Heads is more likely; in the southern hemisphere, the reverse. Even without the benefit of southern exposure, astute northerners might manage to figure this out. They would learn to factor the probabilities accordingly, into mechanical and magnetic components.

3.4 Conclusion

In sum, the project of explaining initial smoothness—the cosmological boundary condition on which all the observed time-asymmetry of thermodynamics seems to depend—does not seem to be inappropriate or misconceived. On the contrary, it seems to be extremely well-motivated, by ordinary scientific standards, once temporal double standards are avoided. And although there certainly puzzles about the nature of explanation in the cosmological context, the project of explaining initial smoothness does not seem to be in tension with the statistical approach, in the light of which it assumes such importance. With the Acausal-Particularist, then, I conclude that this project, and it alone, is the legitimate heir to the old puzzle of the temporal asymmetry of the Second Law. A solution to this new puzzle is not yet in hand. Indeed, it is not yet clear what a solution would look like. But we make considerable progress in seeking the right object, in barking at the right bush.

Acknowledgements

This paper began life as a contribution to a symposium on the Direction of Time at the Annual Meeting of the Eastern Division of the APA in Washington, D.C., December, 1998. I am grateful to David Albert, Larry Sklar and John Earman for comments on that occasion, and to audiences of related talks more recently in Utrecht, College Park and Tucson. I am also grateful to David Atkinson, Frank Arntzenius, Harvey Brown, Craig

Callender, Jason Grossman, Stephen Leeds and Jos Uffink for conversations about this material over several years; to two anonymous referees; and to Jill North, for a paper-length commentary on the penultimate version.

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