Séminaire Poincaré XV Le Temps (2010) 115 – 140

Séminaire Poincaré

Time's Arrow and Eddington's Challenge

Huw Price
Centre for Time
Main Quad A14
University of Sydney
NSW 2006
Australia

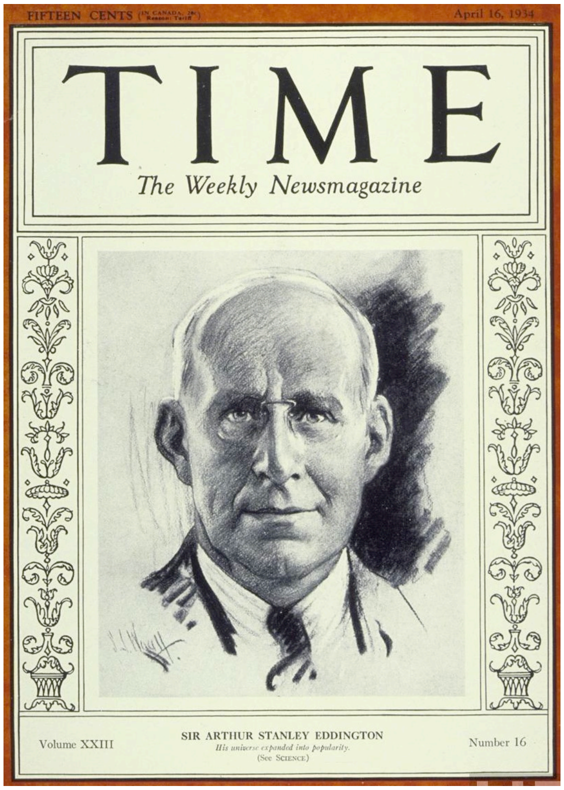

Figure 1 – Sir Arthur Eddington (1882–1944).

1 A head of his time

The phrase 'time's arrow' seems to have been first introduced to physics by Sir Arthur Eddington, in *The Nature of the Physical World* (1928), [14] based on his Gifford Lectures in Edinburgh the previous year. Eddington's work is little-known to contemporary readers, but he was one of the leading scientific writers of his day. He even reached the cover of TIME magazine, in 1934. (See Figure 1—the inscription beneath Eddington's name reads "His universe expanded into popularity.")

When Eddington died, ten years later, TIME reported that "one of mankind's most reassuring cosmic thinkers" had passed away :

> Death came at 61 to cool, unruffled Sir Arthur Stanley Eddington, Cambridge University astronomer. ... To scientists, Sir Arthur was affectionately known as the senior partner in the firm of "Eddington & Jeans, Interpreters of the Universe." Shy, neat, reed-nosed Sir Arthur looked precisely like the British university don he was, and he discoursed on his cosmic subject with a wit and clarity rare among scientists.

Eddington was astute, as well popular, and for those of us interested in the physics and philosophy of time, wit and clarity are not the only reasons to go back to his work. About time's arrow itself, in fact, his famous clarity is sometimes missing. What he himself means by 'time's arrow' is not always entirely clear. I think it is fair to say that he was actually discussing several different notions, and does not completely succeed in distinguishing them, or understanding the connections between them. But there are gems, too, and in some respects Eddington was well ahead not only of all his contemporaries, but also of most writers since.

In this paper, I want to try to provide some clarity, and review progress, concerning some of the issues Eddington discusses, under the heading 'time's arrow'. In some respects, as I'll explain, we have made a lot of progress since Eddington's day. If we haven't found all the answers, at least we have a better understanding where the true puzzles lie. In other respects, I think, progress has not been fast, or extended very far – but it might be encouraged, I suggest, by reminding ourselves of some of the elements of Eddington's discussion of these problems.

2 Introducing 'time's arrow'

Let's begin with the passage in Eddington's book in which the term 'Time's Arrow' makes its first appearance :

> *Time's Arrow.* The great thing about time is that it goes on. But this is an aspect of it which the physicist sometimes seems inclined to neglect. In the four-dimensional world ... the events past and future lie spread out before us as in a map. The events are there in their proper spatial and temporal relation ; but there is no indication that they undergo what has been described as "the formality of taking place" and the question of their doing or undoing does not arise. We see in the map the path from past to future or from future to past ; but there is no signboard to indicate that it is a one-way street. Something must be added to the geometrical conceptions comprised in Minkowski's world before it becomes a complete picture of the world as we know it. ([14], p. 34)

Here already we can usefully distinguish two kinds of elements, which Eddington takes to be missing from Minkowski's four-dimensional picture of the world (in which time and space are treated in much the same way). One missing element is what Eddington elsewhere calls "happening", or "becoming", or the "dynamic" quality of time – the fact that time "goes on", as he puts it in the passage above. Time seems in *flux,* to use a much older term, in a way in which space is not, and Eddington is objecting that this aspect of time is missing from the four-dimensional picture.

The other missing ingredient – which Eddington himself doesn't distinguish from the dynamic aspect of time, but which is usefully treated as a distinct idea – is something to give a *direction* to the time axis in Minkowski's picture ; something to distinguish past from future, as we might say.

Eddington thinks that both these elements are missing from Minkowski's world, and that physics should be trying to put them back in. Since he doesn't distinguish them, he takes for granted that we should be looking for some one element, which would do both jobs. The reason to keep them distinct, in my view, is that in principle we might agree with Eddington about one but not about the other. For example, we might be persuaded that flux, or "becoming" is entirely subjective, and best left out of physics. Famously, this was the view of Hermann Weyl :

> The objective world simply *is,* it does not *happen.* Only to the gaze of my consciousness, crawling upward along the life line of my body, does a section of this world come to life as a fleeting image in space which continuously changes in time. ([33], p. 116)

Yet we might also think that something objective distinguishes one direction of time from the other, even if it isn't some objective process of flux or "happening". (Similarly, perhaps, the other way round : we might think there is something objective about "happening", but treat it as just a conventional matter which way things happen – change is fundamental, but not the *direction* of change !)

So we already have two ideas – not distinguished by Eddington himself – under the heading 'time's arrow'. And we haven't yet got to most important one, which is the time-asymmetry summed-up by the *second law of thermodynamics.* As we shall see later, Eddington sometimes seems deeply puzzled about the relation between this thermodynamic arrow of time and the arrow he calls the "going on" of time. He sometimes seems to think that there might be something more to the latter, accessible only to consciousness. But he seems clear, at least, that the "going on" of time depends on the thermodynamic asymmetry. Considering a region in which matter has already reached thermodynamic equilibrium, he says :

> In such a region we lose time's arrow. You remember that the arrow points in the direction of increase of the random element. When the random element has reached its limit and becomes steady the arrow does not know which way to point. It would not be true to say that such a region is timeless ; the atoms vibrate as usual like little clocks ; by them we can measure speeds and durations. Time is still there and retains its ordinary properties, but it has lost its arrow ; like space it extends, but it does not "go on." ([14], p. 39)

Similarly, returning to this theme in an important lecture published in *Nature* in 1931, he says that in the distant future

> the whole universe will reach a state of complete disorganisation—a uniform featureless mass in thermodynamic equilibrium. This is the end of the world. Time will *extend* on and on, presumably to infinity. But there will be no definable sense in which it can be said to *go* on. ([15], p. 449)

For most of what follows, I shall focus exclusively on the thermodynamic arrow, and especially on the puzzling conflict between the time-asymmetry of thermodynamic phenomena and the time-symmetry of the underlying microphysics on which these phenomena depend. My aim is to provide a guide to the current status of this puzzle, distinguishing the central issue from various issues with which it tends to be confused. In particular, I'll show that there are two competing conceptions of what is needed to resolve the puzzle of the thermodynamic asymmetry, which differ with respect to the number of distinct time-asymmetries they take to be manifest in the thermodynamic arrow. According to one conception, we need two time-asymmetries to explain the thermodynamic arrow ; according to the other, we need only one.

I shall offer some reasons for preferring the one-asymmetry view. On this conception of the origin of the thermodynamic arrow, the remaining puzzle concerns the ordered distribution of matter in the early universe. The puzzle of the thermodynamic arrow thus becomes a puzzle for cosmology. As I'll explain, Eddington's 1931 paper looks surprisingly modern, in this context – Eddington seems to have been the first to put his finger on some of the key points that need to be made at this juncture.

At the end of the paper I shall return to some of Eddington's other ideas – the "going on" of time, and the role of consciousness. I think these ideas haven't stood the test of time quite so well, but that there are still some gems here that deserve to be better known. Even where Eddington takes the wrong turn, we can learn a lot by following such an astute and engaging thinker down a few dead ends.

3 The puzzle of the thermodynamic arrow

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

What has happened to this puzzle since the 1890s ? I suspect that many contemporary physicists regard it as a dead issue, long since laid to rest. Didn't it turn out to be just a matter of statistics, after all ? However, while there are certainly would-be solutions on offer—if anything, as we'll see, too many of them—it is far from clear that the puzzle has actually been solved. Late in the twentieth century, in fact, one of the most authoritative writers on the conceptual foundations of statistical mechanics could still refer to an understanding of the time-asymmetry of thermodynamics as 'that obscure object of desire'. [31]

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

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

Little here is new, but it is surprisingly difficult to find a clear guide to these matters in the literature, either in philosophy or in physics. Accordingly, I think the paper will serve a useful purpose, in helping non-specialists to understand the true character of the puzzle discovered by those nineteenth century giants, the extent to which it has been solved, and the nature of the remaining issues.[1]

4 The true puzzle—a first approximation and a popular challenge

Everyone agrees, I think, that the puzzle of the thermodynamic arrow stems from the conjunction of two facts (or apparent facts—one way to dissolve the puzzle would be to show that one or other of the following claims isn't actually true) :

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

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

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

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

Here's another way to put the point. The thermodynamic arrow isn't just a T-asymmetry, it is a PCT-asymmetry as well. There are many familiar process whose PCT-reversed processes are equally compatible with the underlying laws, but which never happen, in our experience. We might be tempted to explain this asymmetry as

---

[1] For those interested in more details, I discuss these topics at greater length elsewhere.[23, 24, 25, 26]

due to the imbalance between matter and anti-matter, but the above reflections show that this is not so. So instead of the puzzle of the T-asymmetry of thermodynamics, we could speak of the puzzle of the PCT-asymmetry of thermodynamics. Then it would be clear to all that the strange behaviour of the neutral K meson isn't relevant. Knowing that we could if necessary rephrase the problem in this way, we can safely rely on the simpler formulation, and return to our original version of the puzzle.

5 Four things the puzzle is not

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

5.1 The meaning of irreversibility

The thermodynamic arrow is often described in terms of the 'irreversibility' of many common processes—e.g., of what happens when a gas disperses from a pressurised bottle. This makes it sound as if the problem is that we can't make the gas behave in the opposite way—we can't make it put itself back into the bottle. Famously, Loschmidt's reversibility objection rested on pointing out that the reverse motion is equally compatible with the laws of mechanics. Some responses to this problem concentrate on the issue as to why we can't actually reverse the motions (at least in most cases). [29]

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

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

5.2 Asymmetry in time versus asymmetry of time

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

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

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

As we shall see later, Eddington wrestled with these issues. They go a long way to explain why he felt tempted by the view that the true source of the direction of time—the fact that it "goes on", as he put it—is something accessible in consciousness but not in physical instruments. But for present purposes, since our immediate focus is the thermodynamic asymmetry, I shall assume that it is a conventional matter which direction we treat as positive or future time. Moreover, although it makes sense to ask whether time is anisotropic, it seems clear that this is a different issue from that of the thermodynamic asymmetry. As noted, the thermodynamic asymmetry is an asymmetry of physical processes *in* time, not an asymmetry of time itself.

5.3 Entropy gradient not entropy increase

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

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

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

5.4 The term 'entropy' is inessential

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

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

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

5.5 Summary

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

6 What would a solution look like? Two models

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

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

6.1 The two-asymmetry approach

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

1. *The H-theorem.* Oldest and most famous of all, this is Boltzmann's development of Maxwell's idea that intermolecular collisions drive gases towards equilibrium.
2. *Interventionism.* This alternative to the H-theorem, apparently first proposed by S. H. Burbury in the 1890s, [6, 7] attributes entropy increase to the effects of random and uncontrollable influences from a system's external environment.
3. *Indeterministic dynamics.* There are various attempts to show how an indeterministic dynamics might account for the second law. A recent example is a proposal that the stochastic collapse mechanism of the GRW approach to quantum theory might also explain entropy increase. [1, 2]

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

The second point to be stressed is that this asymmetry alone isn't sufficient to produce the observed thermodynamic phenomena. Something which forces entropy to be non-decreasing won't produce an entropy gradient unless entropy starts low. To give us the observed gradient, in other words, this approach also needs a low entropy boundary condition—entropy has to be low in the past. This condition, too, is time-asymmetric, and it's a separate condition from the dynamical asymmetry. (It is not guaranteed by the assumption of molecular chaos, for example.)

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

6.2 The one-asymmetry model

The two-asymmetry model isn't the only model on offer, however. The main alternative was first proposed by Boltzmann in the 1870s,[3] in response to Loschmidt's famous criticism of the H-theorem. To illustrate the new approach, think of a large collection of gas molecules, isolated in a box with elastic walls. If the motion of the molecules is governed by deterministic laws, such as Newtonian mechanics, a specification of the microstate of the system at any one time uniquely determines its entire trajectory. The key idea of Boltzmann's new approach is that in the overwhelming majority of possible trajectories, the system spends the overwhelming majority of the time in a high entropy macrostate—among other things, a state in which the gas is dispersed throughout the container. (Part of Boltzmann's achievement was to find the appropriate way of counting possibilities, which we can call the *Boltzmann measure.*)

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

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

7 Which is the right model ?

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

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

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

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

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

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

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

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

8 The Boltzmann-Schuetz hypothesis—a no-asymmetry solution?

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

Boltzmann himself was one of the first to see the importance of this issue. In a letter to *Nature* in 1895, he suggests an explanation, based on an idea he attributes to 'my old assistant, Dr Schuetz'. [4] He notes that although low entropy states are very unlikely, they are very likely to occur eventually, given enough time. If the universe is very old, it will have had time to produce the kind of low entropy region we find ourselves inhabiting simply by accident. 'Assuming the universe great enough, the probability that such a small part of it as our world should be in its present state, is no longer small,' as Boltzmann puts it.

It is one thing to explain why the universe contains regions like ours, another to explain why we find ourselves in such a region. If they are so rare, isn't it more likely that we'd find ourselves somewhere else? But Boltzmann suggests an answer to this, too. Suppose, as seems plausible, that creatures like us couldn't exist in the vast regions of near-equilibrium between such regions of low entropy. Then it's no

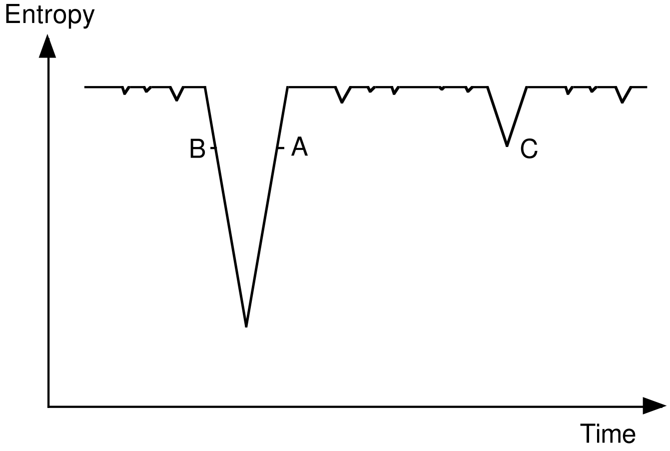

Figure 2 – Boltzmann's entropy curve.

surprise that we find ourselves in such an unlikely place. As Boltzmann himself puts it, 'the ... *H* curve would form a representation of what takes place in the universe. The summits of the curve would represent the worlds where visible motion and life exist.'

Figure 2 shows what Boltzmann calls the *H* curve, except that this diagram plots entropy rather than Boltzmann's quantity *H.* Entropy is low when *H* is high, so the summits of Boltzmann's *H* curve are the troughs of the entropy curve. The universe spends most of its time very close to equilibrium. But occasionally—much more rarely than this diagram actually suggests—a random re-arrangement of matter produces a state of low entropy. As the resulting state returns to equilibrium, there's an entropy slope, such as the one on which we (apparently) find ourselves, at a point such as A.

Why do we find ourselves on an uphill rather than a downhill slope, as at B ? In another paper, Boltzmann offers a remarkable proposal to explain this, too. [5] Perhaps our perception of past and future depends on the entropy gradient, in such a way that we are bound to regard the future as lying 'uphill'. Thus the perceived direction of time would not be objective, but a product of our own orientation in time. Creatures at point B would see the future as lying in the other direction, and there's no objective sense in which they are wrong and we are right, or vice versa. Boltzmann compares this to the discovery that spatial up and down are not absolute directions, the same for all observers everywhere.

We shall return to this aspect of the Boltzmann-Schuetz hypothesis—its elimination of an objective *direction* of time—in a moment, and compare it to Eddington's views on the matter. For moment, however, what matters about the hypothesis is that it offers an explanation of the local asymmetry of thermodynamics in terms which are symmetric on a larger scale. So it is a no-asymmetry solution—the puzzle of the thermodynamic asymmetry simply vanishes on the large scale.

9 The big problem

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

Why does this consequence follow ? Well, imagine that the universe is vast enough to contain many separate fluctuations, each containing everything that we see around us, including the complete works of Shakespeare, in all their twenty-first century editions. Now imagine choosing one of these fluctuations at random. It is vastly more likely that we'll select a case in which the Shakespearean texts are a product of a spontaneous recent fluctuation, than one in which they were really written four hundred years earlier by a poet called William Shakespeare. Why ? Simply because entropy is much higher now than it was in the sixteenth century (as we normally assume that century to have been). Recall that according to Boltzmann, probability increases exponentially with entropy. Fluctuations like our twenty-first century—'Shakespearian' texts and all—thus occur much more often in typical world-histories than fluctuations like the lower-entropy sixteenth century. So almost all fluctuations including the former don't include the latter. The same goes for the rest of history—all our 'records' and 'memories' are almost certainly misleading.

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

So the Boltzmann-Schuetz hypothesis implies that our apparent historical evidence is almost certainly unreliable. One of the first authors to make this point in print was Carl Friedrich von Weizsäcker, in 1939. [32] Weizsäcker notes that 'improbable states can count as documents [i.e., records of the past] only if we presuppose that still less probable states preceded them.' He concludes that 'the most probable situation by far would be that the present moment represents the entropy minimum, while the past, which we infer from the available documents, is an illusion.'

Weizsäcker also notes that there's another problem of a similar kind. The Boltzmann-Schuetz hypothesis implies that as we look further out into space, we should expect to find no more order than we already have reason to believe in. But we can now observe vastly more of the universe than was possible in Boltzmann's day, and there seems to be low entropy all the way out.

So the Boltzmann-Schuetz hypothesis faces some profound objections. Fortunately, modern cosmology goes at least some way to providing us with an alternative – an option noted with great prescience by Eddington himself, in the 1931 paper to which we have already referred. Anticipating Weizsäcker by several years, Eddington sets out very clearly why we need this alternative, and cannot rely simply on chance fluctuations, as Boltzmann and Schuetz had suggested :

> [I]t is practically certain that a universe containing mathematical physicists will at any assigned date be in the state of maximum disorganization which is not inconsistent with the existence of such creatures. ([15], 452)

In other words, a region of space and time containing some intelligent creatures—mathematical physicists, as Eddington puts it, setting the bar for intelligence a little higher than usual—will almost certainly contain very little of the ordered past those creatures *believe* themselves to have ; and very little additional order, when they investigate new realms.

But what is the alternative to relying on chance to produce the low entropy we observe ? In Eddington's view, it amounts to this :

> We are thus driven to admit anti-chance ; and apparently the best thing we can do with it is to sweep it up into a heap at the beginning of time. ([15], 452)

Eddington does not regard this as a novel suggestion. On the contrary, he regards it as implicit in the physics of "the last three-quarters of a century" :

> There is no doubt that the scheme of physics as it has stood for the last three-quarters of a century postulates a date at which either the entities of the universe were created in a state of high organisation, or pre-existing entities were endowed with that organisation which they have been squandering ever since. Moreover, this organisation is admittedly the antithesis of chance. It is something which could not occur fortuitously. ([14], 84)

One of the remarkable developments in recent decades has been that cosmology now tells us quite a lot about this heap at the beginning of time.

10 Initial smoothness

The observed thermodynamic asymmetry requires that entropy was low in the past. Low entropy requires concentrations of energy in usable forms, and presumably there are many ways such concentrations might have existed in the universe. On the face of it, we seem to have no reason to expect any particularly neat or simple story about how it works in the real world—about where the particular concentrations of energy we depend on happen to originate. Remarkably, however, modern cosmology suggests that all the observed low entropy is associated with a single characteristic of the early universe, soon after the big bang—in other words, a single 'heap' of low entropy at the beginning of time, pretty much as Eddington had proposed.

The crucial thing seems to be that matter is distributed extremely smoothly in the early universe. This provides a vast reservoir of low entropy, on which everything else depends. In particular, smoothness is necessary for galaxy and star formation, and most familiar irreversible phenomena depend on the sun.

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

One way to get a sense how surprising this is, is to recall that we've found no reason to disagree with Boltzmann's suggestion that there's no objective distinction between past and future—no sense in which things really happen in the direction we think of as past-to-future. Without such a distinction, there's no objective sense in which the big bang is not equally the end point of a gravitational collapse. Somehow that collapse is coordinated with astounding accuracy, so that the matter involved manages to avoid forming large agglomerations (in fact, black holes), and instead spreads itself out very evenly across the universe. (By calculating the entropy of black holes with comparable mass, Penrose [22] has estimated the odds of such a smooth arrangement of matter at 1 in $10^{10^{123}}$.)

In my view, this discovery about the cosmological origins of low entropy is one of the great achievements of twentieth century physics. It is a remarkable discovery in two quite distinct ways, in fact. First, it is the only anomaly necessary to account for the low entropy we find in the universe, at least so far as we know. So it is a remarkable theoretical achievement—it wraps up the entire puzzle of the thermodynamic asymmetry into a single package, in effect. Second, it is astounding that it happens at all, according to existing theories of how gravitating matter should behave (which suggests, surely, that there is something very important missing from those theories). [2]

11 Open questions

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

It is far from clear how this remaining cosmological puzzle is to be explained. Indeed, there are some who doubt whether it needs explaining. [9, 10, 30] But these issues are beyond the scope of this paper. I want to close by calling attention to some open questions associated with this understanding of the origins of the

---

2. True, it is easy to fail to see how astounding the smooth early universe is, by failing to see that the big bang can quite properly be regarded as the end point of a gravitational collapse. But anyone inclined to deny the validity of this way of viewing the big bang faces a perhaps even more daunting challenge : to explain what is meant by, and what is the evidence for, the claim that time has an objective direction !

thermodynamic asymmetry, and by making a case for an unusually sceptical attitude to the second law.

One fascinating question is whether whatever explains why the universe is smooth after the big bang would also imply that the universe would be smooth before the big crunch, if the universe eventually recollapses. In other words, would entropy would eventually decrease, in a recollapsing universe ?[3] It is often dismissed on the grounds that a smooth recollapse would require an incredibly unlikely 'conspiracy' among the components parts of the universe, to ensure that the recollapsing matter did not clump into black holes. However, as we have already noted, this incredible conspiracy is precisely what happens towards (what we usually term) the big bang, if we regard that end of the universe as a product of a gravitational collapse. The statistics themselves are time-symmetric. If something overrides them at one end of the universe, what right do we have to assume that the same does not happen at the other ? Until we understand more about the origins of the smooth early universe, it seems best to keep an open mind about a smooth late universe.

Another interesting and open question is whether a future low entropy boundary condition would have effects *now.* Events at the present era provide us with evidence of a low entropy past. Could there also be evidence of a low entropy future ? The answer depends on our temporal distance from such a future boundary condition, in relation to the relaxation time of cosmological processes. It has been argued that a symmetric time-reversing universe would require more radiation in the present era than we actually observe—radiation which in the reversed time sense originates in the stars and galaxies of the opposite end of the universe. [17] But because of its anti-thermodynamic character, from our point of view, it is doubtful whether this radiation would be detectable, at least by standard means. [23]

Some people dismiss the question whether entropy would reverse in a recollapsing universe on the grounds that the current evidence suggests that the universe will not recollapse. However, it seems reasonable to expect that when we find out why the universe is smooth near the big bang, we'll be able to ask a theoretical question about what that reason would imply in the case of universe which did recollapse. Moreover, as a number of writers [20, 21] have pointed out, much the same question arises if just a bit of the universe recollapses—e.g., a galaxy, collapsing into a black hole. This process seems to be a miniature version of the gravitational collapse of a whole universe, and so it makes sense to ask whether whatever constrains the big bang also constrains such partial collapses.

12 Scepticism about the second law

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

The case for scepticism goes like this. What we've learnt about why entropy increases in our region is that it does so because it is very low in the past (for some reason we don't yet know), and the increase we observe is the most likely outcome

---

[3] This possibility is often called the "Gold universe", though as Larry Schulman pointed out to me, the attribution of this proposal to Gold [18], e.g., by Davies [13] as well as by me [23], seems to depend on an extrapolation from Gold's own views.

consistent with that restriction. As noted, however, the statistics underpinning this reasoning are time-symmetric, and hence the predictions we make about the future depend implicitly on the assumption that there is no corresponding low entropy boundary condition in that direction. Thus the Boltzmann probabilities don't enable us to predict without qualification that entropy is unlikely to decrease, but only that it is unlikely to decrease, *unless there is the kind of boundary condition in the future that makes entropy low in the past.* In other words, the second law is likely to continue to hold so long as there isn't a low entropy boundary condition in the future. But it can't be used to exclude this possibility—even probabilistically !

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

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

> [I]n that very interesting discussion of the Kinetic Theory which was begun at the Oxford meeting of the British Association in 1894 and continued for months afterwards in *Nature,* Mr. Burbury took a conspicuous part, appearing as the expounder and defender of Boltzmann's H-theorem in answer to the question which so many [had] asked in secret, and which Mr. Culverwell asked in print, '*What is the H-theorem and what does it prove ?*' Thanks to this discussion, and to the more recent publication of Boltzmann's *Vorlesungen über Gas-theorie,* and finally to this treatise by Burbury, the question is not so difficult to answer as it was a few years ago. [19]

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

> Does not the theory of a general tendency of entropy to diminish[4] take too much for granted ? To a certain extent it is supported by experimental evidence. We must accept such evidence as far as it goes and no further. We have no right to supplement it by a large draft of the scientific imagination.[8]

Burbury's reasons for scepticism are not precisely those which seem appropriate today. Burbury's concern might be put like this. To see that the dynamical processes routinely fail to produce entropy increases towards the past is to see that it takes

---

[4] Burbury is apparently referring to Boltzmann's quantity H, which does decrease as entropy increases.

an extra ingredient to ensure that they do so towards the future. We're then surely right to wonder whether that extra ingredient is sufficiently universal, even towards the future, to guarantee that the second law will always hold. As the first clearly to identify the source of the time-asymmetry in the H-theorem, Burbury was perhaps more sensitive to this concern than any of his contemporaries.

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

It is interesting to compare Burbury's scepticism about the second law to Eddington's view that it holds "the supreme position among the laws of Nature" :

> If someone points out to you that your pet theory of the universe is in disagreement with Maxwell's equations—then so much the worse for Maxwell's equations. If it is found to be contradicted by observation—well, these experimentalists do bungle things sometimes. But if your theory is found to be against the second law of thermodynamics I can give you no hope; there is nothing for it but to collapse in deepest humiliation. This exaltation of the second law is not unreasonable. There are other laws which we have strong reason to believe in, and we feel that a hypothesis which violates them is highly improbable; but the improbability is vague and does not confront us as a paralysing array of figures, whereas the chance against a breach of the second law (i.e. against a decrease of the random element) can be stated in figures which are overwhelming. ([14], 74–75)

The appropriate reply to Eddington, in my view, is that he himself accepts that there is "anti-chance" in play in the universe, needed to explain the low entropy past. If someone's "pet theory of the universe" proposes violations of the second law because it proposes that there might be anti-chance in the future, as well as the past, then Eddington's appeal to overwhelming probabilities simply misses the point. We can't appeal to probability to refute the hypothesis that the relevant probabilities are not reliable everywhere—that simply begs the question against the hypothesis.

So Burbury has the better of this argument, in my view. Eddington falls into a trap that has snared many later thinkers. What makes the trap so tempting is the lure of a temporal *double standard*—a temptation to reason differently about the past and about the future. But that's the very temptation we need to resist, if we want to understand where the differences in question come from.

13 Where now for the flow of time ?

To finish, I want to return to the question as to whether time has some other arrow, not captured by the thermodynamic asymmetry. This issue has close connections with one of the oldest debates in philosophy. On one side of the debate are philosophers who think of time just as we experience it, a live process of flow and change. ("All is flux", as Heraclitus of Ephesus put it, around 500BC.) On this view, time really *passes* (or "goes on", as Eddington put it). And the present moment seems to have a special status, as the moving boundary between fixed past and open future. (This is deeply connected to our sense of freedom. Our actions, freely chosen, seem to play a small part in bringing about the future.)

On the other side of the debate are philosophers who think of time the way we describe it in history, as a fixed and unchanging series of events, lined up in a particular order—laid out as in a map, as Eddington says. Here, the original credit often goes to Parmenides of Elea, another of the early Greek philosophers, who argued that existence is uniform and timeless, and that change is impossible. In recent work, this view is often called the Block Universe picture.

Which is the right view of time, the 'dynamic' Heraclitan view, or the 'static' Block Universe ? This is still a live issue in philosophy, and Heraclitus and Parmenides both have their contemporary champions. However, the two sides sometimes seem to be talking past one another—and in part, I think, this is because they don't pay enough attention to precisely what the issues are.

13.1 Three ingredients in 'temporal passage'

In particular, I think it is helpful to distinguish three distinct elements in dynamic conception. These elements tend to be bundled together, but in principle they can be separated, and defended in almost any combination. (We already encountered two of them in Section 2, when we considered what Eddington has in mind when he discusses 'time's arrow'.)

1. The view that the *present moment* is objectively distinguished, and that reality is objectively divided into past, present and future.
2. The view that time has an objective *direction*—that it is an objective matter which of two non-simultaneous events is the *earlier* and which the *later.*
3. The view that there is something objectively "flow-like" about time—that time really "goes on", as Eddington puts it.

Philosophers who defend the Block Universe picture tend to be steadfast in their rejection of (1) and (3), and a little bit more open-minded about (2). In physics, however, it easy to find famous critics of all three elements.

About (1), for example, there is a well-known remark that Einstein makes in a letter to the bereaved family of his old friend, Michele Besso (a few months before his own death). Einstein offers the consoling thought that past, present and future are all equally real—only from our human perspective does the present seem special, and the past seem lost : "We physicists know that the distinction between past, present and future is only a stubbornly persistent illusion," as he puts it. (For this, Karl Popper called him the Parmenides of modern physics.)

Concerning (2), we have already encountered Boltzmann's proposal : "For the universe, the two directions of time are indistinguishable, just as in space there is no up and down." While for (3), we noted a famous remark by Hermann Weyl, which rejects the idea of an objective flow of time :

> The objective world simply is, it does not happen. Only to the gaze of my consciousness, crawling upward along the world-line of my body, does a section of the world come to life as a fleeting image in space which continuously changes in time.

The Block Universe seems to be the majority view among contemporary physicists, though it is easy to find dissenters : for example, physicists who argue that the fact that modern physics *seems* to favour the Block conception is an indication that something is missing from physics.[5]

13.2 Eddington on 'becoming'

As we have seen, Eddington wanted to be a dissenter, too, though he struggles with question as to what the missing ingredient—"becoming"—could actually be, and what reason we really have for thinking that it is something objective. His discussion of these matters deserves a much more thorough examination than I can give it here, but I want to highlight two features of it : first, the role that he takes consciousness to play, in providing a reason to think of becoming as something real, and second a challenge he issues to his opponents.

Eddington begins his chapter on 'Becoming' with a wonderfully characteristic[6] presentation of the view he wants to oppose :

> When you say to yourself, "Every day I grow better and better", science churlishly replies—
>
> "I see no signs of it. I see you extended as a four-dimensional worm in space-time ; and, although goodness is not strictly within my province, I will grant that one end of you is better than the other. But whether you grow better or worse depends on which way up I hold you. There is in your consciousness an idea of growth or 'becoming' which, if it is not illusory, implies that you have a label 'This side up'. I have searched for such a label all through the physical world and can find no trace of it, so I strongly suspect that the label is non-existent in the world of reality."
>
> ... Taking account of [entropy], the reply is modified a little, though it is still none too gracious—
>
> "I have looked again and, in the course of studying a property called entropy, I find that the physical world is marked with an arrow which may possibly be intended to indicate which way up it should be regarded. With that orientation I find that you really do grow better. Or, to speak precisely, your good end is in the part of the world with most entropy and your bad end in the part with least. Why this arrangement should be considered more creditable than that of your neighbour who has his good and bad ends the other way round, I cannot imagine." ([14], 87)

In response to this challenge, Eddington goes on to propose that in the case of becoming, consciousness gives us an insight in the nature of reality which physics otherwise misses. These two passages give some of the flavour of the viewpoint :

---

[5] I have heard this view expressed by Lee Smolin, George Ellis, Chris Fuchs and David Mermin, for example.

[6] That is, witty and clear !

> Unless we have been altogether misreading the significance of the world outside us—by interpreting it in terms of evolution and progress, instead of a static extension—we must regard the feeling of 'becoming' as ... a true mental insight into the physical condition which determines it. ([14], 89)

> We have direct insight into "becoming" which sweeps aside all symbolic knowledge as on an inferior plane. If I grasp the notion of existence because I myself exist, I grasp the notion of becoming because I myself become. It is the innermost Ego of all which *is* and *becomes.* ([14], 97)

As he puts it in another passage :

> The view here advocated is tantamount to an admission that consciousness, *looking out through a private door,* can learn by direct insight an underlying character of the world which physical measurements do not betray. ([14], 91, my emphasis)

Eddington appreciates, of course, that this is not an easy position for a physicist to accept : "The physicist ... does not look kindly on private doors, through which all forms of superstitious fancy might enter unchecked." But he stresses the alternative :

> But is he [i.e., the physicist who renounces private doors] ready to forgo that knowledge of the going on of time which has reached us through the door, and content himself with the time inferred from sense-impressions which is emaciated of all dynamic quality ? ([14], 91)

And at this point, backing up this rhetorical question, he issues what I want to call *Eddington's Challenge :*

> No doubt some will reply that they are content ; to these I would say—Then *show your good faith by reversing the dynamic quality of time* (which you may freely do if it has no importance in Nature), and, just for a change, give us a picture of the universe passing from the more random to the less random state ... If you are an astronomer, tell how waves of light hurry in from the depths of space and condense on to stars ; how the complex solar system unwinds itself into the evenness of a nebula. ... If you genuinely believe that a contra-evolutionary theory is just as true and as significant as an evolutionary theory, *surely it is time that a protest should be made against the entirely one-sided version currently taught.*" ([14], 91–92, my emphasis)

In my view, Eddington is wrong about becoming. I side with Einstein, Boltzmann and Weyl, in rejecting all three elements of the view that time really "goes on". Nevertheless, I think that Eddington's Challenge deserves to be better known. In thinking about how to meet it, we may learn a lot about the consequences of the revolution that has taken place in our understanding of time, since the late nineteenth century. I suspect that friends of the Block Universe have not done enough to free themselves from the shackles of the old viewpoint ; and in the long run, the best arguments for the Block view might flow from a recognition of the advantages of thinking about time in the revolutionary way.

13.3 Meeting Eddington's Challenge

The first response to Eddington's Challenge should be to appeal to Boltzmann, I think. The Boltzmann of the Boltzmann-Schuetz hypothesis is well ahead of Eddington, in offering us a picture in which the entropy gradient is a local matter in the universe as a whole, entirely absent in most eras and regions (and with no single preferred direction in those rare locations in which it is to be found). Combined with Eddington's own view that the asymmetries he challenges his opponent to consider reversing—asymmetries of inference and explanation, for example—have their origin in the entropy gradient, this means that Boltzmann has an immediate answer to the Challenge. Of course we can't "[reverse] the dynamic quality of time" *around here,* for we live within the constraints of the entropy gradient in the region in which we are born. But we can tell you, in principle, how to find a region in the picture is properly reversed; and that shows that the fixity of our own perspective does not reflect a fundamental asymmetry in nature. Analogously (Boltzmann might add), the fact that people in Northern Europe cannot live with their feet pointed to the Pole Star does not prove a spatial anisotropy. If you want to live with your feet pointing that way, you simply need to move elsewhere.

Eddington himself associates the entropy gradient quite closely with the "time of consciousness" :

> It seems to me, therefore, that consciousness with its insistence on time's arrow and its rather erratic ideas of time measurement may be guided by entropy-clocks in some portion of the brain. ... Entropy-gradient is then the direct equivalent of the time of consciousness in both its aspects. ([14], 101)

So the Boltzmann-Schuetz hypothesis certainly threatens the veracity of Eddington's "private door".

In broader terms, however, Eddington's Challenge has not been taken up. Most advocates of the Block view—even those explicit about the possibility that time might have no intrinsic direction—have not explored the question as to what insights might follow from Boltzmann's 'Copernican' shift in our perspective. I want to conclude with some brief remarks on this issue. It seems to me that there are at least two domains in physics in which we might hope to vindicate Boltzmann's viewpoint, by exhibiting the advantages of the atemporal perspective it embodies.

13.4 Eddington's Challenge in cosmology.

The first domain is cosmology. There are two aspects to the relevance of Boltzmann's viewpoint in this context. First, and closest to Boltzmann's own concerns, there is the project of understanding the origin of the entropy gradient, in our region. As I have already noted, one of the great advances in physics over recent decades has been the realisation that this problem seems to turn on the question as to why gravitational entropy was low, early in the history of the known universe – in particular, why matter was smoothly distributed, to a very high degree, approximately 100,000 years after the Big Bang. As we try to explain this feature of the early stages of the known universe, Boltzmann's hypothesis ought to alert us to the possibility that it is non-unique – ought to open our eyes to a new range of cosmological models, in which there is no single unique entropy gradient.

There is some recent work which takes this possibility seriously – see, e.g., [12] (from which Figure 3 is borrowed) and [11]. However, there is much more contemporary work in which it is either overlooked, or dismissed for what, with Boltzmann's symmetric viewpoint clearly in mind, can be seen to be fallacious reasons. For example, the possibility that entropy might decrease 'towards the future' is dismissed on statistical grounds, with no attempt to explain why this is a good argument towards the future, despite the fact that (i) it is manifestly a bad argument towards the past, and (ii) that the relevant statistical considerations are time-symmetric. (This is the 'double standard' of which I accused Eddington at the end of Section 12. See [23] for a discussion of other cases of this fallacy, and the role of the timeless viewpoint in avoiding them.)

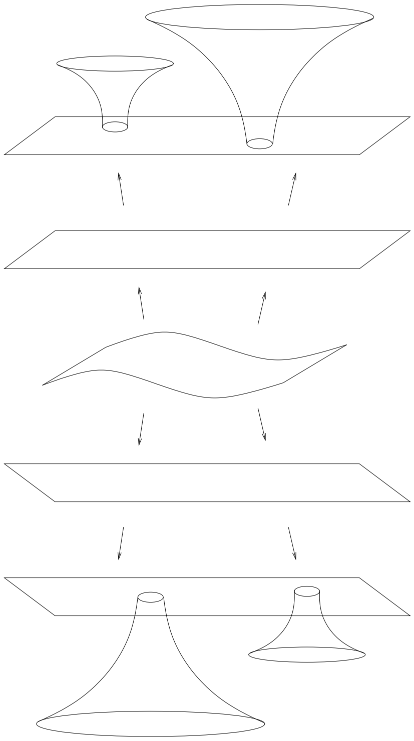

Figure 3 – "The ultra-large-scale structure of the universe. Starting from a generic state, it can be evolved both forward and backward in time, as it approaches an empty de Sitter conguration. Eventually, fluctuations lead to the onset of inflation in the far past and far future of the starting slice. The arrow of time is reversed in these two regimes." [12]

These considerations point in the direction of the second and broader aspect of the relevance of Boltzmann's atemporal viewpoint in cosmology. It alerts us to the possibility that the usual model of 'explanation-in-terms-of-initial-conditions' might simply be the wrong one to use in the cosmological context, where the features in need of explanation are larger and more inclusive than anything we encounter in the familiar region of our 'home' entropy gradient. Here, the point connects directly with Eddington's Challenge, in the way noted above. We can concede our local practices of inference and explanation are properly time-asymmetric, as Eddington observes; while insisting that symmetry might prevail on a larger scale.

13.5 Eddington's Challenge in microphysics.

Even more interestingly, in my view, there is the possibility that the 'pre-Copernican' viewpoint might be standing in the way of progress needed in fundamental physics – that is, that there might be explanations to which this viewpoint is at least a major obstacle, if not an impenetrable barrier. Here the most interesting candidate, in my view, is the project of realist interpretations and extensions of quantum mechanics. Discussions of hidden variable models normally take for granted that in any reasonable model, hidden states will be independent of future interactions to which the system in question might be subject. The spin of an electron will not depend on what spin measurements it might be subject to in the future, for example. Obviously, no one expects the same to be true in reverse. On the contrary, we take for granted that the state of the electron may depend on what has happened to it in the past. But how is this asymmetry to be justified, if the gross familiar asymmetries of inference, influence and explanation are to be associated with the entropy gradient, and this is a local matter ? Are electrons subject to different laws in one region of the universe than in another, or "aware" of the prevailing entropy gradient in their region ? On the contrary, in Boltzmann's picture : we want microphysics to provide the universal background, on top of which the statistical asymmetries are superimposed.

When we explore these issues, it might turn out that the apparently puzzling assumption that hidden variables cannot depend on future interactions is just a manifestation of the time-asymmetry of our ordinary causal notions, grounded entirely in the asymmetry of our own viewpoint—in other words, that the assumption is just a kind of perspectival gloss on underlying dynamical principles which are symmetric in themselves. If so, there would be no new *physical* mileage to be gained by adopting the atemporal viewpoint. Certainly, we would understand better what belonged to the physics and what to our viewpoint, but no new physics would be on offer as a result.

However, the more intriguing possibility is that there is a new class of physical models on offer here—models which are being ignored not for any genuinely good reason, but only because they seem to conflict with our ordinary asymmetric perspective. If that's the case—see [16] and [28] for some recent discussion—and if the models presently excluded have the potential they seem to have in accounting for some of the puzzles of quantum mechanics, then Boltzmann's viewpoint will prove to be truly revolutionary ; and Eddington's Challenge will be well and truly met.

Acknowledgments

I am grateful to Ikaros Bigi and Martin Faessler for permission to reprint material from [27]. My research is supported by the Australian Research Council and the University of Sydney.

References

[1] D.Z. Albert, *British Journal for the Philosophy of Science* **45**, 669 (1994).

[2] D.Z. Albert, *Time and Chance* (Harvard University Press, Cambridge, 2000).

[3] L. Boltzmann, *Sitzungsberichte, K. Akademie der Wissenschaften in Wien, Math.-Naturwiss* **75**, 67 (1877).

[4] L. Boltzmann, *Nature*, **51**, 413 (1895).

[5] L. Boltzmann, *Wied. Ann.* **60**, 392 (1897).

[6] S.H. Burbury, *Nature* **51**, 78 (1894).

[7] S.H. Burbury, *Nature* **51**, 320 (1895).

[8] S.H. Burbury, *Philosophical Magazine, Series 6,* **8**, 43 (1904).

[9] C. Callender, *Metascience* **11**, 68 (1997).

[10] C. Callender, *British Journal for the Philosophy of Science* **49**, 135 (1998).

[11] S. Carroll, S., *From Eternity to Here: The Quest for the Ultimate Theory of Time* (Dutton, New York, 2009).

[12] S. Carroll and J. Chen, arXiv:hep-th/0410270v1.

[13] P. C. W. Davies, *The Physics of Time Asymmetry* (Surrey University Press, London, 1974).

[14] A. Eddington, *The Nature of the Physical World* (Cambridge University Press, Cambridge, 1928).

[15] A. Eddington, *Nature*, **127**, 447 (1931).

[16] P. Evans, H. Price and K.B. Wharton, arXiv:1001.5057v3 [quant-ph].

[17] M. Gell-Mann and J. Hartle in *Physical Origins of Time Asymmetry,* ed. J. Halliwell, J. Perez-Mercader and W. Zurek (Cambridge University Press, Cambridge, 1994).

[18] T. Gold, *American Journal of Physics* **30**, 403 (1962).

[19] E.H. Hall, *Science, New Series* **10**, 685 (1899).

[20] S.W. Hawking, *Physical Review, D* **33**, 2489 (1985).

[21] R. Penrose in *General Relativity: an Einstein Centenary,* ed. S.W. Hawking and W. Israel (Cambridge University Press, Cambridge, 1979).

[22] R. Penrose, *The Emperor's New Mind* (Oxford University Press, Oxford, 1989).

[23] H. Price, *Time's Arrow and Archimedes' Point* (Oxford University Press, New York, 1996).

[24] H. Price, *British Journal for the Philosophy of Science* **53**, 83 (2002).

[25] H. Price in *Time, Reality and Experience,* ed. C. Callender (Cambridge University Press, Cambridge, 2002).

[26] H. Price in *Contemporary Debates in the Philosophy of Science,* ed. C. Hitchcock (Basil Blackwell, Oxford, 2004).

[27] H. Price in *Time and Matter,* ed. I. Bigi and M. Faessler (World Scientific, Singapore, 2006).

[28] H. Price, arXiv:1002.0906v2 [quant-ph].

[29] T.M. Ridderbos and M. Redhead, *Foundations of Physics* **28**, 1237 (1998).

[30] L. Sklar, *Physics and Chance* (Cambridge University Press, Cambridge, 1993).

[31] L. Sklar in *Time's Arrows Today,* ed. S. Savitt (Cambridge University Press, Cambridge, 1995).

[32] C. von Weizsäcker, *Annalen der Physik (5 Folge)* **36**, 275 (1939), reprinted in translation in *The Unity of Nature* (Farrar Straus Giroux, New York, 1980).

[33] H. Weyl, *Philosophy of Mathematics and Natural Science* (Princeton university Press, Princeton, NJ, 1949).