K. G. & J. S. Denbigh, Entropy in Relation to Incomplete Knowledge, Cambridge University Press, 1985.

H. D. Zeh, The Physical Basis of the Direction of Time, Springer-Verlag, Berlin, 1989.

CONTENTS

The philosophy of physics often seems to be a dual discipline, conducted in different and by no means complementary ways from the non-commuting standpoints of physicists and philosophers. The pessimistic view would be that this is inevitable, and simply reflects some fundamental differences between the kinds of activities that physicists and philosophers are engaged in. However, we optimists like to believe that there is indeed a common agenda, an underlying body of questions that both sides are concerned to answer, and that the difficulties in making progress are practical rather than principled; communication is difficult, but not impossible, and needs to be encouraged.

In different ways, the present books are both works from which optimists may take heart. None of the authors is a philosopher. But the Denbighs' book addresses what philosophers will recognize as an important philosophical issue, and manages in the main to be accessible to readers without a strong background in physics or mathematics. And

Zeh's book, although in no sense a philosophical study, not only addresses a topic that will be of great interest to many philosophers, but does so with fine sensitivity to some of the crucial philosophical subtleties.

I: Entropy in Relation to Incomplete Knowledge

The Denbighs' book is a critical discussion of the view that entropy 'really signifies nothing more than a lack of human knowledge' (p. vii),[1] being therefore a subjective or anthropocentric concept. The authors argue that entropy is a 'fully objective' dispositional property of a physical system.

The topic is an important one. Apart from its intrinsic interest, it relates to wider philosophical issues in at least two ways. For one thing, as the authors point out in their preface, it relates to 'some very profound epistemological and ontological issues concerning the scientific enterprise in general.' (p. vii) Of wider interest, however, is likely to be the fact that the notion of entropy is widely seen as central to the perplexing problem as to the nature and origins of temporal asymmetry in the physical world. This is the sort of work to which philosophers will look for professionals' insights into this puzzling issue. The issue is prominent in Denbigh and Denbigh's discussion, and not merely in a motivational capacity: the problem of temporal irreversibility is presented as an argument in favour of the preferred objective interpretation of entropy.

In view of this potential, it is disappointing that the book is weakest at just the points at which it might be expected to connect with these wider topics. Its account of what is at issue concerning 'objectivity' is inadequate and potentially misleading. And in referring to temporal irreversibility the book appears to reproduce, at several crucial points, a seemingly tireless misconception about the capacity of the notion of entropy to explain temporal asymmetry. Taken together, the effect of correcting these failings seems to be not only to undermine the main conclusion, but actually to support a version of the contrary viewpoint. In other words, it seems to me that the correct conclusion of the Denbighs'

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[1] Except where otherwise indicated, page references are to the books under review.

investigation should have been a view which, far from discarding the old intuition about entropy, would reconstruct it on proper philosophical foundations.

In the following discussion of the Denbighs' book I shall confine myself to an attempt to amplify and make good these objections, and the tone of my remarks as a whole will therefore be critical. Before I begin, let me therefore emphasize that this is not the complete picture. There is a lot that I liked about the book—not least the fact that so much of it is accessible to a philosopher without a strong background in the relevant technicalities. Of particular note is the subtle treatment of Gibbs' Paradox in chapter 4, and the associated discussion of identity and indistinguishability. The latter will interest to philosophers concerned with the bearing of modern physics on these old metaphysical issues. So potential readers should not be put off by the tone of the following comments. It is a very useful book.

I.1 Objectivity

The terms objectivity and subjectivity are perhaps two of the most overworked in the philosophical lexicon—surely an indication that the distinctions that more precise terms would have to mark are far from easy to draw. (After all, philosophers are not noticeably reluctant to coin new terms, when the opportunity presents itself.) In this case, it is easy enough to see that these terms cover a tangle of notions, but very difficult to make out the individual strands.

It is into this briar patch that Denbigh and Denbigh wade, when they set out to 'consider the notions of objectivity and subjectivity in rather general terms before taking up the issue concerning entropy.' (p. 5)

> To say that some physical phenomenon or entity is objective can be understood in two different senses, one of them weak and the other strong. The weaker meaning is that the occurrence of the phenomenon, or the existence of the entity, can be publicly agreed. Such would be the case, for instance, if it were said that ice is cold to the touch. Everyone can agree that this is so, but of course the assertion depends on the existence of the human nervous system. By contrast, the strong meaning of objectivity, the meaning most commonly used in science, is that the phenomenon

> or entity in question has a reality quite independent of man's presence in the world. This form of objectivity would apply, for example, to the event of the laying down of the Devonian rocks. Geologists would hold that the fully objective occurrence of that event is in no way impugned by man's non-existence at that point. These two forms of objectivity (which may be called objectivity$_1$ and objectivity$_2$, respectively) are, of course, by no means mutually exclusive. Most phenomena or entities (or events or states-of-affairs) which fall into the category objective$_2$ can also be publicly agreed, and many of the things which can be publicly agreed may enjoy the status of objectivity in the stronger sense. As for the meaning of 'subjectivity', we shall simply take this as being the negation of one or other of the meanings of objectivity. (pp. 5-6)

The reference here to the fact that objectivity$_1$ and objectivity$_2$ are not mutually exclusive is a little puzzling, and not merely because the second piece of evidence offered to this effect adds nothing to the first (i.e., if most things that are objective$_2$ are objective$_1$, then obviously many things that are objective$_1$ are also objective$_2$). It is puzzling because the descriptions 'weaker' and 'stronger' already lead us to expect a logical relationship between the two notions being defined which is incompatible with their being mutually exclusive. The expected relationship is one of containment: generally speaking, we would expect that things that are objective$_2$ are also objective$_1$, but not vice versa.

A further difficulty concerns the proffered account of subjectivity. As it stands, it seems to allow that something might be both objective and subjective; i.e., objective$_1$ and not objective$_2$. It would surely be simpler to fix on one notion of either subjectivity or objectivity, and to regard all the remaining options as degrees of the other notion. For example, we might say that true objectivity is what is here called objectivity$_2$, and that objectivity$_1$ is therefore a form of subjectivity, albeit not the most extreme form.

Although these logical and terminological confusions are of little significance in themselves, they have perhaps obscured for the authors a more important difficulty. This is that their main criterion for objectivity simply doesn't exclude what it is supposed to exclude. The point may be illustrated by their own example of weak objectivity, namely the fact that ice is cold. This fact is supposed to contrast with phenomena which are objective in the stronger sense, such as 'the laying down of the Devonian rocks', whose occurrence is of course 'in no way impugned by man's non-existence at that period.' However, was ice not cold in the Devonian period? Of course it was; it had the very same disposition to feel cold to the touch as it has today, regardless of the fact that there was no one around at the time to give that disposition some exercise.

As for coldness, so for the secondary qualities in general. We may agree that it is true of these qualities—indeed, it is what makes them secondary qualities—that in some sense they are dependent on the capacities of humans or similar creatures. Difficult as it may be to say what this dependence amounts to, it needn't and obviously shouldn't amount to the doctrine that there are simply no such properties in the Universe, except at the rare times and places at which there happens to be somebody about to appreciated them. This means that secondary qualities are objective$_2$, in Denbigh and Denbigh's sense. So if the point being made by those who have favoured subjective interpretations of entropy is effectively that entropy is a secondary rather than a primary quality—if this is what the claimed 'subjectivity' actually amounts to—then there is simply no conflict between the Denbighs' position (as construed in terms of this criterion for objectivity) and that of their supposed opponents.

The geological paradigm of objectivity is no mere aberration, but becomes the standard reference point in the Denbighs' discussion. Thus it is difficult to avoid the conclusion that they are battling an illusory opponent, defending a position that is not under attack. This is not to suggest that the advocates of 'subjective' interpretations of entropy have done a better job of identifying their positions in precise philosophical terms—far from it, to judge by the sorts of remarks Denbigh and Denbigh quote as indicative of the views with which they disagree. However, it is Denbigh and Denbigh who have taken on the difficult task of surveying the field, of sketching the philosophical map. In my view they have missed the landscape's most interesting feature, with the result that their subsequent offensive cannot fail to be misdirected.

I.2 Entropy as a secondary quality?

As noted above, the possibility that Denbigh and Denbigh leave off their map is that entropy might be argued to be subjective in the sense that the classical secondary qualities are subjective. This is not in the sense that they exist only when and where human or other observers exist, rather in the sense that their nature and distinctive character is in some way tied to the responses or capacities of suitably equipped such observers. Despite the long history of the distinction between primary and secondary qualities, it is still very difficult to describe this dependence in a fully satisfactory way. It may be that the required theoretical framework is simply not yet in place. The topic has recently become the focus of considerable interest, however, and we may hope for clarification.[2]

For present purposes let us say that the distinctive feature of a secondary quality is that it cannot adequately be characterised without reference to some human or cognitive response or capacity. In order to characterise the property of redness, for example, we need to mention the colour vision of normal humans. This is not incontestable, of course, but it is a popular and familiar philosophical position. The present question is whether it gives us a way of cashing out the intuition that there is something subjective about entropy. I think that it does.

It is best to approach the point indirectly, by means of related concept which arguably should be treated in a similar way, namely that of probability. There is a well known and very longstanding debate as to whether probability itself is 'subjective' or 'objective'. Roughly speaking, what is usually held to at issue here is whether the formal probability calculus should be thought of as applying primarily to a certain kind of psychological state—a partial belief, or degree of confidence—or whether it should rather be taken to describe the characteristics of a distinctive feature of the external world, namely

[2] One focus of recent interest has been the possible comparison between moral value and the orthodox secondary qualities: see for example Wright [1987] and [1988], and Johnston [1989]. For an extension of the notion of a secondary quality in a different direction, see Menzies and Price [forthcoming].

objective probability, or chance. Among philosophers, at any rate, the objectivist side of this debate has had the best of the running in the past twenty years or so.

The Denbighs are well aware of this debate, and avail themselves of the popular conclusion that quantum mechanics has proved a trump card for objective probability. (I shall come back to the use they make of this, which seems to me questionable.) What they miss, as indeed many philosophers have missed, is the fact that objectivism about probability in this sense is quite compatible with subjectivism about probability in another sense. It is quite compatible with the view that in order to say what objective probability or chance is, we must describe its role in cognitive life; in other words, with the view that chance is a secondary quality, in the broad sense sketched above.

What is the essential connection between chance and cognitive activity? It is that chance is a guide to life, or more precisely a guide to rational decision. The rational weight to give to a possibility that may or may not occur should accord with one's best estimate of its chance of occurring. The philosopher who has done most to mark this crucial connection is perhaps David Lewis, who calls it the Principal Principle (see Lewis [1980]); though its importance had already been recognized by Hugh Mellor, whose [1971] version of the propensity theory is distinctive, inter alia, in that it does recognize the very close conceptual connection between chance and rational partial belief. (Mellor castigates frequency theorists such as Braithwaite for misconstruing what is plainly an analytic link; see his [1971], p. 67.) It is true that neither Mellor nor Lewis says that chance is a secondary quality, and in that sense subjective. Their concern is rather to distance themselves from probabilistic subjectivists of the traditional sort. Nevertheless, both are committed to the principle that an adequate philosophical characterisation of objective probability will inevitably refer to the link between chance and rational decision; that chance cannot be understood in isolation from the matter of its significance for cognitive creatures who must deliberate in contexts of incomplete information.

Here then, in close proximity to the discussion concerning entropy, is a precedent for the view that an important scientific notion has an intrinsically anthropocentric (or at least 'cognocentric') character. This is not to say that Denbigh and Denbigh may justly be taken to task for missing this precedent, or the interpretation of the claim that entropy is subjective for which it is a precedent. On the contrary, as I have tried to emphasize, the view has not been well marked even in the philosophical literature, and non-specialists can hardly be criticized for failing to note it. It is simply that Denbigh and Denbigh have taken on a task which turns out to require more extensive resources than they have at their disposal. Moreover, it needs to be stressed that the above point bears largely on the nature of the conceptual framework, within which the main question arises. I have tried to show that the book misconstrues this second order framework. It doesn't follow, however, that there is anything wrong with the first order argument. Prima facie, it may be that Denbigh and Denbigh succeed in showing that entropy is not subjective in the secondary quality sense, and merely misrepresent their own conclusion.

Indeed, it is not difficult to read some of the arguments the book offers as directed against the view that entropy is analogous to a secondary quality. For central to such a view will be the doctrine that entropy needs to be characterised in terms of knowledge (or some related notion, such as description, representation or information). The need to appeal to such a notion will be what make entropy cognocentric, on this view. Thus the thesis might be that just as an adequate account of chance needs to remark the connection between chance and rational decision, so an adequate account of entropy must mark the fact that entropy is a measure of the 'completeness' of a representation of a physical system—a measure of 'degree of information'. (Note that it would be compatible with this view that cognomorphism itself comes by degrees. Chance and entropy might be held to be less subjective than the familiar perceptibly grounded secondary qualities, in depending on more general and therefore more universal features of a cognitive system; for example, on the capacity to represent information, rather than on the possession of certain highly specific sensory responses.) And as one would expect (given its title) several of the arguments of the book bear directly on the claim that entropy is related to incomplete knowledge, in a manner not characteristic of physical properties in general. Let us now turn to these arguments.

I.3 The significance of the failure of classical determinism

At several points (e.g. pp. 2, 32, 66-7) Denbigh and Denbigh invoke the widely accepted overthrow of classical determinism in quantum mechanics in support of the claim that entropy is objective. They appear to subscribe to the following line of argument. If classical determinism were true, there would be no objective probabilities, or chances. In particular, the probabilities involved in statistical mechanics would necessarily be subjective probabilities. Entropy, being grounded in statistical mechanics, would therefore be subjective as well. So if entropy is objective then classical determinism is false.

From the Denbighs' point of view, however, this argument has what should seem an alarming corollary. It entails that any independent reason for thinking that entropy is objective is also a reason for thinking that classical determinism is false! By their lights (given that they offer independent reasons of this kind) it follows that the failure of Newtonianism was in principle predictable, in virtue of its inability to account for the objectivity of entropy. Moreover, the possibility of deterministic hidden variable theories in quantum mechanics can be excluded on the same grounds.

One response to this difficulty would be to concede that the apparent independent arguments for the objectivity of entropy are not in fact of any consequence, so that the issue of the objectivity of entropy simply turns on that of the probabilities employed in the statistical mechanics on which it is grounded. This would leave the book's main conclusion intact, but at the cost of jettisoning much of its argumentation. Another response would be to attempt to argue that the objectivity of entropy and of probability in physics are, after all, independent matters. Perhaps the most promising way to develop this defence would be to distinguish between two (or more) different notions of objectivity, and hence between two different issues about entropy. First, is entropy objective in the sense in which quantum mechanics is said to have shown probability to be objective? And second, is entropy objective in the remaining sense or senses of the term?

Assume for the sake of argument that the appeal to quantum-mechanically grounded objective probabilities can thus be made to cohere with the rest of the book. It seems to me that the point still backfires, for the reason discussed in the previous section.

We observed that there is an important sense in which even so-called objective chances may plausibly be held to be subjective, namely the sense which depends on their conceptual link to rational belief and hence to rational decision. We noted that it may be argued that chance is cognocentric—a secondary quality, under a natural generalisation of this notion. So if entropy inherits the characteristics of the probabilities employed in the statistical mechanics on which it is grounded, we have here the outlines of an argument to the effect that entropy is also subjective, in virtue of its dependence on the cognocentric notion of probability. Clearly, this argument will be untouched by the issue as to whether physical probability is objective in the more common sense.

I.4 Are all properties 'relative to the data'?

There is a further aspect to the continuing debate about the objectivity of probability, not always properly distinguished from the issue as whether probability is fundamentally a measure of degree of belief. This aspect turns on the fact that probability has a distinctive relational character. The same event or proposition will in general have different probabilities, relative to different bodies of evidence. This feature led some writers—Keynes, for example—to construe probability as a (measure of a) logical relation between on the one hand a set of evidential propositions, and on the other a further proposition, upon whose truth the evidential propositions bear. One of the attractions of this interpretation is that what it offers us as the subject matter of probability theory seems appealing unproblematic. Like logical relations of more familiar kinds, those of probability appear to combine objectivity with a convenient isolation from the nuts and bolts of the physical world. So if the idea that there are objective probabilities in the physical world seems hard to reconcile with respectable Humean metaphysics, it isn't because probability is not objective. It is simply that in looking for it in the physical world we have looked in the wrong place.

As usual, however, these benefits must be paid for elsewhere. The logical interpretation faces a number of difficulties. For one thing, the status of such logical relations is itself problematic. It is arguable that such relations could only be properly characterised in terms of their role in cognitive life; and that they thus have the status of secondary qualities, in the broad sense outlined above. This question aside, the logical interpretation meets a more fundamental obstacle. As Ayer [1963] pointed out, relational probability isn't enough to guide decision. A painstaking investigation might lead us to the conclusion that the proposition that Quantum Leap will win the Grand National is supported to degree 0.75 by evidential proposition P; to degree 0.8 by Q; to degree 0.35 by R; and so on. Of itself, no amount of knowledge of this kind will take us any closer to a decision as to what odds it is rational to accept on a bet that Quantum Leap will win. What is missing is a principle to govern the rational choice of evidence—a principle to tell us which evidential proposition we are entitled to treat as a minor premiss, so as to allow us to detach an unconditional or non-relational judgement to guide our choice of odds. The question then arises as to whether the only such principle is a subjective one—in effect, that each person should use the evidence he or she has to hand—or whether on the other hand the required principle may appeal to some intersubjective base of evidence, so that the rationality of choice of betting odds is correspondingly an objective matter. What has happened is that a thorny issue about the objectivity of probability, an issue apparently evaded by the logical interpretation, has re-emerged under a new description.

Reverting now to the more usual description, we may think of one aspect of the issue as to the existence of objective probabilities as that of the availability of a privileged or canonical evidential frame, for probabilistic statements of certain kinds. Roughly then, to say that there are objective physical probabilities or chances is to say that physics sets a non-trivial limit to the amount of evidence that bears on the occurrence of events of certain kinds; the chance that P is the probability that P, relative to all the evidence that physics allows.

I want to put aside the question as to how this version of the issue about the objectivity of probability relates to the version I mention earlier (which represents subjectivism as a predominantly psychological doctrine). For present purposes, what matters is that this version is familiar enough to mark a contrast to the way in which

Denbigh and Denbigh attempt to respond to the charge that entropy is similarly 'relative to evidence', and therefore subjective.

> Rather than accept that entropy is an anthropomorphic concept, the present authors prefer the way in which the matter has been expressed by Hobson ... . Instead of speaking of the entropy of the physical system, Hobson thinks we should speak of 'the entropy of the data'. He also remarks that this does not render entropy subjective for it is an observable quantity and its value is determined by the observable data such as energy, volume and composition. The value is thus relative, not to the observer but to the observer's data. ... It will be clear, however, that Hobson's position is a very general one and does not solely concern entropy; any physical property is a function of all variables which determine the state of the body in question, rather than being 'a property of the body' *simpliciter*.
>
> ... In short, we conclude that thermodynamic entropy is a fully objective characteristic of the data on a system, of how that system is specified, and in this respect it is entirely on a par with physico-chemical properties in general. (pp. 13-14)

With the probabilistic parallel at hand, it will be clear that the Denbighs might have argued that a non-relational entropy is secured by the existence of a privileged data base; that the 'absolute' entropy of a system is its entropy relative to a (or preferably *the*) maximal set of physically accessible data. Instead they have chosen what in the probabilistic case would amount to the claim that *all* physical properties are relative to evidence, in the same way that probability is.

This claim is simply not true, however. Perhaps the simplest way to exhibit the difference between probability and non-relational physical attributes is as follows. Consider a property such as mass. It may of course be true that an evidential proposition E implies that a given body B has mass $\underline{m}_E$; whereas some different evidential proposition F implies that B has a different mass $\underline{m}_F$. Taken together, these facts show that E is inconsistent with F. By contrast, consider a probabilistic case. The proposition that Quantum Leap will win the Grand National may be high relative to the evidence that he is unbeaten in a dozen starts; but low relative to the evidence that he has just been slipped enough sleeping tablets to fell a rhinoceros. Clearly, however, these logical facts do not entail that there is any inconsistency between the two pieces of evidence. This difference reflects the fact that we take a judgement as to the mass of a body to concern its unique 'true' mass; but in many cases, at any rate, make no such assumption about probability. (Another way to think of the issue about the existence of objective probabilities is as concerned with the question whether there are some circumstances in which we do or should make this assumption about probability.)

Prima facie, then, probability is 'relative to the data' in a manner not characteristic of physical properties in general. Clearly, the same might be held to be true of entropy. In both cases, moreover, it is pointless to try to meet the resulting charge of subjectivity on the grounds that Denbigh and Denbigh take over from Hobson, namely that the 'value is thus relative, not to the observer but to the observer's data.' This would be like defending a charge of bank robbery on the grounds that one had not committed the offence at a larger bank. True, there is room for some terminological plea bargaining. Is the conceded type of relativity to evidence really a species of <u>subjectivity</u>, for example? Does it make entropy the <u>anthropocentric</u> notion that some have claimed it to be? On these points Denbigh and Denbigh may well have the better of some of the less careful advocates of the subjective interpretations of entropy. But they gain this advantage at the cost of admitting the case for a more modest explication of the subjectivist intuitions; the case for the view that entropy is relative to a 'state of information' concerning a physical system.

As I noted earlier, Denbigh and Denbigh would thus have done better to follow the path of probabilistic objectivists, arguing that non-relational entropy is guaranteed by the existence of a privileged evidential base—by the existence and necessary incompleteness of a maximal state of information for a physical system. Absolute entropy would thus be entropy relative to maximal data.

However, although this would be a better approach (from the authors' own point of view) than that offered in the book, it is not clear that its conclusion is strong enough for their purposes. For as in the case of the parallel argument for objective—i.e., non-relational—probability, its effect is to suggest that entropy is at root a relational notion; and that it is rescued from this fate only by the fortuitous fact that nature happens to provide a privileged reference frame, a maximal evidential base. Given the contingent character of availability of such a privileged frame, it seems inappropriate to insist that entropy is the essentially non-relational notion that such a frame makes possible. For this makes entropy inherit the contingency of the existence of privileged frames; roughly, it means that in a non-quantum-mechanical world, there would be no entropy.

True, many philosophers seem happy with the parallel conclusion about probability: they accept that if determinism were true (or turns out to be true, despite quantum mechanics) then there would be (or are) no non-trivial objective probabilities, or chances. But the effect of this hard-headed attitude is likely to be to render the account it accompanies irrelevant. For we still need an account of all that ordinary talk of probability which clearly makes no assumptions at all about the determinism or otherwise of the microworld—and once we have that, who needs these hard-headed chances? Here, as elsewhere, a hard head is no proof against extinction.

In any case, it needs to be emphasized that even in the most favourable indeterministic environment, objective chances may nevertheless be secondary qualities in the broad sense discussed in section **I.2**. That is, an adequate account of what chance is will need to take account of its cognitive role—of the function served by judgements about chances in the deliberations of rational agents. While this account is yet to written, it seems clear that its effect will be to undermine the supposed distinction between chance and other species of probability, emphasising instead the general family resemblance, the general connection with deliberation in the face of inconclusive evidence.

To summarize, it seems to me that in responding to the suggestion that entropy is 'relative to the data', Denbigh and Denbigh are not only mistaken in claiming that entropy shares this feature with physico-chemical properties in general, but that they thereby miss a much more promising response. Even allowing for this response, however, the point does not in the end turn in their favour. The fortuitous availability of a privileged data base will not alter the fact that entropy has its conceptual origins in circumstances in which such good fortune could not be taken for granted. The conclusion seems to be that entropy is at base a relational notion, albeit one that quantum mechanics may give us the right to treat as if it were non-relational.

There is one further argument that plays a significant role in Denbigh and Denbigh's defence of an objective interpretation of entropy. I want to deal with it separately, however, because although in my view it is no more successful than those above, its mistake is more 'physical' than philosophical. First, however, let me summarize my assessment of the philosophical side of the case. As I have tried to make clear, the failings of the book in this area should be seen in the main as reflecting the complex and as yet underdeveloped state of the theoretical framework required to address such issues as that of the status of entropy. The authors have ventured into a field in which, at the moment, even specialists are liable to get lost. However, I have suggested that there does appear to be position that makes sense of the intuition that entropy has a subjective or anthropocentric character, and that Denbigh and Denbigh miss. It is the view that entropy is a secondary quality, in a suitably generalized sense of this term.

I.5 Entropy and time asymmetry

Now to the remaining major argument that Denbigh and Denbigh offer for an objectivist interpretation of entropy. They repeatedly criticize the subjectivist view for its inability to account for the temporal irreversibility we observe in the world. For example, they quote a remark of Max Born's, that 'irreversibility is a consequence of the explicit introduction of ignorance into the fundamental laws'; and object that

> irreversible processes, such as temperature equalisation or the mixing of gases, take place in the same way, however closely we, as their observers, might try to specify them. The temporal asymmetry of macroscopic phenomena (on which the entropy concept is founded) has nothing to do with our amount of information. (pp. 44-5)

Elsewhere they say that

> in regard to irreversibility the subjectivist is driven to the absurd conclusion, as Popper put it, that molecules escape from a 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.' (p. 46)

It seems to me that the Denbighs here make the right criticism, but on the wrong grounds. For the clear implication is that objectivists about entropy are able to do better at explaining entropy—whereas the truth of the matter is that neither side is able to do so. The upshot is that the problem of temporal asymmetry is neutral as regards the objectivity of entropy.

To be fair, the Denbighs are in very good company at this point. Their mistake rests on a common misconception of the outcome of the debate at the turn of the century as to whether Boltzmann's statistical mechanics accounts for the observed temporal irreversibility of many physical processes. What is puzzling about this irreversibility is that it seems difficult to reconcile with the manifest symmetry of the microphysical laws governing the very systems whose behaviour is observed to be so asymmetric. (The point originally arose in a Newtonian context. Quantum mechanics has muddied the waters to some extent, but is widely taken to be temporally symmetric. The more recently noted violations of temporal symmetry in particle physics appear to have no direct bearing on the problem of macroscopic temporal asymmetry.)

In essence, Boltzmann's proposal goes like this. Suppose that the problem is to explain why although gas will leave a pressurised container, the reverse process never occurs; an empty vessel never pressurises 'of its own accord'. Consider the gas molecules at the instant such a pressurised container is opened. Each molecule has a particular position and velocity, and a listing of all of these would provide the complete 'microstate' of the body of gas concerned. Now there are enormously many possible such microstates, compatible with fact that the gas is in the container—compatible with its known 'macrostate', as we might say. Assuming for simplicity that the dynamics of the gas molecules is deterministic, and that the system is otherwise closed, each of these microstates will correspond to a unique microstate at any later time. Boltzmann's basic point, in effect, is that the overwhelming majority of the initial microstates give rise to later microstates such that the gas has left the container. Given this basic numerical point, it follows that so long as the initial microstates may be assumed to be equiprobable—not a trivial assumption, but put that aside—the overwhelming probability is that the gas will leave the container.

A similar chain of reasoning is held to explain why the reverse process does not occur. Of the huge number of microstates compatible with the gas being initially in equilibrium in and around the open container, only a tiny fraction are such as to lead to a later microstate in which the gas entered the container. Again assuming equiprobability, it follows that self-pressurisation is exceedingly unlikely.

Boltzmann's attempt to explain irreversibility on this statistical basis was criticized by Loschmidt and by Zermelo. These two objections are somewhat different, but may be considered to spring from a common intuition, described by Denbigh and Denbigh in these terms:

> The familiar difficulty which statistical mechanics has to confront when seeking to demonstrate irreversibility is that the basic laws and theories of physics are all t-invariant. This means that elementary processes such as collisions, quantum transitions, etc., have precisely the same frequency of occurrence whether they occur from a micro-state 1 to a micro-state 2 or from the 'time-inverted' state of 2 to the time-inverted state of 1 in the same temporal interval. How, then, can it occur that a macroscopic process, involving immense numbers of elementary processes, can show temporal asymmetry? This was ... the basis of the famous 'objections' of Loschmidt and Zermelo to Boltzmann's theorem. (p. 49)

Denbigh and Denbigh go on to describe Loschmidt's objection, employing an example of the above kind:

> Consider a gas and let $\{A_i\}$ and $\{B_j\}$ be those sets of exactly specified microstates which are accessible to the gas in its initial and final thermodynamic states, A and B, respectively. For example $\{A_i\}$ might refer to the set of states before the gas expands into a vacuum and $\{B_j\}$ might refer to the set of states after expansion is complete. Let $\bar{A}_i$ and $\bar{B}_j$ differ from A_i and B_j respectively only in that each molecule has an exactly reversed velocity vector. Since states A_i and $\bar{A}_i$ have the

> same energy it may be assumed that they are equally likely to occur and similarly for states B_j and $\bar{B}_j$. Thus it would appear, in accordance with the t-invariance of the basic laws, that, if the macroscopic process $A \rightarrow B$ occurs, the reverse process $B \rightarrow A$ should occur equally frequently in the same direction of time. On these grounds Loschmidt argued that Boltzmann's H-theorem must be in error. (pp. 49-50)

Denbigh and Denbigh endorse Boltzmann's reply to this objection, which they describe as follows:

> [This objection] does not do justice to the statistics of systems containing a very large number of molecules. For out of the set $\{\bar{B}_j\}$ of inverted velocities only a very small fraction will be suitable for allowing a gas which has expanded into a larger volume to subsequently contract momentarily into its original smaller volume. In short, the existence of a set of suitably orientated velocities which would allow of recontraction becomes exceedingly improbable when the system contains a large number of molecules.

However, this simply fails to engage the essential point of Loschmidt's objection, namely that the statistical considerations involved are entirely time symmetric. This time symmetry implies that the probability of any transition—say from the expanded to the unexpanded state—is exactly the same as that of the inverse transition from the unexpanded to the expanded state. It follows that if we are entitled to conclude that an expanded gas will not contract, we are also entitled to conclude that an expanded gas has not 'uncontracted'—i.e., has not been contracted in the past. In other words, a statistical argument that the entropy of a system cannot become lower in the future is also a statistical argument that the entropy of the same system cannot have been lower in the past.

It might be objected that I have here ignored the crucial 'initial condition' that characterises those actual physical systems that exhibit temporal asymmetry, namely that their entropy is in fact much lower than its maximum possible value. It is this condition, it might be claimed, that prevents us from applying the statistical argument in reverse. However, this response amounts to a concession that temporal asymmetry is not explicable in statistical terms. For it entails that the statistical argument requires two additional premisses: at best, it shows that entropy is overwhelmingly likely to increase provided firstly that it is constrained to be low at some point in the past; and secondly <u>that it is not constrained to be low in the future</u>. This is not quite to reduce the statistical argument to the truism that entropy will increase provided it is low in the past and not low in the future. All the same, it clearly shifts the burden of explaining temporal asymmetry onto the task of accounting for this difference in initial and final constraints.

It might be thought that one additional premiss is enough. Thus it might be admitted that we need to account for the (prima facie unlikely) initial conditions we find in the world, but maintained that final condition are a consequence of Boltzmann's statistical considerations. However, this is again to miss the moral of Loschmidt's objection. In effect, Boltzmann's proposal is to explain the second law of thermodynamics by showing that it is a consequence of certain plausible premisses to do with the frequency distributions of microstates. But Loschmidt points out that if Boltzmann's argument is valid then so is a parallel argument with a conclusion we know to be false, namely that entropy was higher in the past. This is not an uncommon predicament in science. What it suggests is that there is a missing premiss, a hidden ingredient whose importance has been overlooked—an ingredient present in one case but absent in the other, which thus serves to explain why the parallel breaks down.

For example, suppose that we want to know why bats don't lay eggs. Someone suggests that it is because bats are mammals. This may seem a perfectly good explanation. Formally, it looks like this:

> Bats are mammals
> Mammals don't lay eggs
> ———————————
> Bats don't lay eggs.

At this point, however, it may be pointed out that if this explanation is a good one, then so is the parallel argument that entails that platypuses don't lay eggs; for platypuses are certainly mammals. Should we conclude that the explanation is a good one in the case of bats but a bad one in the case of the platypus? Obviously not. The right course is to identify the missing ingredient, and hence to find a more detailed explanation that gives the right answer in both cases. Here, the missing ingredient is essentially the distinction between placental and non-placental mammals. Thus:

> Bats are placental mammals
> Placental mammals don't lay eggs
> ___
> Bats don't lay eggs.

A platypus isn't a placental mammal, so there is no longer a parallel argument to the effect that platypuses don't lay eggs.

Similarly, the proposed statistical explanation of temporal irreversibility runs something like this:

> (i) Decreasing entropy requires a transition from a probable to an improbable state; i.e. from a macrostate which may be realized in many ways to a macrostate which may be realized in relatively few ways
>
> (ii) Probable-to-improbable transitions are statistically unlikely
> ___
> Decrease in entropy does not in general occur.

Loschmidt points out that in virtue of the time symmetry of the statistical considerations, improbable-to-probable transitions have the same statistical likelihood as probable-to-improbable transitions. If the above argument is sound, in other words, then so too is this parallel:

> (i') Increasing entropy requires an improbable-to-probable transition;
>
> (ii') Improbable-to-probable transitions are statistically unlikely;
> ___
> Increase in entropy does not in general occur.

Following the pattern of the zoological case, what is need is a uniform way of modifying these arguments so that the latter one no longer goes through. In the latter case the required addition seems to be a premiss such as (iii') Improbable initial states are not required by non-statistical boundary conditions.

This is untrue of the actual world, a fact which may be held to account for the unsoundness of the inference from (i'), (ii') and (iii') to the conclusion that entropy does not in general increase.

In the former case, the corresponding addition is what we get by substituting 'final' for 'initial' in (iii'); i.e.

(iii) Improbable final states are not required by non-statistical boundary conditions.

Assuming that this is true, the inference from (i), (ii) and (iii) to the conclusion that entropy does not decrease now seems both valid and sound. More importantly, we have a uniform pattern of explanation that properly locates the difference between the increasing and decreasing cases. This difference lies not in the impact in the actual world of initial constraints alone, but in the <u>contrast</u> between this on the one hand, and the apparent lack of impact of final constraints on the other.

Properly understood, Loschmidt's objection thus shows that temporal irreversibility cannot be accounted for in Boltzmann's statistical terms. As I noted earlier (and their own references to the literature amply confirm), the Denbighs are in good company in misunderstanding this point. There has been a widespread failure to appreciate that the numerical arguments involved a simply blind to temporal orientation, and therefore incapable of explaining temporal asymmetry. One of the virtues of our second book is that it is particularly clear on this point. As Zeh puts it,

> statistics as a method of counting has nothing a priori to do with the physical concept of time and its direction. It is therefore not able to explain by itself the thermodynamical arrow of time. The statistical description of irreversible processes uses additional physical assumptions which characterize a direction in time—often in hidden form. (p. 31)

More on Zeh's treatment of temporal asymmetry in a moment. First, in fairness to Denbigh and Denbigh, it needs to be said that although they apparently endorse the general thrust of Boltzmann's response to Loschmidt, they do not ignore the difficulties involved in attempting to present such a response in a rigourous way. They discuss Liouville's theorem, and the attempt to avoid its restrictions by means of Gibbs' notion of coarse graining; and lean towards the popular conclusion that a crucial role is played by the random microscopic influences to which any actual physical system is inevitably subject—the 'sudden impulses originating in the world external to the system in question.' (p. 55; see also pp. 59-60) However, it seems that they are unaware of the objection that in assuming that such influences are random, or incoherent, one imports the very temporal asymmetry one is trying to explain. In other words, it needs to be explained why these inputs from the past are not correlated in such a way as to reduce rather than increase the entropy of the target system. After all, it is the fact that the inevitable 'outgoings' from a physical system are precisely correlated that prevents us from running the argument in reverse; prevents us from arguing that in virtue of these connections with the outside world, entropy must be higher at earlier times than at later times. Once again what has happened is that time reversed case provides a counterexample to the proposed explanation of time asymmetry; thereby demonstrating the need for some further premiss in virtue of which the argument works in one case but not in the other. And once again, to introduce such a premiss is to shift the explanatory burden from one place to another. It now needs to be explained why there should be such a contrast between the uncorrelated nature of influences from the past and the correlated nature of influences from the future.

In summary, it seems to me that the Denbighs are mistaken in taking the problem of temporal asymmetry to bear on the question of the objectivity of entropy. This mistake stems from a more fundamental error, namely the failure to appreciate the moral of Loschmidt's objection: the root of temporal asymmetry lies in the boundary conditions of the universe (or our part of it), and it is these that need to be explained. As I noted at the beginning, the question of the origins of time asymmetry is of more general interest than that of the status of entropy itself, and many of the Denbighs' philosophical readers are likely to be looking for the experts' insights into this wider puzzle. It is disappointing that what they will find is yet another version of some stock misconceptions as to what is at issue, and where the solutions might lie.

II: The Physical Basis of the Direction of Time

Now to Zeh's book. This is a revised and extended version of Zeh's Die Physik der Zeitrichtung (Springer Lecture Notes in Physics, Vol. 200, 1984). It is also in part an attempt to up-date and extend Paul Davies' treatment of the subject in The Physics of Time Asymmetry (Davies [1974]). As Zeh notes, 'our knowledge about the arrow of time that appears in general relativity and cosmology has grown enormously since the appearance of Davies' book, partly due to his own contributions.' (p. v) Like Davies before him, Zeh's main aim is to provide a survey of the different branches of physics that bear on the issue of temporal asymmetry. Though the book is directed primarily at physicists and their students, Zeh expresses the hope that it 'may be of some interest to the philosopher who is familiar with the concepts of theoretical physics.' (p. v) This modest hope should be more than fulfilled. Indeed, this is the sort of book that makes those of us whose acquaintance with these concepts is rather less than familiar, wish that we too could become a little more intimate. As it is, most of us will have to pick and choose. The book rewards the effort, however, and will stand with Davies' as a standard reference for philosophers with a serious interest in the perplexing question of the origins of temporal asymmetry.

One of the attractive features of Zeh's treatment of the issues, from a philosopher's point of view, is his sensitivity to the dangers of assuming in some hidden form the very asymmetry one is trying to explain. (We have already encountered a version of this mistake, in our discussion of Denbigh and Denbigh's treatment of irreversibility.) Davies was far from blind to this danger, but my impression is that Zeh sees it more clearly. Indeed, in discussing attempts in cosmology to explain the low entropy state of the early universe, Zeh is able to convict Davies of an error of this kind. (p. 128) This is not to say that Zeh himself sees all the traps. I shall try to show that he too falls into just such a circularity in trying to show that the supposed temporal asymmetry of radiation may be explained in terms of that of thermodynamics. (Davies is another victim of this particular snare.) Before I come to that point, let me briefly mention some other points of philosophical interest.

II.1 Quantum mechanics and time asymmetry: does the interpretation matter?

The first point concerns quantum mechanics. Zeh's view of the likely relationship between quantum mechanics and the problem of temporal asymmetry goes well beyond the standard claim that quantum mechanics has disproved determinism. Zeh recognises that the problem of interpreting quantum mechanics may well be intimately related to the issue of irreversibility. In doing so, of course, he rejects the view common among physicists, namely that the philosophy of quantum mechanics had its affairs satisfactorily wound up by the Copenhagen interpretation. (Zeh refers to the 'non-concepts' of complementarity and dualism; see p. vi.) However, he settles as an alternative on Everett's many worlds interpretation, a view that many philosophers regard as just as ill-defined and problematic. (See for example Richard Healey [1984]) True, there is probably no interpretation not thus afflicted. However, it seems to me that there is one approach that ought to look particularly promising when viewed in the light of a rigorous attempt to transcend our ordinary asymmetric prejudices about time. It is the interpretation that escapes Bell's Theorem and other apparent obstacles to hidden variables by allowing the values of such variables to depend on future interactions of the system in question.

This is well known as a formal possibility—Zeh himself mentions it on p. 87—but is usually dismissed on the grounds of supposed philosophical objections to the 'backward causation' it seems to involve. It is striking that much more attention is given to accounts such as David Bohm's hidden variable theory, in which a similar dependency is established instantaneously at the time of interaction. The evident tension between this view and special relativity is avoided in the former interpretation, in which influences are confined to light cones. The fact that the instantaneous influence approach is nevertheless regarded as less implausible is an indication of how strongly counterintuitive we find the idea of influencing the past. But what is the basis of this intuition? A natural suspicion is that when we enquire into the origins of temporal asymmetry, it will turn out to be simply another prejudice, grounded like the others in our own asymmetric nature and condition. At any rate, this seems such an attractive possibility, both from a philosophical and a formal point of view, that it is disappointing that it is not explored in the congenial environment provided by Zeh's book.

On the other hand, Zeh's preference for the Everett interpretation is far from idiosyncratic. It appears to have become the accepted interpretation for work in quantum gravity and quantum cosmology. The main reason for this preference seems to the belief that any other interpretation accords an essential role to an external observer or measuring apparatus, whose presence is required and responsible for the 'collapse of the wave packet'. This is felt to be unacceptable in quantum cosmology: cosmology attempts to describe the whole universe, and cannot make sense of an external standpoint. It is difficult for an outsider to tell whether this attachment to the Everett interpretation has a significant influence on the theoretical output of quantum cosmology. However, it is a tantalising possibility that the attempt to bring together quantum mechanics and general relativity is at present being hampered by some misconceptions on the conceptual or philosophical side of quantum mechanics. For example, suppose that we were to accept some form of a hidden variable interpretation, involving backward or instantaneous non-local influences. The wave function might then be viewed as an incomplete description (more or less as Einstein wished to view it). The so-called 'collapse' would correspond to nothing more physical than the (objective) availability of a different (though still incomplete) description—roughly what happens to classical probabilities when evidence changes, in fact. Would this make a difference to quantum cosmology? I don't know, and would greatly appreciated any well-informed suggestions.

II.2 Cosmology and time reversal

In chapters 5 and 6, Zeh discusses the prospects for a cosmological explanation of temporal asymmetry. His treatment is particularly noteworthy for the care he takes to avoid the kind of circularity that here, as elsewhere, infests attempts to account for the 'arrow' of time. We have seen that when properly conducted, the statistical attempt to explain time asymmetry leads to a problem concerning boundary conditions. In a cosmological context, the main task is then to account for the low entropy initial condition of the universe; and also (though this requirement is less commonly noted) for the apparent lack of such a boundary condition in the future. With the advent of the Big Bang theory, a natural suggestion was that the thermodynamic arrow should be a consequence of the arrow or asymmetry provided by the expanding universe. As Zeh describes, however, this suggestion raises the following question:

> Will the thermodynamical arrow be reversed when the universe starts to recollapse towards the big crunch after having reached its maximum expansion? The answer must be 'yes' if the cosmic expansion really represents the master arrow. It is often answered 'no' on the basis [that] ... a reversal of the thermodynamical arrow of time would ... require 'conspirative' correlations to be present in [the present universe]. (p. 124)

In other words, the objection is that reversal of the thermodynamic arrow of time would require that the present microstate of the world be such that all sorts of statistically improbable processes will later occur—the same sorts of 'irreversible' processes that occur in this part of the universe, but with the opposite time sense. But as Zeh points out, 'this argument ... contains a typical error, as it presupposes the "retarded causality" in question, that is, the absence of conspirative correlations.' (p. 124)

Here the circularity infects the attempt to show that the universe does not have a low entropy future boundary. At the other end of things, it also tends to find its way into attempts to explain why the universe does have a low entropy past boundary. There is a tendency to accept as a 'natural' constraint on the initial state of the universe a condition that doesn't seem a plausible constraint on its final state—in other words, to see the Big Bang as more constrained than the Big Crunch. However, this difference is surely an indication that one has slipped in an assumption of temporal asymmetry in some disguised form. From a symmetric perspective, after all, what could possibly distinguish these two extremities? Neither is really initial or final, from this perspective; at both ends of the universe we just have a 'Big Brunch' (as Zeh elsewhere describes it). To apply different constraints is thus to beg the question. (Zeh convicts Paul Davies of an error of this kind; see pp. 127-8. Another culprit may be Stephen Hawking: see Zeh pp. 147-8, and Price

[1989].) It appears that the arrow of time is explicable in cosmology only if we are prepared to accept that it may change direction. Perhaps the boomerang would provide a more suitable metaphor.

II.3 Retarded radiation: the propagation of a myth?

There is one major topic on which it seems to me that Zeh fails to avoid the mistakes of previous writers; one area in which he too falls for a version of the circularity fallacy. The topic in question is the so-called time arrow of radiation, which is the subject of Zeh's chapter 2. It has been almost universally accepted that radiation phenomena exhibit a temporal asymmetry distinct from that of thermodynamics. Zeh describes this asymmetry as follows:

> After a stone has been dropped into a pond one observes concentrically outgoing waves. Similarly, after an electric current has been switched on, one finds a retarded electromagnetic field. Since the laws of nature which successfully describe these events are invariant under time-reversal, they are equally compatible with the reversed phenomena in which, for example, concentrically focussing waves would eject a stone out of the water. Such solutions of the dynamical laws have however never been observed in nature. (p. 12)

Accordingly, it is said that in nature radiation is always retarded rather than advanced.

As readers may recall, the example of the stone in the water is particularly associated with Popper, who used it in an influential [1956] note to argue that temporal irreversibility is not simply a matter of thermodynamics. In the electromagnetic case the point is considerably older. It was at the centre of a notable exchange between Einstein and Ritz in the early 1900's. Ritz argued that the retarded nature of radiation should be regarded as a law of nature, and might then be used to explain the thermodynamical arrow of time. Einstein on the other hand held that the asymmetry of radiation was 'exclusively based on reasons of probability', and therefore presumably of the same origin as the thermodynamic asymmetry.

There is some contemporary support for Ritz's position. For example Cranmer [1983] has argued in favour of the attempt to explain the asymmetry of thermodynamics in terms of that of radiation. More popular is the intermediary view, which seems to be Popper's, namely that the two 'arrows' are essentially independent. Proponents of this position usually also disagree with Ritz as to the nominological status of the retarded nature of radiation. It is regarded as a de facto asymmetry, 'fact-like' rather than 'law-like' in character. (Of course, we are not always told what this difference amount to.) Finally, there is also a solid body of support for the view that the radiative asymmetry is an aspect or consequence of the thermodynamic asymmetry; or perhaps that both asymmetries stem from some common master asymmetry, such as cosmological expansion.

Zeh and Davies both regard the view that the radiative arrow is a consequence of the thermodynamic arrow as the most attractive alternative, and discuss two main arguments to this effect. However, it seems to me these arguments again involve a logical circularity of the kind that litters the literature on temporal asymmetry. Moreover, I think that in this case the failure to notice the fallacy has obscured some more basic mistakes. I want to show that the problem these arguments address is partly illusory, and grounded on some conceptual confusions. The non-illusory part turns out to be amenable to some of the treatments already at hand, but presently deployed in the wrong way. The upshot is that there is no longer any distinct problem about the asymmetry of radiation itself—no way that radiation might have been, whose absence from the actual universe needs to be explained. Instead there is an issue that is clearly an instance of the general issue as to why entropy is low in the past but not (apparently) in the future.

Thus in my view Zeh, Davies and others have reached the right conclusion by the wrong route. The thermodynamic arrow does appear to be basic. However, this is not because it gives rise to the radiative arrow, but simply because the phenomena that have been misconstrued as involving a radiative arrow form a sub-species of those whose occurrence constitutes the thermodynamic asymmetry.

II.4 Radiation and thermodynamics (I): Popper's circular wave argument

In outlining the case for this conclusion, the first task is to exhibit the logical circularity involved in the standard route. As I noted, there are two main arguments purporting to show that the retarded nature of radiation is a consequence of thermodynamics. The simpler of the two may be illustrated in terms of Popper's example. In this case, we want to know why still ponds are never observed to produce converging circular waves; let alone converging waves which arrive at their common centre at just the right moment to give an added impetus to a stone recently miraculously expelled from its resting place on the bottom of the pond. The suggestion is that we consider what would be necessary at the edges of the pond (to say nothing of the bottom) in order for this to take place. At each spot the random motion of the edging material would have to cooperate to give the right sort of 'nudge' to the adjacent water; and all these nudges would have to be precisely coordinated, one with another, at the different points around the pond. (In other words, in a circular pond they would all have to occur simultaneously to give a wave converging to the centre; and at appropriate temporal intervals in any other case.) However, isn't this just the kind of entropy-decreasing behaviour that is excluded by the thermodynamic arrow? If so, then the radiative asymmetry is a consequence of the thermodynamic asymmetry. As Davies puts it,

> waves on a real pond are usually damped away at the edges by frictional effects. The reverse process, in which the spontaneous motion of the particles at the edges combine favourably to bring about the generation of a disturbance is overwhelmingly improbable, though not impossible, on thermodynamic grounds. ([1974], p. 119)

Why doesn't the same argument count against ordinary 'retarded' water waves? Because the motion of the particles at the edges is in this case in accordance with the usual thermodynamic arrow.

Although the argument is here formulated in terms of water waves, the electromagnetic case is exactly parallel so long as there is an absorbing boundary corresponding to the edge of the Popperian pond. As Davies says, it applies 'quite generally to all types of waves in finite systems.' ([1974], p. 119) Zeh describes the electromagnetic version of the argument, concluding that 'in this [finite] situation, the radiation arrow may very easily be derived from the thermodynamical one.' (p. 19)

The status of this argument depends on that of the thermodynamic principle on which it relies. It may indeed be valid if the second law of thermodynamics is assumed as a fundamental principle. However, the important point is that it is invalidated by the move to a statistical understanding of thermodynamics. Moreover, the fallacy it then involves is very similar to that of the common attempt to derive the second law from purely statistical considerations, without the necessary attention to boundary conditions. In discussing Denbigh and Denbigh's treatment of irreversibility we observed that the essence of Loschmidt's objection to Boltzmann was that the transition from a probable to an improbable microstate had exactly the same statistical likelihood as the transition from improbable to probable. Statistics alone could therefore not account for the fact that in nature we find the latter sort of transition but not the former. The plausibility of Boltzmann's derivation rests on an implicit appeal to the familiar boundary conditions, namely that the world is in a statistically improbable state in the past, but is not so constrained in the (foreseeable!) future.

Let us apply Loschmidt's lesson to the proposed derivation of the radiative asymmetry. The first thing to note is that both the abnormal case of advanced radiation and the normal case of retarded radiation involve events at the edges which are overwhelmingly unlikely on statistical grounds alone. If the cases are to be distinguished, we must appeal to boundary conditions. The coordinated events at the boundaries of Popperian ponds occur despite their statistical improbability, because the initial conditions are right (and the final conditions are at least not wrong). The coordinated events that would be required in anti-Popperian ponds do not occur not only because they are statistically improbable—remember that that is true in the ordinary case as well—but also, crucially, because this statistical handicap is not overridden by favourable boundary conditions. But what would be a favourable boundary condition? Why, the occurrence in the future of the kinds of events that require such coordinated predecessors; in other words, in particular, the occurrence of converging 'advanced' waves. So in order to distinguish the normal and abnormal cases, the argument implicitly assumes as boundary conditions the very asymmetry it claims to explain. It assumes that in nature we do have diverging radiation and don't have converging radiation.

It is important to note that the argument is quite correct in pointing out that advanced radiation would constitute an exception to the general thermodynamic orientation we observe in the world. That is, it would involve probable-to-improbable transitions, whereas what we normally observe are improbable-to-probable transitions. However, this does not amount to a demonstration that the absence of advanced radiation is a consequence of the thermodynamic orientation. The connection is logical, not physical.

One way to highlight the distinction is to suppose that God wanted to ensure that radiation was purely retarded. The fact that the connection with the thermodynamic asymmetry is a logical one means that even He couldn't achieve this goal indirectly, by fixing the thermodynamic orientation and the laws of physics, and then simply allowing nature to take its course. For the upshot of the statistical treatment of the arrow of thermodynamics is that its orientation is a matter of boundary conditions. Thus to align the thermodynamic arrow, God would need to set the appropriate boundary conditions—and in the cases of interest, this in itself will amount to excluding advanced radiation. There will be nothing left for nature to achieve (and no space for God miraculously to intervene, should He then change His mind about the orientation of radiation). The resulting asymmetry of radiation will be no more a physical consequence of God's intervention than the fact that Adam begins life as a bachelor is an effect of the unmarried male condition that God initially bestows on him; even God couldn't have made Adam an unmarried male but not a bachelor. Thus we may say that the radiative asymmetry is special case of the thermodynamic asymmetry; but not that it is a consequence of (or causally explained by) the thermodynamic asymmetry.

The point is admittedly a rather subtle one, and I hesitate to insist that in the cases I have mentioned (dealing with radiation in finite systems) either Zeh or Davies really intends to use the argument in the sense in which turns out to be fallacious. After all, both authors incline to the view that the asymmetry of radiation is subordinate to that of thermodynamics. I have distinguished two kinds of subordination—logical and physical—and objected that the above argument for the reduction of the radiative arrow to that of thermodynamics is only valid if interpreted in the former sense. In may seem uncharitable to deny that this is the sense that Zeh and Davies have in mind. Indeed, Zeh himself may be alluding to the above objection in making the following remark:

> Trying to explain the situation by the remark that the advanced solutions would require improbable initial conditions would be analogous to the arguments frequently used in statistical mechanics. ... [T]he phenomena observed in nature are precisely as improbable. (p. 13)

On the other hand, there is a further use of the appeal to thermodynamics, which again both Zeh and Davies seem to endorse; and here the fallacious interpretation appears to required by the context in which the argument appears.

Before we turn to that case, let me note that the valid logical interpretation of the above appeal to thermodynamics appears quite sufficient to refute the conclusion that Popper draws from his pond example. Unlike Zeh and Davies, Popper's sympathies lie more with Ritz than with Einstein. Popper contends that there is a temporal asymmetry in the world which is quite independent of that of thermodynamics. The above argument shows that on the contrary, the asymmetry of wave phenomena is simply a sub-species of the problem of asymmetric boundary conditions, to which we are led by the statistical treatment of thermodynamics.

II.5 Radiation and thermodynamics (II): the circularity of the absorber theory

The second attempt that Zeh and Davies describe to reduce the asymmetry of radiation to that of thermodynamics involves the Wheeler-Feynman absorber theory of radiation (first described in Wheeler and Feynman [1945]). This theory addresses the problem of the radiative asymmetry in the following way. It begins by assuming that radiation is not intrinsically retarded, but rather symmetric: the radiation from an isolated accelerated charge is half retarded and half advanced; i.e., it consists of a diverging wave 'to the future', though of half the amplitude normally observed, plus a converging wave from the past of the same halved amplitude. The theory then attempts to show that when we take into account the response to such waves from the absorbing matter they encounter—when we add in the radiation that this encounter will produce—the net effect is precisely what we observe in practice. The response interferes with the original wave in such a way as to exactly cancel its advanced component, and to enhance its retarded component to exactly the amplitude normally expected.

Thus again we have a theory that claims to pluck an asymmetric rabbit from a symmetric hat. From the sceptical viewpoint to which we have become accustomed, it is natural to wonder how the trick was turned. Where did the asymmetry come from? Again, both Davies and Zeh recognize the need to raise this question. The answer they offer is closely parallel to the argument we have been discussing above. As Davies puts it,

> [The] thermodynamic asymmetry in the absorber imposes an asymmetry on the electromagnetic radiation, by permitting the transport of energy from the source at the centre of the cavity to the cavity wall, but not the other way round. The advanced self-consistent solution, which is allowed on purely electrodynamic grounds, is thus ruled out as being overwhelmingly improbable, because it would require the cooperative 'anti-damping' of all the particles in the cavity wall ... . Ions would become collisionally excited, and radiate at the precise moment throughout the wall to produce a coherent converging wave to collapse onto the cavity centre at just the moment that the charged particle there was accelerated. ([1974], p. 144)

Why is this argument more clearly fallacious than the parallel point in the previous context? Because here the basic assumptions of the absorber theory provide the symmetric boundary conditions which invalidate the attempt to establish a thermodynamic contrast between the advanced and retarded cases. What goes on at the absorber in the retarded case would be overwhelmingly improbable, on statistical grounds, were it not for the assumed initial condition (i.e., the presence of the retarded wave from the central source). Symmetrically, what goes on at the (past) boundary in the advanced case would be overwhelmingly improbable, <u>except that we are entitled to assume as a final condition the presence of the advanced wave from the source</u>. So in this context the argument starts with the assumption that the relevant boundary conditions are symmetric. Having done so, it cannot then invoke thermodynamic considerations to generate an asymmetry—except, of course, by improperly ignoring the role of boundary conditions in the statistical treatment of the thermodynamic asymmetry.

So far as I know, this fundamental flaw in the absorber theory's proposed explanation of the asymmetry of radiation has not previously been noted. No doubt it has been disguised by the widespread failure to appreciate Loschmidt's lesson, namely that thermodynamics cannot rest on statistics alone.

I want to finish by outlining what seems to me to be a much more promising way of understanding the asymmetry of radiation. It draws on the mathematical core of the Wheeler-Feynman theory, though substantially reinterpreting its significance. This reinterpretation avoids the circularity just mentioned, as well what seems to me to be another logical flaw in the original version of the argument.

II.6 Radiation and banking

In my view, the question as to why an accelerated charged particle produces retarded rather than advanced radiation—a wavefront that diverges into the future, rather than converging from the past—is very much like the question as to why money deposited in a bank account appears in one's balance after but not before the time of the transaction. There is one important difference between the two cases, which is to do with the fact that radiation is a dispersive phenomenon. Thus the energy 'deposited' in the electromagnetic field by a radio transmitter propagates away through space in all directions. I shall come back to this difference, for it is in this respect that the reinterpreted absorber theory may be used to show that there is nothing intrinsically asymmetric about radiation. But first of all I want to use the banking analogy to clarify the issue of what symmetry here consists in.

I take it that there is no mystery about the temporal asymmetry of making a bank deposit. It is not the product of any intrinsic asymmetry in the activity of banking, but simply follows from what we mean by the term 'deposit'. A deposit just is a transaction in which a sum of money is <u>added</u> to the <u>prior</u> balance, thereby increasing the <u>subsequent</u>

balance. Of course, the italicized terms all presuppose a temporal orientation. If we don't know whether a film taken in a bank is being projected forwards or backwards, we can't tell whether the masked figure it portrays is a thief or a somewhat eccentric depositor. What appears as a deposit from one temporal orientation appears as a withdrawal from the other, and vice versa. However, this dependence of the terminology on an assumed temporal orientation is clearly extrinsic to the nature of banking itself. It doesn't mean that banking is intrinsically asymmetric in time.

Holding fixed the conventional temporal orientation, we may observe the following symmetry between deposits and withdrawals: the sum transferred appears in the account balance <u>after</u> a deposit but <u>before</u> a withdrawal. Again, there's nothing mysterious about this. On the contrary, it reflects an evident symmetry in the process of banking itself, namely the sense in which a withdrawal may be thought of as the temporal inverse of a deposit. In the light of this symmetry, however, it may seem mysterious that there another sense in which a withdrawal is not simply a deposit in reverse. For when do the <u>effects</u> of a deposit and a withdrawal manifest themselves in the banking system? In both cases, obviously, it is <u>after</u> the transaction takes place. The effect of a withdrawal is a reduction in one's balance; and this shows up after but not before the time of the transaction. Hence we might be tempted to say that banking turns out to be retarded rather than advanced—that although the opposite orientation is not ruled out by the laws of nature, in practice all banking transactions have an impact in the future and not the past.

However, I hope it is clear in the case of banking that it would be a mistake to locate the asymmetry in the mechanisms of banking itself. In particular, it would be a mistake to think that there is some alternative structure that banking could have had, but turns out not to have. The asymmetry is somehow a product of the way in which we apply the notions of cause and effect. Just what this 'somehow' amounts to is a nice philosophical issue. The simplest possibility is that it is simply a terminological matter—that an <u>effect</u> is by definition the later of two suitably related events. This was Hume's approach to the problem of causal asymmetry. Many philosophers have felt that it is unsatisfactory, however, particular in excluding by fiat such apparent conceptual possibilities as simultaneous and backward causation. But whatever the answer, the asymmetry has nothing to do with the intrinsic processes of banking. These are as reversible as arithmetical operations of addition and subtraction. Indeed, to all intents and purposes they are these operations, together with a means of maintaining a constant balance over the temporal intervals between transactions.

To what extent may we apply the lessons of the banking case to that of radiation? The obvious suggestion is that we should compared deposits with transmitters, which transfer energy to the electromagnetic field (or other medium of wave propagation); and withdrawals with receivers, which transfer energy from the field. Notice that again this description assumes a temporal orientation—from the opposite orientation the flow of energy will appear reversed—but again this dependence on temporal orientation does not reflect or give rise to any intrinsic asymmetry in the processes themselves. The comparison also holds up in the sense that, concentrating on the energy balance alone (i.e., ignoring the dissipative aspects of the phenomena), reception is the temporal inverse of transmission. And finally, the parallel also holds in the sense that the effects of transmitters and receivers on the energy balance of the field both show up after the time of transmission or reception.

In the banking case we saw that this last feature does not show that banking is intrinsically retarded rather than advanced. The same is true for radiation. Whatever content there may be in the claim that radiation in nature is temporally asymmetric in being retarded but not advanced, it doesn't lie in fact that both receivers and transmitters have retarded effects. On this point I disagree with Zeh, who appears (p. 13, and in correspondence) to take the fact that the effects of a receiver are delayed to refute the suggestion that radiation is intrinsically symmetric, reception simply being the inverse of transmission.

Note that the intrinsic symmetry is not undermined if, as may well be the case, there are no pure receivers in nature; i.e., if all receivers re-radiate some of the incoming energy to the field. Zeh mentions this point in his initial characterisation of the radiative arrow. (p. 3) However, it would be easy to construct banking systems with the analogous feature. Suppose that there are no overdrafts, so that withdrawals are only permitted up to the amount of one's current balance; and that the bank insists on re-depositing 25% of any withdrawal. The result is that complete withdrawal of one's funds is possible only asymptotically, in the limit at infinity of an endless sequence of transactions. But clearly the resulting asymmetry is very superficial. The system may be described in the original symmetric terms, with the addition of the stipulation that all withdrawals are to be accompanied by proportional deposits. Moreover, as the relevant proportion is reduced towards zero, the banking system in question approaches the original pure case; and this alone suggests that the impurity of receivers cannot account for the 'all or nothing' asymmetry supposedly displayed by radiation in the real world.

So it appears that if there is any substance to the standard claim that radiation is temporally asymmetric, we should look for it the dissipative characteristics of the phenomenon; for it is only these that seem to afford any relevant distinction between radiation and banking. On the face of it, the relevant characteristic may seem obvious. A receiver may be the temporal inverse of a transmitter from the point of view of energy balance, but surely there is this crucial difference: only a transmitter is centred on a coherent wavefront; the waves incident on receivers are centred on transmitters elsewhere. This means that if we were shown a film depicting radiation, and could see the wavefronts, then we could tell whether what was depicted was transmission or reception (in the usual temporal orientation), without first knowing whether the film was being projected forwards or backwards. Indeed we could tell whether the film was running forwards or backwards, by noting whether concentric wavefronts appeared to be diverging or converging, respectively. Isn't this the crucial difference between transmitters and receivers, in virtue of which radiation may be said to be intrinsically asymmetric?

It seems to me that this apparent difference does reflect the most common understanding of the doctrine that radiation is retarded but not advanced. However, I have deliberately approached it indirectly, via the banking analogy; for I wanted to ensure that when this difference turns out to be quite superficial, it cannot be objected that I have misinterpreted the doctrine (and that there is some more successful version waiting in the wings).

What shows that this difference is superficial, in my view, is the correct interpretation of the mathematical core of the Wheeler-Feynman absorber theory. I shall describe this interpretation in the following section. It appears to show that without inconsistency, we may say that both transmitters and receivers are centred on coherent wavefronts. The same radiation may be represented either as a sum of wavefronts diverging from its transmitters, or as the sum of wavefronts converging on its receivers. The beauty of the Wheeler-Feynman result—a beauty apparently missed by its authors—is to show that these representations are mathematically equivalent.

II.7 Reinterpreting the absorber theory

As we noted earlier, the Wheeler-Feynman argument addresses the question as to why an accelerated charge does not radiate symmetrically into the past and the future. The guiding idea is that even if radiation were actually symmetric in this sense, the contribution of future absorber might be such as to give rise to the asymmetric result apparently observed. To this end, Wheeler and Feynman consider the effect on the charged particles of a future absorber of a full retarded wave from an accelerated charge i. (See diagram 1)

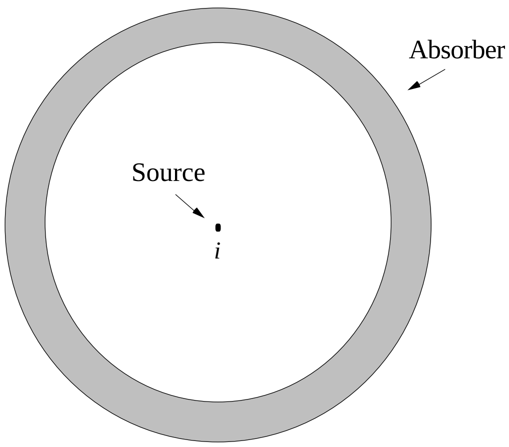

Diagram 1

On the assumption that the response of the absorber is

1/2 advanced + 1/2 retarded

(i.e., in the assumed terms, 'fully symmetric'), Wheeler and Feynman show that in the space between i and the absorber, this absorber response contributes a wave equal in value to one half the retarded field from i. On the assumption that the initial contribution from the source is also one half retarded plus one half advanced, this result is interpreted as explaining the existence of a full retarded wave in the space between i and the absorber. This wave is thus attributed one half to i itself and one half to the response of the absorber. In a similar way it is proposed that the observed absence of an advanced wave from i is really the net result of destructive interferences between a one half advanced wave from i and a minus one half advanced absorber response. In summary, the source contributes a radiation field of the form

1/2 retarded + 1/2 advanced.

The absorber contributes

1/2 retarded – 1/2 advanced.

The net result is therefore a retarded wave of the full value assumed originally.

The argument represents the original retarded wave between i and the absorber as a sum of two equal components, one identified as the one half retarded wave from i and the other as the combined one half advanced wave from the absorber. If we are to be justified in summing these components, however, we must have grounds for taking them to be distinct in the first place. If instead all we have is two different ways of describing the same wave, then there are not really two components to constructively interfere with one another. Do Wheeler and Feynman have any justification for the claim that these component waves are actually distinct? At first sight, it might be thought that the justification lies in the fact that the waves concerned have different sources. The former component originates from i, whereas the latter originates at the absorber (albeit under the influence of the retarded wave which itself originates at i). This move lands the argument in further trouble, however. For in order to derive a response of the required magnitude from the absorber, the argument requires a full strength retarded wave from i. At this stage the full retarded wave needs to be treated as 'sourced' at i. (After all, if we began with only the one half retarded wave, the response of the absorber would also be halved—leaving us 25% short of our target, even if summing is allowed.) By the time the argument reaches its conclusion, one half of this fully retarded wave is being accounted for as an advanced wave from the absorber. This in itself is not inconsistent, as long as we are prepared to allow that waves needn't have a unique source—i.e., that it is simply a matter of our own temporal perspective whether we say that the given wave originates at i or originates at the absorber particles. However, if we do allow that sources are non-unique and perspective-dependent in this way, we can't then distinguish the one half retarded wave from i and the one half advanced wave from the absorber by appealing to a difference in their sources. In other words, we still have no reason to deny that these 'components' are really one and the same wave—no justification for 'adding them up'.

This objection to the Wheeler-Feynman argument seems to me to be quite unanswerable. Certainly it can't be avoided by talk of 'self-consistency' and the like, for it is not a problem of inconsistency in the first place. It is rather an issue of our entitlement to a crucial assumption, an assumption concerning the physical distinctness of the waves meeting two different characterisations.

However, let us now reinterpret the Wheeler-Feynman argument in the light of our conclusion in the previous section that the real puzzle about radiation is that only transmitters appear to be centred on coherent wavefronts. The reinterpreted argument begins in the same way as before, with the supposition (now no longer in need of justification) that a transmitter i radiates a fully retarded wave. As before, we suppose i to be surrounded by a shell of charged particles, and that these act as receivers, transferring energy from the field to non-electromagnetic forms. Because they are receivers, we now expect that from their point of view the radiation associated with this field is fully advanced. (Recall that this is simply analogous to the point that when we withdraw money from a bank, it appears in our account before but not after the time of the transaction.)

However, let us also assume that contrary to appearances, this radiation is coherently centred on the absorber particles. In other words, we assume that each absorber particle is centred on what in the usual time sense looks like a converging coherent wavefront.

The mathematical reasoning employed by Wheeler and Feynman then shows that in the region between i and the receiver, this field is equal in value to the original wave from i. (The original argument had a one half field at this point: the difference stems from the fact that the 'response' of the receiver is now expected to be fully advanced.) How is this equality to be explained? By the fact, I suggest, that these waves are physically identical. In other words, we have shown that the same radiation field may equivalently be described either as a coherent wavefront diverging from i, or as the sum of coherent wavefronts converging on the absorber particles. Interpreted in this way, the Wheeler-Feynman argument shows that the asymmetry identified in the previous section is no more than apparent: without inconsistency we may say that both transmitters and receivers are actually centred on coherent wavefronts. More generally, in the case of a free charged particle accelerated by incoming electromagnetic radiation, which then re-radiates the received energy to the field, we may say that both the outgoing and the incoming radiation takes the form of a coherent wavefront centred on the particle concerned.

In passing, let me note a further advantage of this reinterpretation of the Wheeler-Feynman argument. We have seen that on the orthodox interpretation, the observed fully retarded wave is represented as the sum of two equal components, one from the source and the other from the absorber. The presence of the absorber is therefore crucial; without it, the same reasoning would suggest that we would observe only a one half retarded field from the source—and also a one half advanced field, since this would now be uncancelled by an advanced field from an absorber. It is true that Wheeler and Feynman suggest a way to avoid this consequence. This involves other problems, however, for it requires the presence of explicit advanced effects. Thus the orthodox version of the absorber theory gives us a choice between on the one hand a very strong cosmological constraint, namely that the future universe is an (almost) perfect absorber; or on the other hand the conclusion that in in so far as the future universe is transparent, a corresponding amount of source free radiation needs to be postulated in the early universe. Most physicists have seen this as a disadvantage of the theory, in that it exposes what purports to be the proper defence of the time-symmetric view of electromagnetic radiation to the contingencies of future cosmology.

The present reinterpretation does not appear to have any such consequence. It allows us to say that where the absorber is absent, the retarded wave from the source particle $\underline{i}$ simply propagates to future infinity. What matters is that to whatever extent there is an absorber, its interaction with the field may be consistently redescribed in terms of coherent waves converging on the absorber particles. The general result will be that the combined contribution of these waves, when taken together with the remaining unabsorbed component, yields the same physical entity as does the original description. Thus the crucial difference is that the usual retarded wave is no longer taken to <u>need</u> two (finite) sources, one in the past and one in the future; the claim is simply that in so far as such a wave does have two such sources, their contributions are entirely consistent. Time-symmetric electromagnetism is thus freed of the constraints of cosmology.

II.8 <u>Conclusion</u>

With this result in place, I think we at last have an understanding of the true asymmetry, such as it is, of radiation in the world as we know it. It is simply the fact that there are large transmitters, but not apparently any large receivers. In other words, it is rather as if a bank account were to gain much of its funds from a relatively small number of very large deposits, but to lose them only to a very large number of very small withdrawals. There is certainly a temporal imbalance in such a case, but it doesn't lie in the banking processes themselves. To explain it we should look to the boundary conditions at the bank's connections to the outside world. Similarly in the case of radiation: what needs to be explained is why there are large coherent sources, and why there are not large coherent receivers or sinks, at least in our region of the universe. This seems to be a sub-species of the question we are let to by the statistical treatment of thermodynamics, once

Loschmidt's insight is taken into account: Why do we find statistically improbable initial conditions, and not analogous final conditions?

It turns out then that the asymmetry of radiation is not a separate problem to that of thermodynamics. However, the connection is more direct than most treatments of the problem would suggest. In particular there is only one sort of radiative process, which is intrinsically neither retarded nor advanced. The temporal imbalance is purely a matter of boundary conditions. The question as to why these boundary conditions should obtain is an instance of the general question as to why real physical systems are so often found to be initially in low entropy states.

As we have already observed, the arrow of time thus directs our attention to cosmology; to the temporal extremities of the universe, and to the theoretical limits of our current understanding of the universe. We come to the point at which even the most determined philosopher must be content to await the deliverances of theoretical physics. In this frustrating yet unavoidable condition, we are very much indebted to the skills of philosophically inclined physicists such as Davies and Zeh. Our dependent condition should not blunt our critical faculties, however. On the contrary: if we must beg for the answers, the best way to redress the balance is surely to try to ensure that our benefactors have not begged the questions.[3]

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[3] I am very grateful to H. D. Zeh for comments and considerable patience, in correspondence on these issues.

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