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Recent Work on the Arrow of Radiation*

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Abstract

In many physical systems, coupling forces provide a way of carrying the energy stored in adjacent harmonic oscillators from place to place, in the form of waves. The wave equations governing such phenomena are time-symmetric: they permit the opposite processes, in which energy arrives at a point in the form of incoming concentric waves, to be lost to some external system. But these processes seem rare in nature. What explains this temporal asymmetry, and how is it related to the thermodynamic asymmetry? This paper attempts to clarify these old issues, in the light of recent contributions.

After brief introductory remarks (§1), the paper is in three main parts. §2 examines the so-called 'Sommerfeld Radiation Condition', arguing that its link to the observed asymmetry is much less direct than commonly supposed. §3 begins with Zeh's proposal to make the Sommerfeld condition an ingredient in an explanation of the observed asymmetry, and makes explicit a useful distinction between two ways in which the thermodynamic asymmetry might connect to the radiation asymmetry. §4 reviews a proposal I have defended in earlier work about the relation of the radiative asymmetry to that of thermodynamics, and defends it against recent objections by Zeh and Frisch. I also distinguish it from a recent proposal due to North. I agree with North that the observed asymmetry of radiation stems from the low entropy history, but argue that she mis-characterises the asymmetry, and hence misses a crucial element in a proper account of the role of the low entropy past.

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1 Introduction

In many physical systems, coupling forces provide a way of carrying the energy stored in adjacent harmonic oscillators from place to place. The process is called *radiation,* and the moving patterns of oscillations are known as *waves.*

When energy is added to such a system at a point, it radiates away as a concentric outgoing wave. Imagine, say, the explosion produced by a floating firecracker. The resulting pressure waves under the water, ripples on the water surface, sound waves in the surrounding air, and electromagnetic waves comprising the flash of light from the explosion, are all examples of radiated energy of this concentric outgoing kind. (Alternatively, consider the case in which the source of the radiated energy is a small boy in a tub, as in Figure 1.[1])

Figure 1: The young James Clerk Maxwell, making waves.

It is well known that in the general case, as in Maxwell's theory, the wave equations governing such phenomena are time-symmetric. Accordingly, they permit the opposite processes, in which energy arrives at a point in the form of incoming concentric waves, perhaps then to be lost to some external system. But these processes seem to be very rare in nature. What explains this temporal asymmetry, and how, if at all, is it related to the thermodynamic asymmetry?

For almost a century, these issues have been a source of controversy. Most discussion has focussed on electromagnetic radiation, but as writers such as Davies (1974) and Zeh (2001, 2005) have pointed out, the problem is more general. Many kinds of radiation are governed by wave equations of the same time-symmetric form; and in all cases, the same surface temporal asymmetry seems manifest in the real world. This paper attempts to clarify the issue of the source of this asymmetry, in the light of some recent contributions to this old debate. I defend a conclusion that I have argued for in earlier work (especially

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[1] Reproduced from L. Campbell and W. Garnett, *The Life of James Clerk Maxwell,* London: Macmillan, 1882, p. 42, by kind permission of Sonnet Software, Inc.

Price 1996, Ch. 3), viz., that the observed asymmetry of radiation has the same origins as that of thermodynamics (in a sense to be made more precise).

The remainder of the paper is in three main parts. §2 focusses on the so-called *Sommerfeld Radiation Condition,* often invoked in attempts to characterise or explain the asymmetry of radiation. Taking characterisation first, I argue that the link between the Sommerfeld condition and the observed asymmetry is less direct than commonly supposed. A common proposal about how the asymmetry should be characterised, closely related to the Sommerfeld condition, turns out to be neither necessary nor sufficient for the asymmetry we actually observe. This part of the paper thus aims to clarify the nature of the observed asymmetry, and the proper role, if any, of various interpretations of the Sommerfeld condition.

As noted, some writers offer the Sommerfeld condition as an ingredient in an *explanation* of the observed asymmetry. §3 begins with Zeh's proposal of this kind, and develops some more general points. In particular, I want to make explicit a distinction presently only implicit in the literature (to my knowledge), between two ways in which the thermodynamic asymmetry might connect to the radiation asymmetry. I'll defend one conception of the connection rather than the other (thereby disagreeing with Zeh himself, amongst others). In this context, I'll also call attention to a significant lacuna in the implementation of this solution in one particular case, that of classical electrodynamics; but argue that the lacuna should be regarded as another manifestation of a familiar defect of the classical theory, rather than an objection to the proposed diagnosis of the connection between the asymmetries of radiation and thermodynamics.

Finally, in §4, I'll restate my own proposal about the relation of the observed asymmetry of radiation to that of thermodynamics, and defend it against several recent objections by Zeh and Frisch. I'll also distinguish it from another recent proposal about the connection between the radiation arrow and the low entropy past, due to Jill North. I agree with North that the observed asymmetry of radiation does stem from the low entropy past; but I'll argue that she mischaracterises the asymmetry, and hence misses a crucial element in a proper account of the role of the low entropy history.

2 Sommerfeld's puzzling prescription

For definiteness and simplicity, let's begin with a familiar non-electromagnetic case. Consider a water surface, infinite in all directions. Add a boundary S surrounding some finite region. According to a well-known result due to Kirchhoff (see, e.g., Davies 1974, Ch. 5, Zeh 2001, Ch. 2) the amplitude of the wave at a particular place and time $(\mathbf{r}, t)$ within S may be represented in two equivalent ways, according to what I shall call the *Representation Theorem*:

$$\phi(\mathbf{r}, t) = F_{ret} + F_{in} = F_{adv} + F_{out}.$$

Here F_{ret} is a sum over contributions of outgoing concentric waves of sources within S on the past 'light cone' of the point $(\mathbf{r}, t)$. F_{in} comprises two components, one the corresponding integral over sources outside S and the other the sum of 'free fields' – waves not associated with sources, coming in from 'past infinity'. Similarly, F_{adv} is a sum over contributions of incoming concentric waves converging on sources within S on the future 'light cone' of the point $(\mathbf{r}, t)$. And F_{out} involves two components, one the corresponding contribution associated with future sources outside S and the other the sum of 'free fields' – source-free waves going out to 'future infinity'.

I'll often use the case of water waves as an illustrative example, but the Representation Theorem itself is quite general, applying to wave phenomena of all the kinds already mentioned. From now on, I'll write the general result in the following familiar form (suppressing the coordinates on the left-hand side):

$$F = F_{ret} + F_{in} = F_{adv} + F_{out}. \tag{RT}$$

At first sight, RT seems in tension with observed asymmetry with which we began. After all, doesn't RT imply that what we observe must be equally describable in terms of either outgoing or incoming waves – in which case, surely, it isn't true that nature prefers one to the other? But that can't be right, because we can see that there is an asymmetry in the phenomena. So the correct moral is simply that we need to do some more work to say what the asymmetry is.

The stock suggestion is that we recover the observed asymmetry from RT by paying attention to the 'boundary conditions' – i.e., to the terms F_{in} and F_{out}. It is suggested that asymmetry stems from – or at least may be characterised in terms of – an asymmetry between F_{in} and F_{out}. The crucial thing, it is claimed, is the Sommerfeld Radiation Condition:

$$F_{in} = 0. \tag{SRC}$$

Taken together, RT and SRC immediately imply what I'll term the *Pure Retardation Condition*:

$$F = F_{ret}. \tag{PRC}$$

It is widely assumed that PRC provides a characterisation of the asymmetric character of radiation observed in the real world. Thus Davies (1974, 114) says that an expression of this form "correctly describes the situation illustrated by the examples cited above", where the examples in question include such things as the results of throwing stones into ponds. Later, in discussing the electromagnetic case, Davies says that "to obtain the usual retarded fields that are experienced in the real world it is necessary to impose a boundary condition on the system … known as the Sommerfeld radiation condition." (1974, 128)

The persistence of the view that PRC characterises the temporal asymmetry observed in the real world is puzzling, for as we'll see in a moment, it is easy to show that PRC is neither necessary nor sufficient for the observed asymmetry. While there is some connection between the observed asymmetry and SRC, it turns out to be very much less direct than the simple argument to PRC suggests.

In exploring these ideas, we'll see that there are several distinct versions or interpretations of the Sommerfeld condition, which need to be carefully distinguished. Since they are logically distinct, at most one could provide necessary and sufficient conditions for the observed asymmetry – and it is possible that none of them does so. As we'll see, however, one version comes at least usefully close to doing so.

Henceforth, because we are going to be discussing several versions of the Sommerfeld condition, I'll use the term 'SRC' generically. I'll call '$F_{in} = 0$' the *exact* SRC. Our first task is to challenge to common view that the observed asymmetry of radiation is properly characterised by – or follows immediately from – this exact form of the Sommerfeld condition.

2.1 The exact interpretation of SRC

Should we interpret SRC as maintaining that $F_{in} = 0$ exactly, or merely that $F_{in} \approx 0$? There is certainly some attraction in the thought that the former condition is required to explain the observed asymmetry. After all, isn't the observed asymmetry 'pure' or 'perfect', in the sense that radiation is 100% outgoing? And to get this from RT, don't we need that $F_{in} = 0$ exactly?

Well, let's see. As noted above, the formal argument from RT and the exact SRC to the observed asymmetry simply rests on the fact that RT and $F_{in} = 0$ together imply PRC. If PRC did amount to a characterisation of the observed asymmetry, this reasoning would be unassailable. But it does not! PRC and the observed asymmetry turn out to be logically independent of one another. To the extent that there is a connection between some version of SRC and the observed asymmetry, it is a good deal more subtle, and doesn't rest on PRC.

Here are two reasons for thinking that the exact SRC (or equivalently, PRC) is neither necessary nor sufficient for observed asymmetry.

(1) At least for some kinds of wave phenomena, there are possible solutions in which both $F_{in} = 0$ and $F_{out} = 0$. (Intuitively, imagine that the sources of ripples are surrounded by a good absorber, so that no waves can escape over the boundary S.) If such cases are possible – and *mere* possibility is enough, for the purposes of this point – then the former condition cannot be *sufficient* for an observable asymmetry, on pain of contradiction. Whatever the observed asymmetry amounts to, it is certainly an asymmetry, and so couldn't consistently hold in both orientations at once.[2]

(2) There are many cases in which there are incoming waves across the boundary S, yet normal outgoing ripples produced by sources such as falling stones within S. (This will be the case if S is an arbitrary boundary on any normal non-ideal pond.) In these cases the exact SRC fails ($F_{in} \neq 0$)

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[2] In the EM case, there is a very well-known approach to the radiative asymmetry, the Wheeler-Feynman Absorber Theory, which begins with the assumption that both $F_{in} = 0$ and $F_{out} = 0$, and then proceeds to try to derive the observed asymmetry from thermodynamic considerations. Whatever the merits of this approach, it clearly requires that $F_{in} = 0$ not be sufficient for the observed asymmetry, on pain of the inconsistency just noted.

> and yet we do have the observable asymmetry. So the exact SRC is not *necessary* for that asymmetry, either.

These are such obvious points that it hard to see how anyone could think that PRC accurately characterises the observed asymmetry – and yet, as I've noted, the view that it does so is common in the literature.[3]

With an eye to discussion later in the paper, let's reflect on the kind of case considered in (1), in which both $F_{in} = 0$ and $F_{out} = 0$. As noted above, these conditions are met, at least to a high degree of approximation, by the case in which the outgoing ripples produced by a pebble are absorbed within the boundary S (perhaps by the kind of device used as lane-dividers in swimming pools, for example, for exactly this wave-damping purpose). The representation theorem shows that the outgoing ripple from the pebble impact can also be described as a sum of many incoming ripples, each converging on a particular location within the absorber. So it is simply *false,* in this case, to say that there is an observed asymmetry which consists in the fact that we observe outgoing ripples but not incoming ripples. On the contrary, what we observe is precisely what we should expect observe, on the hypothesis that there are incoming ripples! This doesn't mean, of course, that there is no observable asymmetry. Once again, all it means is that the observed asymmetry does not consist in the (supposed) fact that we observe outgoing ripples but not incoming ripples – for that supposed fact is no fact at all.

What, then, is the observed asymmetry? The obvious thought is that we need to take the size and the number of ripples into account. This case seems to exemplify a much more general feature of the wave phenomena we observe, in displaying a sharp contrast between a *small number* of *large* outgoing ripples, and a *large number* of *small* incoming ripples. We'll return to this idea in due course.[4] For the moment, we're interested in whether the observed asymmetry can be characterised in terms of some version of SRC. So far, we've learnt that the orthodox suggestion – i.e., that the crucial condition is $F_{in} = 0.$ and hence PRC – is inadequate.

It might be suggested that what the orthodoxy actually has in mind is a stronger version of the exact SRC, requiring not only that $F_{in} = 0$ but also that $F_{out} \neq 0$. This stronger exact SRC couldn't be necessary for the observed asymmetry, of course (given that the weaker condition isn't necessary).[5] But could it perhaps be sufficient, where the weaker condition was not?[6]

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[3] It might be objected that it is simply a terminological matter that PRC amounts to the claim that radiation is fully retarded. If this is what we mean by 'fully retarded', however, then the above arguments show that the observed asymmetry of radiation is *not* that radiation is fully retarded – on the contrary, the arguments show that being fully retarded (in this sense) is neither necessary nor sufficient for the observed asymmetry.

[4] I'll call this the *FLOMSI* asymmetry: *F*ew *L*arge *O*utgoing, *M*any *S*mall *I*ncoming.

[5] Moreover, as Frisch (2005a, 5) points out, one man's F_{out} is the next man's F_{in}, so that it simply can't be true generally that $F_{in} = 0$ and $F_{out} \neq 0$, for arbitrary choice of hypersurfaces.

[6] Sufficiency might be enough to enable this version of SRC to *explain* the observed asymmetry, at least in some cases. As we'll see in §3, Zeh defends a view of this kind. He argues that thermodynamic factors ensure that $F_{in} = 0$ but not that $F_{out} = 0$, in typical circumstances; and that this explains the observed asymmetry of radiation, at least in many cases.

Again, however, it seems not, for two reasons:

(3) There are cases in which $F_{in} \neq 0$ and $F_{out} = 0$. For example, imagine there is an absorber surrounding the sources within S, and hence preventing any waves escaping over the boundary S 'towards the future'. But imagine further that this absorber is not in place in time to prevent waves coming in 'from the past'. This case is clearly compatible with the fact that a pebble will produce the normal outgoing ripple. By symmetry, then, the time-reversed case is also at least *mathematically* possible: this would be a case in which we do have $F_{in} = 0$, and $F_{out} \neq 0$, but without the usual asymmetry – in fact, with the reverse asymmetry! This shows that the revised condition – $F_{in} = 0$, and $F_{out} \neq 0$ – is not sufficient for the observed asymmetry.

(4) Imagine all sources are microscopic – dust settling over a previously still pond, for example. In this case, surely, we might have $F_{in} = 0$ and $F_{out} \neq 0$, but without any *observable* asymmetry. (Certainly there will be no readily observable rings of outgoing ripples.) If so, then again, the revised condition turns out to be insufficient for the observed asymmetry.

Once again, then, the moral of these cases is that it is a mistake to think that the observed asymmetry follows from SRC, at least in the exact forms we've considered so far. In particular, the observed asymmetry isn't correctly characterised either by PRC (which is what follows from RT and the condition $F_{in} = 0$); or by PRC and the additional requirement that $F_{out} \neq 0$.

Nevertheless, the examples considered in (3) and (4) do serve to confirm our earlier suggestion about what's missing, for an adequate characterisation of the observed asymmetry. Both examples depend crucially on the presence of large numbers of very small sources. (This is immediate in the case of (4), and true of (3) because of the essential role the absorber plays in the example.[7]) Thus they suggest that SRC might be on the right lines, at least in the stronger form which adds that $F_{out} \neq 0$, provided we restrict attention to cases involving small numbers of *macroscopic* or *large scale* sources.

We'll return to this suggestion below. First, I want to consider two different proposals about how a modified version of SRC might characterise the observed asymmetry. One relies on a different form of the exact SRC, and the other proposes an inexact SRC, requiring only that $F_{in} \approx 0$, not that $F_{in} = 0$ exactly.

2.2 Counterfactual interpretations of SRC

There is at least one case in which the exact Sommerfeld condition seems appropriate. It is when we reason counterfactually, thinking about the *difference* between a case in which there is and a case in which there isn't some disturbance to the medium at a time t_1. Ordinary counterfactual reasoning seems to require

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[7] A fact which provides a response for Zeh, who is able to argue that the case described in (4) is physically unrealistic, given the thermodynamic properties of absorbers. More on this below.

that the two scenarios are the same before t_1, so that SRC applies exactly to the *difference* between the two cases.

This fact provides a very natural interpretation of some of the things that people say in attempting to characterise the radiation asymmetry. ('If we were to disturb the water surface, the effects would show up after the disturbance.')[8] However, the fact about counterfactual reasoning on which this relies – the fact that in some sense, we normally 'hold fixed the past' when reasoning counterfactually – is clearly of much wider scope than just the radiation cases. Moreover, it is clear that it also characterises our thinking about cases in which it seems obvious that there is no observable time asymmetry in the processes themselves, such as the behaviour of small collections of Newtonian particles. ('If we were to jiggle this particle, the effects would show up after but not before the jiggle.') This should make us suspicious about whether it correctly captures the observed asymmetry of radiation.[9]

Here's another way to make this point. On this counterfactual reading, SRC concerns not the state of the actual world alone, but a relation between the actual world and some other possible world – viz., the one we think about when we assess the counterfactual. Yet the observed asymmetry – the obvious (even if difficult to characterise) imbalance between outgoing and incoming ripples – is surely a fact about the actual world, all by itself.

The association of something that looks very like SRC with this counterfactual asymmetry has probably contributed to the popularity of the idea that SRC correctly characterises the observed asymmetry. To the extent that this is so, however, the view rests on an equivocation – on a failure to distinguish 'actual' from counterfactual readings. For present purposes, our interest is in the *actual* and *actually observable* asymmetry of radiation. We should therefore put these counterfactual issues to one side; while emphasising that we need to be on our guard that apparent asymmetries elsewhere are not merely the counterfactual asymmetry in disguise.[10]

2.3 Comparative interpretations of SRC

Before putting the counterfactual cases entirely to one side, however, it is worth asking whether there is related 'actual' asymmetry in the near vicinity. It is a

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[8] Frisch offers an explicitly counterfactual characterisation of the 'retardation condition' he takes to characterise EM radiation: "The field component associated with a source is that component of the total field that *would be absent, if the source were absent.* ... And the retardation condition tells us that the field physically associated with a charge is fully retarded, rather than being some other linear combination of retarded and advanced fields." (Frisch 2005b, Ch. 7, my italics)

[9] Indeed, these cases ought to make us suspicious about whether the counterfactual asymmetry is an objective asymmetry at all, in my view, rather than a kind of projection of our own asymmetric temporal perspective. (See Price 2006, for more on this issue.)

[10] Cf. Zeh (2005, 20): "In causal *language,* where $[F_{in}]$ is regarded as fixed and given, the source 'creates' precisely its retarded field that has to be added to $[F_{in}]$ in the future of the source." (Zeh himself is well aware that the time-asymmetry of ordinary causal and counterfactual notions needs to be treated with suspicion, rather than simply taken for granted, in attempting to account for observed asymmetries.)

familiar idea that counterfactual claims rest on generalisations. Perhaps there is some generalisation related to the counterfactual SRC, that does capture the observed asymmetry of radiation?

The following seems a tempting thought. Let C be a configuration of waves in a spacetime region, characterised in a moderately general manner, and of a kind found to occur in our experience. Let C'_{ret} be the kind of configuration that differs from C by the addition of a source of observable size, in a manner such that $F_{in} = 0$ applies to the *difference* between C and C'_{ret}. Similarly, let C'_{adv} be the kind of configuration that differs from C by the addition of a source of observable size, in a manner such that $F_{out} = 0$ applies to the difference between C and C'_{adv}. Then if configurations of type C are common, so too, typically, are configurations of type C'_{ret}. But configurations of type C'_{adv} are rare.

I'll call this the *comparative* version of SRC (or the *actual* comparative version, if necessary, to distinguish it from the counterfactual version). I'll return to it below. First, I want to consider an alternative approach, resting on the idea that what is essential to SRC is that $F_{in} \approx 0$, where the approximation is in comparison to the radiation associated with typical observable sources.

2.4 Inexact interpretations of SRC

By an inexact interpretation of SRC, I mean an interpretation requiring only that $F_{in} \approx 0$, not that $F_{in} = 0$ exactly. There are two main motivations in the literature for interpreting SRC in this form. One is that the stricter condition is simply unrealistic, because F is rarely exactly zero, in real situations. As Frisch puts it, there are 'many situations where the free incoming fields are not equal to zero, even in the presence of a single coherent source (for example when I turn on the light in a room on a sunny day).' (Frisch 2005a, 6)

The second motivation stems from a proposal to explain the observed arrow of radiation in terms of the thermodynamic properties of typical absorbers, a proposal which rests on the suggestion that absorbers ensure that $F_{in} \approx 0$ in many situations. I'll defer an examination of this latter proposal until §3, and concentrate here on the first motivation for an inexact interpretation of SRC.

With explanatory issues set to one side, the issue, once again, is whether the inexact SRC can provide an adequate characterisation of the observed asymmetry. On the face of it, it seems subject to same problems as the exact version.

(5) At least for some kinds of wave phenomena, there seem to be possible solutions in which both $F_{in} \approx 0$ and $F_{out} \approx 0$. (Indeed, this possibility is bound to be easier to achieve than in the exact case, because the condition is more relaxed.) Again, then, the former condition cannot be *sufficient* for an observable asymmetry, on pain of contradiction.

(6) There are many cases in which there are *substantial* incoming waves across the boundary S, yet normal outgoing ripples produced by sources such as falling stones within S. Here the possibility is less easily achieved than in

> the exact case, but still common. Imagine throwing rocks into a patch of stormy sea: one observes outgoing ripples, even though F_{in} is itself large (corresponding to the fact that the storm supplies vastly more energy to the water surface than is introduced by one moderately-sized rock). In such cases the inexact SRC fails, then, and yet we do have the observable asymmetry. So the exact SRC is not *necessary* for that asymmetry, either.

In the case of (5), however, a response immediately suggests itself, comparable to a point made in §2.1. It seems that in practice we only find both $F_{in} \approx 0$ and $F_{out} \approx 0$ in cases in which either there are no observable sources within the region S, or there are very many sources – enough to provide complete absorption of the outgoing ripples produced by falling pebbles, as it were. If this is correct, it suggests that $F_{in} \approx 0$ is sufficient for the observed asymmetry, in cases in which there is a small number of (significantly sized) sources within S.

This leads in the direction of the kind of characterisation offered by Frisch (2005a, 6). There are many cases in which we have a few large sources, and can represent what we see in terms of the retarded fields of these sources plus small F_{in}. But there are few, if any, cases of the reverse kind: cases in which we have a few large sources, and can represent what we see in terms of the advanced field of these sources, plus small F_{out}. Thus if we are told that a system contains a small number of macroscopic sources, the inexact SRC seems sufficient to ensure that those sources are associated with retarded or outgoing waves.

We seem to be making some progress, but it is important to recognise what we have *not* established. We have not shown that the inexact SRC provides a *characterisation* of the observed asymmetry, even subject to the additional restriction to systems containing a small number of macroscopic sources, for we have not established that $F_{in} \approx 0$ is necessary for the observed asymmetry. On the face of it, as (6) notes, there are many cases in which the inexact SRC does not hold, in which we still find the usual asymmetry.

2.5 Summary: Sommerfeld and the observed asymmetry

We've been exploring the prospects for various versions of the Sommerfeld condition, as a *characterisation* of the observed asymmetry of radiation (having first distinguished the task of characterisation from that of *explanation*). I've argued for the following main conclusions:

a. The exact SRC, $F_{in} = 0$, is neither necessary nor sufficient for the observed asymmetry. Accordingly, the usual conflation of the condition $F = F_{ret}$ (i.e., PRC) with the observed asymmetry is simply mistaken, as it stands. To put it another way, if PRC is what we mean by saying that radiation is 'fully retarded', then the observed asymmetry is *not* that radiation is fully retarded.

b. All of this applies, *mutatis mutandis,* to the inexact case, $F_{in} \approx 0$. However, some of the limitations of the inexact SRC (as a characterisation of the observed asymmetry) can be met by a kind of 'coarse-graining', in which we restrict our attention to macroscopic sources. As we've just seen, there are many cases in which we can represent what we see in terms of the retarded fields of such sources plus a small F_{in}, but few, if any, cases in which the reverse is true.

c. Nevertheless, example (6) stands squarely in the way of a general characterisation of the observed asymmetry in terms of the condition $F_{in} \approx 0$. In the familiar cases we've been considering (water waves within a boundary S), F_{in} can be much larger than the component of F_{out} intuitively associated with the sources within S, without any inconsistency with the observed asymmetry.[11]

d. The counterfactual and the comparative versions of SRC both deal with this difficulty easily, by focussing on the *difference* between two cases, so that the large F_{in} of such examples simply gets factored out. However, the counterfactual SRC is not a plausible characterisation of the *observed* asymmetry, for the reasons noted in §2.2. So the comparative interpretation seems to provide the most general characterisation of the observed asymmetry, retaining something of the spirit of the Sommerfeld condition.

e. However, we've seen that we only get this far with SRC by restricting our attention to 'macroscopic' sources, and it is arguable that if we are taking this step in any case, the observed asymmetry can be characterised more directly: in our experience, macroscopic sources of retarded or outgoing waves are common; macroscopic sources of advanced or incoming waves are extremely rare.[12]

It is true that characterising the observed asymmetry 'directly' in this way, as an imbalance (at the macroscopic level) between two kinds of sources, goes against a long tradition in discussions on this topic. Motivated in part by the fact that any given representation of the field associated with a source is not unique – so that a retarded wave can always be represented, instead, as a sum of an incoming wave and a suitably chosen free field – writers have tried to appeal to some asymmetry in the initial and final free fields, to pin down the asymmetry so evident in ordinary wave phenomena. But this tradition deserves to be challenged, in my view. Among other failings, it conflates two problems, that ought to be kept more distinct: one the one hand, the problem of the nature and origins of the observed asymmetry; on the other hand, the problem (or supposed problem) associated with the fact that fields can be represented in these alternative ways.

So far, I've argued that the tradition fails, at least to a considerable extent, in its project of *characterising* the observed asymmetry in terms of boundary conditions, as in the Sommerfeld condition. I haven't yet addressed a second element, commonly part of the same tradition: viz., the idea that the SRC

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[11] Again, simply imagine a small pebble falling into the surface of a large Pacific swell.

[12] In many common situations, we have what I called the FLOMSI asymmetry: a contrast between a *few large outgoing* waves, and *many small incoming* waves.

explains the observed asymmetry. True, the omens for this explanatory project do not seem promising. Our best version of the SRC is the comparative version, which – as a mere characterisation of a distinctive pattern – doesn't seem likely to yield an explanation. And I've suggested that it is more illuminating to abandon even this version of SRC, in favour of a characterisation in terms of the asymmetry of macroscopic sources themselves (thus moving even further from any SRC-like *explanans,* presumably).

In the next section of the paper, I want to address this explanatory part of the tradition directly. Again, my general argument is going to be that in explaining the radiative asymmetry, we need to keep our eyes on the sources, not on the kinds of boundary conditions reflected in F_{in} and F_{out}. I'll begin by considering the views of a writer who argues that the inexact SRC does explain the observed asymmetry (at least in some familiar cases), and that it itself is a consequence of the thermodynamic properties of ordinary matter.

3 An explanatory role for SRC?

In the §2.4 we distinguished the question whether the condition $F_{in} \approx 0$ can adequately characterise the observed asymmetry, from the question whether it can *explain* this asymmetry (*in toto,* or in part). One prominent writer who defends an affirmative answer to latter question is Dieter Zeh. Zeh argues that the thermodynamic properties of ordinary absorbers, such as laboratory walls, explain why $F_{in} \approx 0$ in typical situations; and that this in turn explains the observed asymmetry of typical sources within such environments.

Zeh's argument goes like this. He first introduces the notion of an ideal absorber: "A spacetime region is called an '(ideal) absorber' if any radiation propagating within it is (immediately) thermalized at the absorber temperature $T(=0)$." He then notes that this definition "means that no radiation can propagate within ideal absorbers, and in particular that no radiation may leave the absorbing region along forward light-cones"; illustrating the point with the diagram reproduced here as Figure 2.[13]

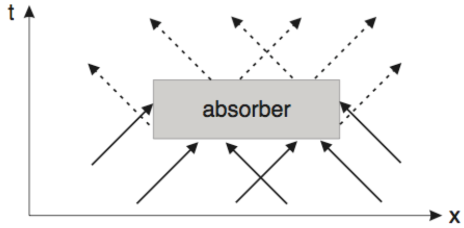

Figure 2: Zeh's ideal absorber.

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[13] Zeh's own caption to this diagram is as follows: "Ideal absorbers do not contribute by means of G_{ret}. (Arrows represent here the formal time direction of retardation.)" (2005, 23)

Zeh then argues as follows:

> Such a boundary condition simplifies the initial value problem considerably. If the space-like part ... of the boundary required for the retarded form of the boundary value problem ... consists entirely of ideally absorbing walls (as is usually an excellent approximation in a laboratory), the condition $[F_{in} = 0]$ applies shortly after the initial time t_1 that is used to define incoming fields. So one finds precisely the retarded fields of sources present in the laboratory. On the other hand, absorbers on the boundary would not affect contributions to the Kirchhoff problem by means of G_{adv}; in the nontrivial case one has $[F_{out} \neq 0]$. Therefore, in this laboratory situation the radiation arrow is a simple consequence from the thermodynamical arrow characterizing absorbers. (2005, 23)

In my view, the weak point in this argument is the assumption that laboratory walls are 'an excellent approximation' to ideal absorbers, in Zeh's sense. Given this assumption, the argument certainly seems unassailable. However, it seems to me that the assumption effectively begs the question, albeit in a rather subtle way. I want to argue that we have no right to assume that ordinary matter acts as an ideal absorber, in this sense, unless we've already assumed the radiation arrow.[14]

The point turns on a deep issue concerning the explanatory priority between what we might call the sources and the absorbers of low entropy, or concentrated energy (using the term 'source' and 'absorber' in a more general sense than in the radiation case). I will approach the point I want to make by means of an example which will also be useful later in the paper – initially, it is an example concerned only with particles, and not with radiation.

3.1 Dampers and anti-dampers

Imagine a hollow cylinder with elastic walls, containing a dilute gas of classical Newtonian particles. Suppose that one end of the cylinder is permeable to fast particles, though reflective to slow particles. Call this apparatus a *tub*.

Imagine that a fast particle from the outside arrives at the permeable wall of such a tub, and duly passes through the wall, into the interior. We know what is likely to happen. Provided that the gas is not too dilute (in which case a typical incoming particle simply bounces around until it leaves again), the incoming particle is likely to collide with particles of the gas, each of which will then collide with others, until the excess kinetic energy of the intruder becomes distributed, on average, among all the original occupants (producing a slight increase in the temperature of the gas).[15]

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[14] My criticism of Zeh's argument is similar in spirit to that of Frisch (2000), although I'll be developing it by a different route.

[15] It is *possible* that this does not happen – possible that the incoming particle acquires *more* energy, being speeded up by collisions with the molecules of the gas. But we know that in practice, this would rarely be the case. Damping is the normal frictional behaviour. (Indeed, systematic anti-damping would generate runaway solutions, in which the particle rapidly acquired most of the available energy.)

Call the intrusion a *pop,* and the subsequent dissipation a *squelch.* And call the entire combination – tub, pop and squelch – a *damper.* Figure 3 shows a tub behaving as a damper. (The double-headed arrow represents the incoming fast particle, and the single-headed arrows the slow particles with which it collides.)

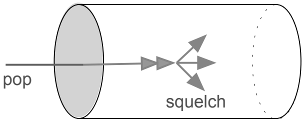

Figure 3: A damper.

What would a time-reversed damper look like? It would be a case in which collisions *boost* the momentum of a particle sufficiently for it to escape through the permeable end-wall of the tub in question. Let's call the series of collisions involved in this process an *anti-squelch,* and the escape of the fast particle an *anti-pop.* The entire process thus comprises an *anti-damper* – see Figure 4.

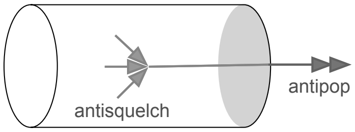

Figure 4: An anti-damper.

Provided that the relevant laws are time-symmetric, a physics which allows dampers also allows anti-dampers. However, it may seem that anti-dampers are likely to be much rarer than dampers, for reasons which mirror those given by Popper, in a famous note in *Nature* (Popper 1956), to explain why we don't see incoming water waves, expelling stones. Apparently, such a process requires an incredibly unlikely coordination between the initial motions of the many particles which need to cooperate to produce the incoming ripple, or the anti-squelch.

Let's think about this argument a little more closely, however. Imagine setting up a damper and an anti-damper end-to-end, as in Figure 5. Accidental anti-squelches are unlikely, as we've just noted, but they will occur occasionally, given enough time (and provided the parameters of the experiment are not set so as to exclude them – provided anti-pops don't require more than the total energy of the entire system, for example). When such an accidental anti-squelch occurs, it produces an outgoing pop, or anti-pop. This then becomes an ingoing pop in the adjoining tub, where it produces a squelch.

Moreover, if there's no other source of incoming fast particles, then all squelches on the right are produced by anti-squelches on the left – from which it follows, trivially, that squelches will be no more common than anti-squelches. (Of course, the same process can occur in the opposite direction. A random anti-squelch on the right can produce a squelch on the left. But again, no more squelches than anti-squelches.) In this case, then, neither tub displays a temporal bias, in the sense that it damps more than it anti-damps, or *vice versa.*

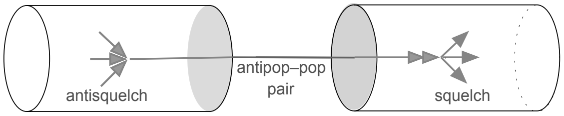

Figure 5: Using an anti-damper to produce a damper.

Yet it seems obvious that there are a lot more dampers than anti-dampers in the real world.[16] Note that it is not true to say that there are no anti-dampers at all. With suitable adjustments to the number of particles and the definition of a fast particle, anti-dampers can be made likely enough to occur on ordinary laboratory time-scales. Suitably constructed anti-dampers could be set to work in Las Vegas, for example. If you're lucky enough to witness an anti-pop, you win the jackpot. It would be easy to set the parameters so that there was say one winner a week, on average. But a casino which paid out on dampers as well as anti-dampers would soon go broke. Dampers are far more common than anti-dampers, in the world as we know it. Why?

Apparently, because there are many other sources of the incoming fast particles needed by dampers, other than neighbouring anti-dampers. If dampers paid jackpots, canny gamblers would break the bank by exposing such tubs to the sun, or firing revolvers at them – in general, by introducing a *source* of fast particles. And this points us in the direction of an explanation of the asymmetry we find in the actual world between dampers and anti-dampers. Anti-dampers occur only by chance, apparently, but dampers are common for another reason: because there are lots of (appropriately located) high temperature sources.

Why are there so many high temperature sources? Apparently, because we live in a universe in which entropy was very low at some point in the past, though not so far in the past that our region has reached thermal equilibrium. At present, then, there are still lots of high-temperature concentrations of energy, such as stars, and these in turn are responsible for lots of dampers, and many analogous processes in which these concentrations of energy gradually dissipate.

If this is the right diagnosis of the observed imbalance of dampers over anti-dampers, then it should follow that if we were to eliminate the asymmetry of hot sources, we would eliminate the imbalance between dampers and anti-dampers. Is this true? Well, there are two ways we might eliminate the asymmetry of

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[16] Or, to be more exact, more damper-like events than anti-damper-like events, under obvious generalisations of the definitions.

hot sources. The first would be to eliminate the hot sources themselves. In effect, this takes us back to the case in which fast particles are generated only by anti-dampers, and we know that in that case, there is no imbalance. We expect exactly as many anti-dampers as dampers.

The second way to eliminate the asymmetry of hot sources is perhaps less obvious. It is to introduce the same kind of low entropy condition *in the future* that is responsible for the hot sources *in the past.* If the low entropy past condition guarantees the (later) existence of stars, then, by symmetry, the time-reversed condition guarantees the (earlier) existence of what we can call anti-stars; objects that behave just like stars, only in reverse. In particular, they are 'anti-sources', or *sinks,* of fast particles. Put one next to the permeable wall of one of our devices, and it guarantees what in the ordinary time sense look like outgoing fast particles, from the tub to the anti-star. In other words, it makes the device an anti-damper.

Thus I want to say that if we take our chamber of gas, and place it next to an anti-star, it will behave anti-thermodynamically. It will become a net anti-damper, in other words, producing many outgoing fast particles. This is guaranteed by the presence of the anti-star, in just the same way that incoming fast particles (and hence net damping) are guaranteed by the presence of the normal star.[17] To accept the reasoning in one case but not the other would be to introduce a temporal double standard.

Someone might concede that anti-thermodynamic sinks would eliminate the imbalance between dampers and anti-dampers, by increasing the number of anti-dampers; but object that the implied *modus ponens* should be a *modus tollens.* In other words, it might be objected that the huge improbability of anti-dampers provides an excellent reason for discounting the possibility of anti-stars and similar anti-thermodynamic objects – and hence, eventually, for discounting the possibility of a future low entropy boundary condition.

But again, this objection skates close to a double standard fallacy. If it is based simply on statistical considerations (I'll mention another possibility below), then so long as the combinatorially-grounded statistics in question are not already time-asymmetric, anti-dampers are not intrinsically more unlikely than dampers. Yet we know that the argument doesn't exclude dampers – unlikely as they seem, by these combinatorial lights, the world nevertheless contains lots of them. So without any prior reasoning for thinking that the argument is better in one direction of time than the other, we aren't entitled to assume that the analogous reasoning is conclusive in the case of anti-dampers.

Popper himself is guilty of a similar fallacy in the note mentioned above, in my view. Recast in the present context, the Popperian claim is that anti-dampers are non-existent, or at least very rare, because they require unlikely patterns or correlations, involving many particles. However, while it is true that anti-dampers require such correlations, so, too, do dampers – and yet they do

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[17] For these purposes, in fact, we can assume that 'star' just *means* 'system whose presence guarantees *outgoing* fast particles (in our time sense), at a rate above thermodynamic fluctuation. 'Anti-star' means the same, but with *incoming* fast particles. Nothing (yet) turns on the radiative properties of either kind of object.

occur! So it can't be *simply* the unlikeliness of the correlations that explains the rarity of anti-dampers. It must, also, be the fact that this improbability isn't overridden by a low entropy boundary condition, of the kind that produces dampers (*despite* their apparent improbability). Thus, *pace* Popper, the asymmetry gets traced to the same boundary condition that seems responsible for the thermodynamic asymmetry.[18]

At any rate, this is how it seems to work for particles. I'll say more in a moment on the application of these ideas to radiation. Before that, I want to draw attention to a tradition that provides an implicit challenge to time-symmetric statistical reasoning of the kind here invoked. It rests on the idea that there might be a non-statistical reason – a reason based on what we know about *the behaviour of the material of which dampers are made* – for excluding the possibility of anti-dampers, and hence of such things as anti-stars.

3.2 Absorber-priority *versus* source-priority

According to the diagnosis just proposed, the thermodynamic orientation of our tubs of gas depends on the environment in which they are placed. In an environment containing what from our temporal point of view we describe as thermodynamic sources – *sources* of fast particles – tubs behave thermodynamically (i.e., as dampers). In an environment containing what from our temporal point of view we would describe as anti-thermodynamic sources – *sinks* of fast particles – the same tubs behave anti-thermodynamically (i.e., as anti-dampers). Let's call this the *source-priority* view.

By contrast, consider the suggestion that because the ordinary gas in a chamber has the property of 'behaving thermodynamically' – i.e., being a net damper – its presence *excludes* the possibility of anti-stars (at least in its immediate environment). It seems to be a common view that ordinary absorbers do have such a bias. In discussion reproduced in (Gold 1967, 17), for example, 'Mr X' (widely supposed to be Richard Feynman) refers to 'the assumption that matter is thermodynamically "one-sided", in the ordinary sense that it damps when you try to shake it.' On this view – the *absorber-priority* view, as we may call it – the presence of the absorber has different implications for stars and anti-stars. It allows stars, but excludes anti-stars. Since the difference between the two cases is nothing but temporal direction, this can only be the case if the absorber is thought of as having some sort of intrinsic temporal bias.

In my view, there is no reason to suppose that matter is 'one-sided' in

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[18] What Popper actually says is incoming ripples would require coordination from a common centre. The premise seems to be that without such coordination, the correlation would be wildly improbable. If so, then this is the double standard at work. (Cf. Dieter Zeh on this point: 'The popular argument that advanced fields are not found in nature because they would require improbable initial correlations is known from statistical mechanics, but absolutely insufficient .... The observed retarded phenomena are precisely as improbable among *all possible* ones, since they describe equally improbable *final* correlations.') Another way to make the point would be to agree that there has to be coordination from a common centre, but to note that we commit a double standard fallacy if we assume that that coordination has to lie in the past, and not in the future.

this way. It certainly doesn't follow from the observed bias – the familiarity of damping but not anti-damping – that the asymmetry lies in the absorber. Why not? Because in all the observed cases, there is also an asymmetry in the sources. Each observed piece of shaken matter is being shaken by some low-entropy system – some concentration of usable energy, that does work and gives up some of its usable energy in the process. (And each known case of this kind has the same time sense, of course – as I've described it, the usual time-sense of our experience.) So the asymmetry of sources is available to explain the asymmetry of absorbers; and nothing we know of in the internal dynamics of absorbers gives us any reason to locate the asymmetry there.

The choice between absorber-priority and source-priority mirrors the choice between two very different approaches to explaining the thermodynamic asymmetry in general. Some approaches to the thermodynamic asymmetry seek a dynamical factor that causes or 'forces' entropy to increase. Because this factor needs to be time-asymmetric, and yet couldn't produce the entropy gradient we observe unless entropy also *starts* at a low value, such approaches are committed implicitly to the view that it takes *two distinct temporal asymmetries,* to explain the observed thermodynamic asymmetry.

This two-asymmetry view isn't the only approach to the thermodynamic asymmetry on offer, however. The alternative was first proposed by Boltzmann in the 1870s, in response to Loschmidt's reversibility objections. To illustrate Boltzmann's approach, think of a large collection of gas molecules, isolated in a box with elastic walls. If the motion of the molecules is governed by deterministic laws, a specification of the microstate of the system at any one time uniquely determines its entire trajectory. The key to Boltzmann's approach is that in the overwhelming majority of possible trajectories, the system spends the overwhelming majority of the time in a high entropy macrostate—among other things, a state in which the gas is dispersed throughout the container. (Part of Boltzmann's achievement was to find a measure on the space of possibilities whose predictions do accord with what we see.[19])

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

On this view, then, there's no time-asymmetric factor which *causes* entropy to increase in one direction. This is simply the most likely thing to happen, given the combination of the time-symmetric Boltzmann probabilities and the

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[19] At least in one temporal direction – more on this in a moment.

single low entropy restriction in the past. This 'boundary condition' is time-asymmetric, so far as we know, but it is the only time-asymmetry in play, according to Boltzmann's approach. There's no need for a second asymmetry in the dynamics – a major advantage, it seems.

Similarly in the present case, apparently. The source-priority view is a close analogue of Boltzmann's one-asymmetry approach – indeed, it could easily be formulated as a kind of restricted case, using the Boltzmann measure itself. Whereas the absorber-priority view is committed implicitly to the existence of two time-asymmetries, in order to explain what we see: the claimed asymmetry in the dynamics of absorbers themselves, and then the asymmetric boundary condition, providing incoming fast particles.[20]

Thus if Boltzmann's statistical approach is correct in general, the source-priority view seems appropriate for the simple particle models we've been considering. And conversely, I think, the plausibility of the source-priority view in these simple cases provides support for the one-asymmetry approach in general. But the distinction between source-priority and absorber-priority is in a sense more important than the question as to which view is right. These two views provide very different accounts of the origins of many familiar 'thermodynamic' processes. If we don't recognise that they are distinct alternatives, we'll be unable to make sense of the resulting disagreements about the origins of time-asymmetric behaviour.

One more point about the particle case, before we return to radiation. Because Boltzmann's one-asymmetry approach traces the observed asymmetry of thermodynamics entirely to the low entropy initial condition, it doesn't provide a statistical argument against the existence of a similar *final* condition. On the contrary: within the abstract framework of Boltzmann's approach, the issue as to whether there is such a future low entropy boundary condition is effectively the same as the question whether the time-symmetric Boltzmann measure is reliable towards the future, in the way in which it turns out not to be reliable towards the past – so we certainly can't appeal to the Boltzmann measure itself to settle the issue! The question of the future boundary is therefore empirical, and currently open, apparently (more on this below). We may have no current evidence for it, but we can't appeal to the Boltzmann measure to exclude it.

3.3 Small buoys in a tub: the radiation case

Suppose that we accept the source-priority view for frictional damping of particle motion. Do the same considerations apply to the case of radiation? I want to argue that they should, and that this is why we should object to Zeh's view, which is an absorber-priority view. However, I also want to call attention to an apparent lacuna in the radiation case – a reason for thinking that the extension is more problematic than it seems at first sight. I close with some comments on the significance of this lacuna.

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[20] Asymmetric dynamics might exclude the corresponding future boundary condition, but this is not the same as guaranteeing the past boundary condition – which therefore needs to be separately postulated.

First, let's imagine a radiation analogue of the two-tub damper–anti-damper apparatus. We could use the very same apparatus, actually, by considering the shock waves created in the gas in a tub by a sufficiently energetic incoming particle. But instead, let's consider something more familiar, based on the main example we've been using throughout the paper, that of waves in water.

Imagine two large pools or tubs of water, with reflecting edges. Suppose that floating in each tub are some small buoys. What happens if a new buoy is thrown into one of the tubs? Intuitively, we know what to expect. The impact creates an outgoing ripple, which disperses the kinetic energy brought in by the buoy. However, the opposite thing can happen, too. An incoming wave can give sufficient energy to one of the buoys to throw it out of the tub.

Suppose that the two tubs are separated by a potential barrier. Particularly energetic buoys can make it over this barrier, and hence from one tub to the other, thus providing a mechanism for exchange of energy between the two. If we consider the equilibrium case, in which this exchange of energy is on average the same in both directions, it seems reasonable to expect the same kind of symmetry as we found in the particle case. So the cases in which an incoming buoy arrives to create an outgoing wave will be matched by cases in which an incoming wave arrives to propel a buoy over the potential barrier.[21]

In the particle case, we used this equilibrium model to suggest an explanation of the observed asymmetry of damping over anti-damping. The proposal was that the asymmetry results from the fact that the symmetrically disposed absorbers are placed in an asymmetric environment, in which there are sources but not anti-sources of fast particles – stars but not anti-stars, for example. It is attractive to apply the same strategy to the case of radiation: to suggest that the observed imbalance between outgoing and incoming radiation stems from the fact that the wave media with which we are familiar (such as water surfaces) are close to sources and not anti-sources of concentrated energy. Put a source of small buoys next to your tub, and you get outgoing ripples. Place an anti-source next to it – i.e., impose a boundary condition requiring outgoing buoys – and you get incoming ripples.

As in the particle case, it is difficult to reject this conclusion without rejecting the symmetric description of the equilibrium case. For the latter is associated with a measure from which the two asymmetric cases simply result by conditionalisation. Or at any rate that's how it is supposed to work, and in the particle case, I think we know pretty much how to make it work. The relevant measure is the Boltzmann measure, and the device of deriving the observed asymmetry by conditionalising this symmetric measure on an asymmetric low entropy past boundary condition is the essence of the statistical approach to the explanation of the thermodynamic asymmetry.

So to make it work properly in the radiation case, we need an analogue of

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[21] As in the particle case, this is not the only possible behaviour. In particular, just as it is possible for an incoming fast particle to be accelerated rather than damped by subsequent collisions, so it is possible for an incoming buoy to receive extra energy from an incoming wave. As in the particle case, however, there would be a risk of instability if this were the norm.

the Boltzmann measure ... and this is where we run into a problem, of course. The problem is well-known, at least in the EM case, where it is the source of the famous 'UV catastrophe'. Boltzmann's recipe for the distribution of energy over the frequencies simply doesn't work for radiation – indeed, it leads to the absurd result that the total energy of any sample of radiation is infinite.

This is the lacuna I referred to above. There are two possible views of its significance. One is that we need to add some asymmetry to account for the asymmetry of wave phenomena. The other is to acknowledge that classical wave theories are only an ideal approximation, inapplicable at the microscopic level, and to propose that the problem is an artifact of the idealisation, likely to go away when the idealisation is replaced by something more accurate.

The second view seems clearly right for the mechanical cases, such as water waves. The upshot is that there isn't a rigorous analogue of the particle case argument for source-priority, because that argument relied on the treatment of the equilibrium case. But this is not specifically a problem about asymmetry, but about the equilibrium case, from which we want to derive asymmetry by conditionalisation. So it is an aspect of a much broader problems about the application of statistical mechanical ideas to the treatment of the thermodynamic behaviour of radiating systems.

Still, in the real world, at least in the mechanical cases, the right attitude seems to be that since the wave treatment is only an idealisation, everything can in principle be grounded at the particle level. Thus the symmetry of radiation in the equilibrium case – in so far as the radiative description is applicable at all – will follow from the corresponding result at the particle level. Similarly, the case for source-priority will go through, just as before.

Is there any additional difficulty in the EM case? Here, too, we know that the source-priority view is right at the quantum level that underlies EM phenomena – and even if we didn't know this, the lack of a rigorous argument *for* source-priority would not amount to an argument *against* source-priority. So in the EM case as in the mechanical cases, there seems no reason to think that there is any intrinsic time-asymmetry in the thermodynamic behaviour of absorbers, of the kind required by the absorber-priority view. While there is an interesting lacuna in the application of statistical ideas to the radiation case, it doesn't provide what Zeh needs: an argument for absorber-priority.

The issue of the limits of the radiation treatment raises some further issues, including another suggestion about the fundamental asymmetry of EM radiation, which Frisch attributes approvingly to Einstein. I'll return to these issues in §4. Before leaving Zeh's treatment of the radiative asymmetry, however, I want to consider his suggestion that we have empirical evidence for the Sommerfeld radiation condition, at least in the EM case.

3.4 What can we infer from the darkness of the night sky?

After arguing that in the "laboratory situation the radiation arrow is a simple consequence from the thermodynamical arrow characterizing absorbers", Zeh goes on to consider the cosmological case:

> Do similar arguments also apply to situations outside absorbing boundaries, in particular in astronomy? The night sky does in fact appear black, representing a condition [$F_{in} \approx 0$], although the present universe is transparent to visible light. Can the darkness of the night sky then be understood in a realistic cosmological model? (Zeh 2005, 23).

This seems to be an argument that we have empirical grounds for ruling out the possibility that there might be significant amounts of advanced radiation, converging on astronomical sources in the future. If the argument is a good one, then even someone who disagrees with Zeh about whether $F_{in} \approx 0$ can *explain* the absence of such sources, might agree that we can have present evidence that there are no such sources. To put it another way, it might be agreed that the observation provides grounds for thinking that the observed asymmetry will persist in the future, however the asymmetry itself is to be explained.

But is the argument a good one? It seems to me that it is vulnerable to an objection I have raised elsewhere to a proposal by Gell-Mann and Hartle, about the empirical consequences of assuming that the universe produces stars and galaxies at both ends, in the way first proposed by Thomas Gold (1962). Gell-Mann and Hartle argue as follows:

> Consider the radiation emitted from a particular star in the present epoch. If the universe is transparent, it is likely to reach the final epoch without being absorbed or scattered. There it may either be absorbed in the stars or proceed past them toward the final singularity. If a significant fraction of the radiation proceeds past, then by time-symmetry we should expect a corresponding amount of radiation to have been emitted from the big bang. Observation of the brightness of the night sky could therefore constrain the possibility of a final boundary condition time-symmetrically related to the initial one. (1994, 326–327)

In other words, Gell-Mann and Hartle's argument is that the symmetric 'Gold universe' implies that there should be more radiation observable in the night sky than we actually see. As well as the radiation produced by the stars of our own epoch, there should be radiation which – in the reverse time sense – is left over from the stars of the reverse epoch, at the far end of the universe. As Davies and Twamley (1993) put it, "by symmetry this intense starlight background should also be present at our epoch . . . , a difficulty reminiscent of Olbers' paradox."

As I argued in (Price 1995) and (Price 1996, Ch. 4), however, it is very unclear that if there were additional radiation of this kind, we could actually detect it. After all, the radiation concerned is neatly arranged to converge on its future sources, not on our eyes or instruments. For example, imagine that there is a reverse galaxy in the distant future (in our time sense) in direction $+x$. In its own time sense, it is emitting radiation toward a distant point in direction $-x$ (see Figure 6).[22]

Suppose we stand at the origin and look towards $-x$. Can we see the light which in our time sense is traveling *from* $-x$, *towards* $+x$? No, because if we

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[22] Reproduced from (Price 1996, 110).

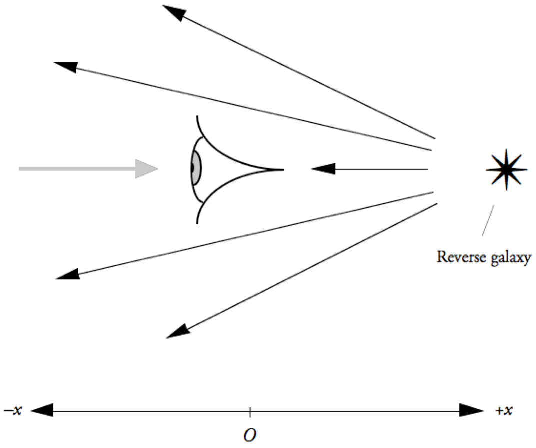

Figure 6: Could we observe radiation converging on future sources?

are standing at the origin (at the relevant time) then the light emitted from the reverse galaxy falls on us, and never reaches (what we think of as) the past sky. So when we look toward $-x$, looking for the radiation converging on the reverse galaxy at $+x$, then the relevant part of the radiation doesn't come from the sky in the direction $-x$ at all; it comes from the surfaces at the origin which face $+x$ – that is, from the backs of our own heads. So the advanced radiation associated with future sources is not necessarily detectable by normal means.

The issue of the observability of $F_{in} \approx 0$ is a different issue from whether such an initial condition could *explain* the absence of macroscopic sources of converging radiation. Observability would not imply explanatory priority. But observability itself seems doubtful, for the reasons just given. Once again, this seems to be the analogue of a familiar point in the particle case. It is often noted that once a deterministic gas has reached equilibrium, its microstate (or, if necessary, the combined microstate of the gas plus its environment), contains a kind of unreadable record of its low entropy past: play the film backwards, after all, and we recover that original state. Obviously, the same applies in the other direction, too – which means that we can't tell by looking whether the entropy of a system will eventually decrease, because we can't exclude the possibility of 'hidden' microscopic order of this kind.[23]

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[23] Indeed, just as in the radiation case, it would turn out that 'anti-thermodynamic' behaviour would defeat ordinary means of detecting the presence of future low-entropy boundary condition. If we put up a screen to detect the outgoing fast particles associated with the kind of anti-source of fast particles considered in the two-tub cases, for example, the particles will emerge from the back of the screen.

4 The asymmetry of large sources

Let's assume that the two-tub argument works for radiation, as well as for particles; and that the moral of the argument is applicable in the phenomena we observe in the real world, and not merely to a few idealised examples. Then the lesson of the argument is that the observed asymmetry of radiation depends on an asymmetry in the environment in which wave media are embedded: the asymmetry is that the environment supplies large 'kicks' in one time-sense but not in the other. In our sense, it adds large amounts of energy to the media ('all in one go' – in a coherent way, in other words) much more frequently than it subtracts or removes large amounts of energy, in a similar coherent fashion.

This could only produce the observed asymmetry if there were some sense in which addition of energy was associated with outgoing or retarded waves, and subtraction with incoming or advanced waves. In the former case, this doesn't seem like a controversial assumption: as I put it at the beginning of the paper, when energy is added to a system of the kind we are discussing, it radiates away in the form of a concentric outgoing wave.

In earlier work, I have defended the view that the time-inverse assumption is also true, of the world as we find it: energy is removed from a system of the kind we are discussing, it radiates inwards to the point of absorption, in the form of a concentric advanced wave. The fact that we don't seem to observe these incoming ripples is a consequence of the fact that in most real situations, there are huge numbers incoming ripples, all superposed to form the patterns we actually see. In other words, the observed asymmetry is a consequence of the FLOMSI asymmetry noted earlier: a contrast between a *few large outgoing* waves, and *many small incoming* waves. That contrast, in turn, is explained by the thermodynamic nature of the environment, which supplies the large 'kicks' or additions of energy, but not large 'anti-kicks', or subtractions of energy. (And *that* contrast, in turn, is a product of the low entropy past – and the prevalence, still at our era, of large low-entropy sources.)

On this view, then, the observed asymmetry reflects no intrinsic or microscopic asymmetry in radiative processes, but merely an asymmetry in the 'clumping' or arrangement of emitters and absorbers of radiation: large coherent emitters, but no large coherent absorbers. It seems to me that the two-tub argument provides strong support for this view.

Two significant objections to this account have been raised in the literature, particularly by Zeh and Frisch. I'll consider Zeh's objection first, since it is at least partly answered by Frisch, before he presents his own objection.

4.1 The retarded shadows objection

Frisch himself notes the attractions of the kind of view I have proposed:

> The strict identification of absorption processes with advanced waves and of emissions with retarded waves has some intuitive appeal. Given a specific temporal orientation, the retarded solution to the wave equation describes a disturbance that originates at the source at a time t_0 and

> travels outwards for times $t > t_0$, while the advanced solution describes a disturbance that converges into the source at times $t < t_0$ and 'disappears' at the source at time t_0. Intuitively, the former solution seems to characterize an emission process and the latter solution an absorption process. (2000, 396)

"However," Frisch continues, "an absorber can be associated with a retarded field as well, as Zeh . . . argues."

Zeh's most recent formulation of the objection in question is as follows:

> Some authors take the view that retarded waves describe emission, advanced ones absorption. However, this claim ignores the fact that, for example, moving absorbers give rise to *retarded* shadows, that is, retarded waves which interfere destructively with incoming ones. In spite of the retardation, energy may thus flow from the electromagnetic field into an antenna. (Zeh, 2005, 16)

Frisch goes on to note that Zeh's point applies equally in reverse, but also that it may miss the point, as an objection to my view:

> Similarly, the emission of energy can be associated with a purely advanced field. Of course one could have represented the absorption process in terms of advanced fields. The point here is that this representation is not the only one possible; both emission and absorption processes can be represented in terms of either retarded or advanced waves by including appropriate free fields.
>
> However that may be, presumably it would be enough for Price's purposes to establish the weaker claim that if an absorber is centered on a coherent wave front, then it can be associated with an advanced field; and that claim is true. (2000, 396–397)

I would put the point slightly differently. The crucial requirement is the following symmetry between the case of emission and the case of absorption: that *in whatever sense* it is true that what we observe is that emissions are associated with outgoing concentric waves,[24] then in *that same sense* what we observe is that absorptions are associated with incoming concentric waves; *except* for any differences associated with the differences in the relative sizes and numerosity of typical emitters and absorbers. If this requirement is met then there is no observed asymmetry of radiation, other than of the number and size of sources – and that's my basic claim. (Someone who agrees that the requirement is met, but wants to claim that there is some further asymmetry of radiation, can't be talking about the *observed* asymmetry.) Nevertheless, I think that Frisch is right: in this respect, Zeh's point doesn't touch my main claim.[25]

It may seem that this reply misses the main point of Zeh's objection. After all, think of what happens when we walk in the open air on a sunny day. Our

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[24] Note that if there weren't some sense in which this were true, it couldn't be maintained that we observe any asymmetry between outgoing and incoming waves.

[25] However, Frisch is also correct in pointing out that the crucial point can be established much more directly than I appreciated – it doesn't depend on a detour through a reinterpretation of the Wheeler-Feynman Absorber Theory.

body becomes a moving absorber, blocking direct sunlight before it reaches other surfaces. But the resulting shadow is produced later than the absorption – not much later, to be sure, in typical circumstances, but later, nonetheless. Absorption thus has 'retarded' consequences – and the same is true in non-electromagnetic cases as well, of course.

However, there's an easy reply to this form of the objection. Absorbers create shadows when they block incoming *particles,* too, but this doesn't show that particle absorption is anything other than the temporal inverse of particle emission. There's more to be said on this matter, but this reply is sufficient for present purposes – I'll resist the temptation to explore the temporal foundations of 'umbrology'.[26]

4.2 The no pure absorbers objection

My proposal is that at the microscopic level, absorptions are the temporal inverses of emissions – at that level, if I am right, there is no asymmetry of radiation. The observed asymmetry stems from the fact that in the case of emitters but not the case of absorbers, it is common for large numbers of small sources to be grouped in a coherent way, so as to produce the coherent macroscopic outgoing waves, familiar in our experience.

Frisch raises a second objection to this kind of view:

> But are absorptions really temporal inverses of emissions? For this to be true, it has to be the case that we can represent absorptions in terms of fully advanced fields. A simple model of a microscopic absorption process is the absorption of radiation by a harmonically bound charge .... In response to an incident radiation field the charge begins to accelerate and oscillate. The field has to do work against the binding force and, thus, part of the energy of the incident field is removed from the field and converted into mechanical motion of the oscillating charge. Since the charge accelerates, it not only absorbs energy, but also radiates off energy. Therefore, the effect of a microscopic absorber is partly to absorb energy and partly to re-radiate and scatter the incident field. If such an absorption process is to be the temporal inverse of an emission process, then it has to be possible to represent any contribution to the total field due to the presence of the bound charge in terms of a fully advanced field. However, this is in general not possible. Since any microscopic absorber re-radiates energy, the field associated with the absorber has a component along the forward light cone of the charge and, therefore, cannot be represented as a fully advanced field. There are emissions without absorptions, but no absorptions without re-emissions. (2000, 398)

There are some puzzles about emission and absorption in classical EM in any case, due especially to the problem of radiative damping. (See Arntzenius

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[26] If we were to embark on that project, a good place to start would be the two-tub models. It would be easy to modify our examples so that incoming fast particles created shadows; in which case symmetry would again show that in the equilibrium case, outgoing fast particles have advanced 'anti-shadows'. Again, then, the asymmetry we observe in the real world comes from the low entropy environment.

1994, for an insightful discussion of this point.) So let's set EM aside for the moment, and consider the analogous objection in the case of non-EM radiation, such as water waves.[27]

In such mechanical cases, the symmetry of the underlying mechanics immediately implies that whatever *actually* happens in the case of emissions, is physically *possible,* in reverse, in the case of absorptions. So to whatever extent we actually have pure emissions, the theory also allows pure absorptions. Moreover, the two-tub argument suggests that in the equilibrium case, pure absorptions will occur as frequently as pure emissions – while in real life, the low entropy environment produces a lot more of the latter than of the former. Again, then, there is nothing here to trouble my diagnosis of the observed asymmetry. The pure emissions (or approximations to pure emissions – presumably no real case has an incoming field of exactly zero) occur when the low entropy environment produces a large, localised, coherent oscillation, which then dissipates through the medium in question in the former of a outgoing wave. Of course, there are also many less noticeable emissions, of the kind which, as the objection itself points out, will be associated with normal microscopic absorptions.[28] In some cases, if the mechanical motion of the oscillators in question cooperates, the emission will be of slightly greater energy than is provided from the incoming wave. In other cases, it will be slightly less. But we have no reason to postulate any overall asymmetry at this level.

The observable asymmetry arises because we don't find cases in which the mechanical motions conspire to remove a large amount of energy suddenly from the field, with little residue. And here, I've urged, the asymmetry is explicable in terms of the thermodynamic properties of the environment – the fact that it provides large 'kicks' from the past, but no large 'anti-kicks' from the future.

The issue of asymmetry aside, it is hard to see how the observation that absorbers typically re-radiate at least part of the incoming energy, could be an objection to the suggestion that we should associate retarded radiation with emitters and advanced radiation with absorbers. For this suggestion obviously *entails* that if a source does act both as an absorber and as a (re-)emitter, it should be associated with both an incoming and an outgoing component of the field. It is hardly an objection, then, to point out that this is actually the case.

4.3 The no elementary absorbers objection

Frisch endorses a different view, that he attributes to Einstein (and Popper). In an illuminating discussion of Einstein's views, Frisch first notes that there are passages in Einstein which do indeed support the usual interpretation of his (i.e., Einstein's) views on this matter.

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[27] In his (2000) paper, Frisch himself distinguishes at one point between the EM and non-EM cases, and shouldn't be taken to have endorsed this extension of the objection. The Frisch of (Frisch 2005b) does not endorse even the EM case.

[28] It can't be that *all* emissions are pure emissions, if absorptions provide a common class of emissions that are not pure.

> The elementary processes responsible for electromagnetic radiation are symmetric, Einstein seems to have believed, and there is a strong suggestion that the source of the appearance of irreversibility is related to the source of the thermodynamic asymmetry. (T/s of 2005b, Ch 5, p. 10)

However, Frisch continues, there are other passages that suggest that Einstein held a different view:

> The trouble is that in very same year Einstein and in the very same journal also had this to say about the processes governing wave phenomena:
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> > The basic property of wave theory, which leads to these problems [in trying to account for what we today think of as quantum phenomena], appears to be the following. While in kinetic molecular theory the inverse process exists for every process involving only a few elementary particles, e.g. for every elementary collision, according to wave theory this is not the case for elementary radiation processes. An oscillating ion produces a diverging spherical wave, according to the standard theory. The reverse process does not exist as elementary process. A converging spherical wave is mathematically possible; but in order to realize such a wave approximately a tremendous number of elementary objects are needed. *The elementary process of the emission of light is, thus, not reversible.* In this respect, I believe, our wave theory goes wrong. (Einstein 1909, 819, my italics)
>
> Einstein goes on to discuss the advantages of a quantum treatment of radiation that allows for the entire energy radiated by a source to be absorbed in a single "elementary process," instead of being dispersed as in the wave theory.
>
> Einstein here seems to contradict directly what he had said earlier. In the letter with Ritz he expressed the view that that the irreversibility of radiation processes "is exclusively based on reasons of probability" while here he says that the elementary radiation processes are irreversible. Unlike in the case of kinetic theory and the second law of thermodynamics, even the fundamental micro-processes of radiation are asymmetric in the wave theory of light, according to Einstein. To be sure, Einstein thinks that this aspect of the classical theory of radiation is problematic. But this does not affect the point that Einstein here seems to assert what he seems to deny elsewhere: that within classical electrodynamics elementary radiation processes are asymmetric. (T/s of 2005b, Ch 5, p. 10)

I want to make several brief comments about this argument.

a. First, it seems to me that classical EM gives us no reason to think that there *are* elementary processes, as Einstein (and Frisch) seem to require. Of course, one might have other reasons – proto-quantum reasons, in effect – for thinking that there must be elementary processes. But this doesn't mean that the classical theory contains or requires them, or can even accommodate them. In classical EM, there seems to be no such thing as a smallest possible emission of radiation.[29]

b. Even if there were elementary processes, Einstein has given us no non-question-begging reason for thinking that such processes couldn't occur in both temporal directions. When Popper tries to offer such a reason, he falls into a double standard fallacy. Absent some prior temporal asymmetry, after all, what reason do we have for denying that a source in the future is just as good a way of 'producing' a converging wave as a source in the past is of producing a diverging wave? (And conversely, if absorption could occur without elementary processes, wouldn't this undermine any case for thinking that emission required elementary processes?)

c. It is true that if there were elementary processes, there might be consistency problems in allowing them in both temporal directions. However, these problems would apparently be resolved if we allowed both $F_{in} \neq 0$ and $F_{out} \neq 0$. And in any case, the threat of consistency problems couldn't provide a *general* objection to time-symmetric elementary processes, because in some cases, geometry solves the problem: consider a single charge at the centre of a reflecting sphere, acting as both emitter and absorber.

d. Even if there were time-asymmetric elementary processes, it wouldn't be obvious that our world required elementary emitters rather than elementary absorbers. Unless we knew in advance at what 'level' the elementary process lay, the possibility would remain open that all the various emissions we actually observed were 'in fact' sums of many much smaller elementary *absorptions.*

e. Finally, and most importantly, it needs to be pointed out that even if classical EM theory were asymmetric in this way – with elementary emitters but no elementary absorbers – this would not explain the *observed* asymmetry of EM radiation. The classical theory has been superseded as an explanation of what we actually *observe,* and this asymmetric element (if any) has not survived the transition.[30] Even if classical EM theory were asymmetric, in other words, this would not be relevant to the observed asymmetry of EM radiation, in the real (quantum) world.

4.4 Two views of the role of the low entropy past

To finish, I want to distinguish my view of the role of the low entropy boundary condition from another recent suggestion in the literature. In a recent paper, Jill North argues that the asymmetry of radiation is due to the low entropy past. While I agree with her conclusion, I think her argument reveals a subtle

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[29] Perhaps Einstein's thinking is really this: there are reasons for thinking that there are elementary processes, in the transfer of energy in electromagnetic phenomena; if so, then one reason for thinking that classical radiation theory needs to be modified is that if there were elementary processes, in the classical theory, they would need to be time-asymmetric.

[30] See Atkinson 2005, for an account of these matters in QED.

misunderstanding of the nature of the observed asymmetry. In other words, she has the right *explanans* but the wrong *explanandum,* in my view.

On North's account, the asymmetry of radiation is a product of the thermal disequilibrium – the huge difference in temperature between stars and galaxies on the one hand, and empty space and the cold matter it contains, on the other. This thermal disequilibrium came into being early in the history of the universe, a product of gravitation and a particular homogeneous initial distribution of matter, and persisted until the present day.

> The temporally symmetric laws say that both advanced and retarded radiation could be emitted. However, given the universe's thermal disequilibrium, the charges are overwhelmingly likely to radiate towards the future, as part of the overwhelmingly likely progression towards equilibrium in that temporal direction. They are overwhelmingly unlikely to radiate towards the past because the universe was at thermal equilibrium in that direction. Note that on this view the retarded nature of radiation is statistical: advanced radiation is not prohibited but given extremely low probability. (North 2003, 1095)

This proposed explanation of the asymmetry is unsatisfactory in two respects, in my view. First, the two parts of the explanation – 'future-wards' and 'past-wards' – seem in tension with one another. We are told that progression towards thermal equilibrium towards the future makes radiation in that direction 'overwhelmingly likely'. On the other hand, we are told that charges are 'unlikely' to radiate towards the past, *because* the universe goes towards thermal equilibrium in that direction!

These claims are simply contradictory, and it is the second one which is at fault. In statistical terms alone, the fact that the universe is in thermal equilibrium early in its history does make it likely that hot objects lose heat in that direction – this is simply the well-known time-symmetry of Boltzmann's statistical reasoning in thermodynamics (which he himself first appreciated in his reflections on Loschmidt's famous reversibility objections). The reason it doesn't actually happen like that is that towards the past, statistical reasoning is 'trumped' by the low entropy boundary condition – by the so-called 'Past Hypothesis'. So what North should have said is that the particular history associated with the Past Hypothesis ensures that charges don't radiate 'towards the past', despite the fact that they are otherwise likely to do so. The Past Hypothesis thus plays a much more direct role here than North appreciates, in my view.

However, this is not what I had in mind when I said that North had the wrong *explanandum.* The second problem emerges most clearly when North extends her proposal to a quantum treatment of electromagnetic radiation:

> This explanation should carry over to the quantum realm. Indeed, we must turn to quantum theory. For there is no adequate classical account of an equilibrium state between matter and fields. Hence there is no classical description of the initial condition of the universe. Quantum

> electrodynamics, however, treats matter and field particles in the same way and can describe an equilibrium between them. ...
>
> In the quantum picture, the early universe comprised photons in thermal equilibrium with material particles—there were, on average, just as many photons emitted as absorbed. As material particles clumped up and the universe expanded, the universe moved to a state containing charges in excited states (in the hot masses) and photons less densely distributed throughout space. These factors result in a high probability of emission of photons towards the future by accelerating charges, and a low probability of emission of photons to the past. The probability for retarded radiation will remain high until thermal equilibrium is reestablished. (2003, 1096)

On North's view, then, the quantum equivalent of the asymmetry of radiation is an imbalance between emissions and absorptions of photons: in the ordinary time sense, in fact, a lot more of the former than the latter.

The problem with this characterisation of the asymmetry is that it implies that there is *no* asymmetry of radiation, in many ordinary situations – situations in which it seems, intuitively, that radiation has its 'normal' retarded character. Consider what happens when we turn on an electric light inside a closed laboratory, for example. To a good approximation, *all* the photons emitted by the light bulb are absorbed by the walls. So there is no significant net retardation, in North's sense, in other words, because the 'retarded' photons from the perspective of the light are 'advanced' photons from the perspective of electrons in the walls.

Readers who have followed my own proposal will realise that in one respect, I'm very sympathetic to this conclusion. In my view, there's no *microscopic* asymmetry of radiation in such a case, because retarded radiation diverging from emitters can equally well be characterised as advanced radiation converging on absorbers. Nevertheless, I've argued that this microscopic symmetry is compatible with a macroscopic asymmetry – as I put it earlier, an asymmetry which is a product of the fact that the low entropy environment provides big 'kicks', or emitters, but no big 'anti-kicks', or absorbers. Because there's no place for this macroscopic asymmetry in a view which simply pays attention to the net imbalance, if any, between emissions and absorptions, it seems to me that North is working with the wrong *explanandum.*

4.5 Summary: the asymmetry of big kicks

Thus I agree with North that at the microscopic level, absorption of radiation is the time-inverse of emission of radiation. Indeed, I take this to be true not only of the quantum replacement for classical EM radiation, but also of wave phenomena in general, EM and non-EM, in so far as wave descriptions are applicable to the real world.

In all such cases, apparently, it is possible that we may find a net imbalance between the amount of energy emitted and the amount of energy absorbed, in particular circumstances. Thus there may be temporal asymmetries of this kind, local or global, to varying degrees and with either temporal orientation.[31]

But asymmetries of this kind do not constitute the arrow of radiation, as usually understood. To see why, we need look no further than Popper's famous example. The energy supplied to the pond's surface by the pebble is fully absorbed, very soon, by the molecules at the edges of the pond. So no net imbalance between emission and absorption of energy, in other words – and yet the outgoing, stubbornly asymmetric ripples are there for all to see.

To characterise the time-asymmetry of these ripples, as of their outgoing cousins in other media, we need to appeal to the *scale* of some the sources involved. In the pond case, there are certainly absorbers, and what we observe is precisely what we ought to observe, if those absorbers are centered on incoming waves. But each of these tiny waves is individually invisible, observable only in its contribution to the sum. There are microscopic emitters, too, of which the same is true. But in addition, sometimes, thanks to the low entropy past, there are also macroscopic emitters, such as incoming pebbles. These objects disturb so many water molecules, in such a coherent fashion, that they produce the observable macroscopic ripples we actually see. This doesn't happen in reverse, at least in our region of the universe, because there's no corresponding low entropy future boundary (or because it is so far away that its effects are not significant here, at least in ways that we know how to look for).

At the beginning of the paper, I noted that wave phenomena arise in systems in which couplings provide a way of carrying the energy stored in adjacent harmonic oscillators from place to place. I then observed that when energy is added to such a system at a point, it radiates away as a concentric outgoing wave. One way to characterise the view just outlined is to say that it combines two claims. First, it maintains that the latter observation is true in both temporal senses – whichever we choose to regard as the positive direction of time, addition of energy (in that time sense) is associated with outgoing waves (in that time sense). Second, it notes that in one direction but not the other (at least in our experience), energy is often added in big, relatively coherent kicks. That fact alone is responsible for the observed asymmetry of radiation, on this view. Finally, its relation to the thermodynamic asymmetry is not particularly mysterious: the low entropy conditions persisting in our region of the universe provide many suitable boots, of one sort or another.

Bibliography

Arntzenius, F. 1994. 'The Classical Failure to Account for Electromagnetic Arrows of Time,' in T. Horowitz and A. I. Janis, eds., *Scientific Failure,* Boston: Roman & Littlefield Publishers, 29–48.

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[31] Arntzenius (1994) provides a nice discussion of the circumstances in which, on the cosmological scale, there will be a net imbalance between emission and absorption of photons. LIke North, however, he seems mistakenly to believe that what he is discussing is the quantum analogue of the classical problem of the arrow of radiation.

Atkinson, D. 2005: 'Does Quantum Electrodynamics Have an Arrow of Time?', typescript.

Beck, A. (Trans.) 1989. *The Collected Papers of Albert Einstein. Volume 2, The Swiss Years: Writings 1900–1909,* Princeton, N. J.: Princeton University Press.

Davies, P. C. W. 1974: *The Physics of Time Asymmetry,* London: Surrey University Press.

Davies, P. C. W. and Twamley, J. 1993. 'Time-symmetric Cosmology and the Opacity of the Future Light Cone,' *Classical and Quantum Gravity,* 10, 931.

Einstein, A. 1909. 'Über die Entwicklung unserer Anschauung über das Wesen und die Konstitution der Strahlung,' ('On the Development of Our Views Concerning the Nature and Constitution of Radiation') *Physikalische Zeitschrift,* 10, 817–25; English translation in Beck(1989), 379–394.

Einstein, A. and Ritz, W. 1909. 'Zum gegenwrtigen Stand des Strahlungsproblems' ('On the Present Status of the Radiation Problem'), *Physikalische Zeitschrift,* 10, 323–324; English translation in Beck(1989), 376.

Frisch, M. 2000. '(Dis-)Solving the Puzzle of the Arrow of Radiation,' *British Journal for the Philosophy of Science,* 51, 381–410.

———2005a. 'A Tale of Two Arrows,' typescript.

———2005b. *Inconsistency, Asymmetry and Non-locality: Philosophical Issues in Classical Electrodynamics.* New York: Oxford University Press.

Gell-Mann, M. and Hartle, J. 1994. 'Time Symmetry and Asymmetry in Quantum Mechanics and Quantum Cosmology,' in Halliwell, J., Perez-Mercader, J. and Zurek, W. (eds.), *Physical Origins of Time Asymmetry,* Cambridge: Cambridge University Press, 311–345.

Gold, T. 1962. 'The Arrow of Time', *American Journal of Physics,* 30, 403–410.

———(ed.) 1967. *The Nature of Time,* Ithaca: Cornell University Press.

North, J. 2003. 'Understanding the Time-Asymmetry of Radiation,' *Philosophy of Science,* 70, 1086–1097.

Popper, K. 1956. 'The Arrow of Time,' *Nature,* 177, 538.

Price, H. 1995. 'Cosmology, Time's Arrow and That Old Double Standard,' in Savitt, S. (ed.), *Time's Arrows Today,* Cambridge University Press, 66–94. [Available on-line at http://xxx.lanl.gov/abs/gr-qc/9310022.]

———1996. *Time's Arrow and Archimedes' Point*, New York: Oxford University Press.

———2006. 'Causal Perspectivalism,' forthcoming in H. Price and R. Corry, eds., *Causation, Physics and the Constitution of Reality: Russell's Republic Revisited,* Oxford University Press. [Available on-line at http://www.usyd.edu.au/time/price/preprints/CausalPerspectivalism.pdf.]

Wheeler, J. A. and Feynman, R. P. 1945. 'Interaction with the Absorber as the Mechanism of Radiation,' *Reviews of Modern Physics,* 17, 157–181.

Zeh, H. D. 2001. *The Physical Basis of the Direction of Time,* 4th edn., Berlin: Springer-Verlag.

———2005. 'The Time Arrow of Radiation', draft of Chapter 2 of *The Physical Basis of the Direction of Time,* 5th edn. Accessed online on 22 February 2005, at http://www.time-direction.de/.