Once and for all: the curious role of probability in the Past Hypothesis*

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Abstract

The Past Hypothesis defended by David Wallace in his 2011 account of macroscopic irreversibility is technically distinct from, but in the same spirit as, that of David Albert in his 2000 book *Time and Chance*. I am concerned in this essay with the role of objective probability in both accounts, which I find obscure. Most of the analysis will be devoted to the classical treatments by both authors, but a final section will question whether Wallace's quantum version involving unitary dynamics removes this obscurity.

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1 Introduction

In his remarkable recent work on the foundations of statistical mechanics, David Wallace has urged that there is more to the theory than just recovering thermodynamics, and that lessons need to be learnt from the quantitative way practitioners of statistical mechanics go about accounting for the spontaneous process of equilibration in isolated systems.[1] I endorse these claims, and think Wallace's detailed 2011 treatment of irreversibility, of the subtle business of accounting for the thermodynamic arrow of time in statistical mechanics, deserves careful attention. Although clearly influenced by David Albert's formulation of the *Past Hypothesis* in his 2000 monograph *Time and Chance*, Wallace goes beyond Albert's account, not just by providing a more detailed mechanism for irreversibility, but by bringing *unitary* quantum mechanics into the picture in parallel with classical mechanics. In doing so, he questions the widespread notion that the relevant conceptual problems appear in the same guise in both classical and quantum statistical mechanics.

In this paper, I will concentrate mostly on the classical accounts of irreversibility offered by the two Davids. I will try to articulate my misgivings about both versions of the past hypothesis, my main qualms having to do with the role probability plays in these arguments. At the end of the paper I raise the question as to how much of Wallace's classical analysis survives in the light of his quantum analysis. To this end, I examine the treatment of the "irreversible" thermalization process associated with a quantum field theoretic version of Boltzmann's H-theorem.

2 Preliminaries

In order to set the stage, I would first like to recall some of the basic conceptual issues that arise when dealing with the arrow of time in a theory

[1] See Wallace (2015a).

with time-symmetric dynamics, as I see them. I start with the following statements made by David Wallace and Jos Uffink respectively:

> "It is virtually tautologous that if microscopic physics has no time asymmetry but the emergent macroscopic dynamics does have a time asymmetry, that time asymmetry must be due to an asymmetry in the initial conditions of the universe."[2]

> ". . . a clear and commonly accepted answer on the question how to explain irreversible phenomena in statistical mechanics has not been reached."[3]

One might quibble with the suggestion that initial conditions can be "asymmetric", but it is fairly clear what Wallace means in the first statement. Given that the fundamental micro-dynamical laws are time reversal invariant, it should be obvious that the time asymmetry ubiquitously displayed in the behaviour of macroscopic systems cannot be explained in terms of these laws alone. The explanation – if that is the right word – must incorporate something like boundary/initial conditions of the system in question, and I will take this simple point for granted from now on. It might seem odd then that according to Uffink, the explanation enjoys no consensus in the literature. What can the debate be about? I will not attempt any sociological analysis of the matter, but raise a number of issues that come to mind.

First, there is the question as to whether the initial conditions of the universe (with or without inflation) are disagreeably "fine-tuned", thus themselves calling for an explanation. A further question is whether such an explanation is possible in principle. A positive answer to this last question could only make sense in the context of a multiverse scenario, and this involves more speculation than I feel comfortable with in the present essay.[4]

Second, it may seem arbitrary, given our fundamental understanding of determinism in physics, that the conditions specified in the putative explanation of entropic behaviour are the *initial* ones. If determinism is, at root, related to the existence of a Cauchy "initial" value problem, the Cauchy data can in principle lie on any space-like hypersurface, even one to the "future"

[2] Wallace (2011).

[3] Uffink (2013).

[4] For a critique of the notion that cosmological inflation can explain the low-entropy state of the early universe, see Carroll (2010), chapter 14. The question as to whether initial conditions for the universe need explanation at all is addressed by Callender (2004).

of the apparently irreversible behaviour in question.[5] That we standardly appeal to *past* conditions is surely a reflection of our habit of thinking that causality defines a temporal arrow. But isn't this folk physics? And if we are appealing to the past, the further question arises as to how far back into the past we should go. Does it matter if the age of the universe is finite or not?[6]

Then there is the question as to what extent the existence of irreversibility in the world really is a *problem* given the time symmetrical nature of micro-dynamics. We see symmetry-breaking all around us! If, for instance, we rotate our bodies by less than 360 degrees, or move sideways in space, we normally see a different arrangement of things in our line of sight, yet we take space to be locally isotropic and homogenous. Similarly, the world today is different from the world yesterday, yet we believe time to be homogeneous.[7] Should the thermodynamic arrow of time be considered any more problematic? Philosophers of physics agonise far more over it. Indeed, the temporal asymmetry of goings-on in the universe is sometimes referred to as a "paradox" in the light of the temporal symmetry of the fundamental laws.

I do not see that any inconsistency is involved.[8] It is widely accepted that phenomena such as the expansion of the universe, or the absence of white holes, are consistent with the time-symmetric field equations in general relativity. Thermodynamic irreversibility in the behaviour of macroscopic systems is arguably no different in classical statistical mechanics. It is,

[5]Carroll (*op. cit.*, chapter 3) stresses this point, but goes on (in footnote 51) to endorse Huw Price's claim that a consistent cosmology based on time-symmetric laws should have a time-symmetric history. (See Price (1996).) This claim strikes me as wrong-headed, for reasons outlined in the following paragraphs.

[6]Carroll (*op. cit.*, chapter 3) suggests entropic considerations might have ruled out steady state cosmologies from the beginning, since an eternal cosmological entropic gradient would be impossible to reconcile with such a cosmology. But in this context such an entropic assumption is surely questionable; in his seminal 1948 steady state model, Hoyle claimed that "thermodynamics has only localized applicaltion. There is no general thermodynamic degeneration of the observable universe as a whole." (Hoyle (1948), p. 381.) It is noteworthy that Einstein, keenly interested in the thermodynamic and electrodynamic arrows of time, was prepared to entertain a steady state cosmology until as late as 1931. (See O'Raifeartaigh (2016).)

[7]Note however that one of the motivations of the steady state cosmology was the notion that the cosmological principle should hold for time as well space; see Hoyle (1948).

[8]The consistency position is defended in Earman (1974). On the other hand, Michael Mackey has argued that the thermodynamic arrow of time for closed systems strictly requires non-invertible dynamics. See Mackey (2003), pp. xi and 102. For a more recent defence of the view that deterministic time-symmetric dynamics cannot account for the thermodynamic arrow of time, see Drossel (2014, 2015).

again, a question of (something like) initial conditions. If anything makes the thermodynamic case more dramatic, it is the ubiquitousness of the *same* entropic behaviour in quasi-isolated subsystems of the universe.

The first philosopher to appreciate this was (I think) Hans Reichenbach, who bequeathed to the world the terminology of *branch systems* in his 1956 book *The Direction of Time*.[9] These are macroscopic subsystems of the universe, which happen to peel themselves off, either naturally or through human intervention, from the rest of the universe. They then remain in a state of near-isolation either indefinitely or for some finite period of time. Reichenbach stressed, as did Paul Davies after him, that all our experience of thermodynamic irreversibility is gained by observation of such branch systems. (In 1974 Davies himself used the example of a cube of ice in a glass of lukewarm liquid, later to play a prominent role in David Albert's 2000 book.[10]) The striking thing about the branch systems that we observe in the universe is that they all display entropic behaviour in the same temporal sense. Doesn't this synchronisation of mutually non-interacting branch systems demand an explanation?[11]

A useful distinction in this context was made in 1994 by Davies when he separated the question of the "nature" from that of the "origin" of the arrow of time.[12] The first concerns the behaviour of any branch system following its initial formation in a low entropy, non-equilibrium state. The second concerns what accounts for the low entropy states in the first place, which inevitably introduces cosmological considerations. Davies, following Reichenbach, argued that the nature of the arrow of time has a relatively straightforward explanation[13]; his account of the origin is more tentative, based on big-bang cosmology. Lawrence Sklar, in his 1993 book *Physics and Chance*, criticised Reichenbach's treatment, and wrote:

> We could also just assume that the initial total [micro]state of the universe fully determines all its subsequent states. Then we would simply posit an initial state that gives rise to parallel entropic increase in branch systems with each other and with the main system. But to characterise the state in that way would, of

[9] Reichenbach (1991).

[10] Davies (1974), p. 69.

[11] As Zeh (2001), p. 54, has written, "The universality of the arrow of time seems to be its most important property." See also Penrose (2001), p. 79.

[12] Davies (1994).

[13] This is based on the notion that in a time-symmetric universe, states of low entropy are almost certainly followed and preceded by states of higher entropy; see Davis (1974), section 3.4.

> course, not be offering us an explanation of the sort we expected.
>
> ... to derive the Second Law from a bald assertion that "initial conditions were such that they would lead to Second Law behaviour" hardly seems of much interest.[14]

Can we do better? Should we?

The final quotations in this warm-up section are due to Ludwig Boltzmann:

> The Minimum Theorem [H-theorem], as well as the so-called Second Law of Thermodynamics, are only theorems of probability. The Second Law can never be proved by means of the equations of dynamics alone.[15]

> Since in the differential equations of mechanics themselves there is absolutely nothing analogous to the second law of thermodynamics, the latter can be mechanically represented only by means of assumptions regarding initial conditions.[16]

So is it initial conditions or probability considerations that take front stage in the attempt to understand thermodynamic behaviour from a mechanical viewpoint? According to the versions of the Past Hypothesis we will now discuss, it is both, but the details differ in important ways.

3 Albert's past hypothesis

In chapter 4 of *Time and Chance*, David Albert took a very dim view of Paul Davies' 1974 discussion of branch systems. Davies, following Reichenbach, was of the opinion that the familiar synchronised entropic behaviour for branch systems following an initial low entropy state in each (the "nature" issue) could be accounted for probabilistically; we need not concern ourselves for the moment with the precise argument. Davies wrote in 1974:

> ... it may be asserted confidently that almost all branch systems will show parallel entropy change. It is the asymmetry regarding the formation of the branch systems which brings about the parallel increase in all (nearly) branch system entropies. This asymmetry is definitely not supplied by statistics, coarse graining, the H-theorem or anything else. If the branch systems are

[14] Sklar (1993), p. 330.
[15] Boltzmann (1895), p. 414.
[16] Boltzmann (1904), pp. 170-171.

> regarded as not existing in the past, then the entropy of the overwhelming majority of these systems will increase with time. It is through branch systems that the customary intuitive notion that entropy increases with time is derived.[17]

The key claim here is that if branch systems *come into being* in low entropy states, then their entropy will, in all probability, not decrease over time, and observers will, in all probability, *fail to see anti-thermodynamic behaviour anywhere in the universe*. The separate question as to why the initial states are low entropy is what Davies (later) called the "origin" issue, as we have seen.

Albert considered Davies' reasoning to be "sheer madness". The basis of his scorn consists in the observations that the beginnings of branch systems are not exact events in time, that the branch systems themselves are part of larger systems within larger systems, and that a question of mutual consistency arises in relation to the statistical hypotheses being introduced in relation to all branch systems. Such qualms led Albert to believe that Davies' reasoning must therefore rest on an "epistemic" interpretation of the probabilities in the argument.[18] For those readers with doubts about the objective nature of probabilities, particularly in a theory involving deterministic dynamics (see section 7 below), Albert's ire may seem a little perplexing. However, our concern right now is more with what Albert inherits from Davies than what he rejects. He accepts Davies' temporal (if not probabilistic) logic, but regards it as applicable *only to the whole universe* "at the moment *it* came into being."

Here then are the elements of what Albert calls the *Newtonian statistical mechanical contraption for making inferences*. First, the microscopic goings-on conform to the laws of Newtonian mechanics. Second,

> The *Past Hypothesis* (which is that the world first came into being into whatever particular low-entropy highly condensed big-bang kind of macrocondition it is that the normal inferential procedures of cosmology will eventually present to us).

Third,

> The *Statistical Hypothesis* (which is that the right probability-distribution to use for making inferences about the past and the future is that one that's uniform, on the standard measure, over

[17] Davies (1974), pp. ??

[18] See footnote 16 in chapter 4, in Albert (2000).

those regions of phase space which are compatible with whatever information – either in the form of *laws* or in the form of *contingent empirical facts* – we happen to have).[19]

4 Comments on Albert's contraption

Some of the comments I wish to make about Albert's arguments for the Newtonian contraption apply equally, or nearly so, to David Wallace's version of the past hypothesis, so I defer them until later (section 6).

(i) It was noted by Uffink that insofar as it depends on developments that will "eventually" emerge in cosmology, Albert's Past Hypothesis is rendered impotent in relation to *current* inferences about the behaviour of statistical mechanical systems.[20] Whether Albert needed to be so tentative is not clear to me. At any rate, the hypothesis has also come under considerable criticism for presupposing that the notion of thermodynamic entropy can be applied to the universe as a whole (at least when gravity is taken into account), whatever the developments in cosmology might be.[21] This question has been discussed in a penetrating paper by David Wallace, which contains a nuanced defence of Albert in this regard.[22]

(ii) At what point in the history of the universe are Albert's "statistical" (i.e. probabilistic) considerations to be applied? The question may seem inapposite, given the assumption that the probability density is a solution of the deterministic Liouville equation, but bear with me. At the end of chapter 4 and in chapter 6, the statistical "posit" is applied to the present state of affairs (hence the wording of the Statistical Hypothesis above):

> Start with a probability-distribution which is uniform – on the standard measure – over the world's present macrocondition. Conditionalize that distribution on all we take ourselves to know of the world's entire macroscopic past history (and this will amount to precisely the same thing – if you think it over – as conditionalizing it on the *past hypothesis*). Then evolve this conditionalized present-distribution, by means of the equations of motion, into the future.

[19] Albert (2000), p. 96.
[20] Uffink (2002).
[21] See Earman (2006).
[22] Wallace (2009).

> This will yield (among other information) everything we take ourselves to know of the future.[23]

Of course we know next to nothing of the world's present macrostate (however we define it), simply because we have information only of events in our past light cone, but given that Albert is dealing with a Newtonian universe, perhaps this complication can be ignored. Uffink has also correctly pointed out that conditionalising on all we know of the world's past history is *not* the same as conditionalising on the past hypothesis;[24] I will return to this question shortly. The point I want to emphasise now is that a significant part of chapter 4 is taken up with the application of the statistical posit not at the present time but *at the first instant of the universe*; indeed strong arguments are adduced to the effect that such a statistical posit should be introduced *only* at this instant, and these arguments have in part to do with the time-symmetrical nature of the hypothesis (a feature to which I return below):

> ... *all* such posits are bound to fail – unless they concern nothing less than the *entirety* of the universe at nothing later that its *beginning*.
>
> That's what the statistical posit is going to have to be about, then. And if the project of statistical mechanics is on anything like the right track, then, when all the data are in, the initial macrocondition of the universe had better turn out to be one relative to which – on the standard uniform probability-distribution over micro conditions – what we think we know of the history of the world and its future, is *typical*.[25]

Likewise, in addressing the Davies/Reichenbach analysis of branch systems, and worrying (to repeat) about the consistency of statistical hypotheses applied to different individual branch systems, Albert writes:

> And all that aside, why in God's name *bother* with all this, when the uniform probability-distribution over the possible microconditions compatible with the macrocondition of the *world*, at the moment *it* came into being, will very straightforwardly give us everything we need?[26]

[23] Albert (2000), p. 122.

[24] Uffink (2002)

[25] *Op. cit*, p. 85.

[26] *Op. cit.*, p. 89. It is noteworthy that in his critique of Albert's contraption, Earman assumes that the moment of application of the Statistical Hypotheses is close to the initial instant of the world, not the present; see Earman (2006), section 6.

Yet Albert goes on in Chapter 4 to argue that it is *not* everything we need. The probabilistic reasoning is switched to the present (hence the wording in the Statistical Hypothesis), with the acknowledgment that this makes a low entropy start to the world vastly improbable. However, a new consideration justifies, according to Albert, conditionalising on the Past Hypothesis of low entropy: the hypothesis is predictively fecund. Whether this claim makes sense within Albert's contraption will be discussed shortly.[27] At any rate, it seems that for Albert this consideration allows us to simply ignore his earlier argument that applying the statistical posit at any time after the Big Bang makes a lower entropy past (relative to that time) hugely unlikely.

At this stage I feel that I am losing my grip on Albert's claim that probabilities in statistical mechanics are objective, and evolving according to the Liouville equation. This is not helped by reading that the reason we believe in the Past Hypothesis is because of its

> ... conspicuous success ... in making *predictions* about how *future* particular observations are likely to *come out*, and (more profoundly, perhaps) because it manages to render various of the *other* most fundamental convictions (about the veracity of our memories, and about the truth of the second law of thermodynamics, and about the accuracy of the dynamical equations of motion and about the reliability of the techniques of prediction and retrodiction, and so on) *compatible* with one another.[28]

What the reference to "success" presupposes is that in using the Newtonian contraption in the past we have made probabilistic predictions about the behaviour the universe that were compatible with what later actually transpired. But if one applies the contraption *now* to predict future such behaviour, we are conditionalising on (in part) what we have learnt about the world since the previous application. The relevant probability distribution will not be exactly the (Liouville) time-evolved distribution used last time, since certain arrangements of the world that are possible according to the previous predictions have now been ruled out. It appears we face exactly the kind of consistency problem that Albert sees in Davies' 1974 treatment.[29] The suggested (Bayesian) updating of the probabilities is non-problematic if the probabilities represent credences, but that interpretation is precisely what Albert rejects.

[27] See also the critical comments in Wallace (2010), section 1.

[28] *Op. cit*, p. 119.

[29] Earman (2006), p. 405.

(iii) In chapter 4 of *Time and Chance* we read that

> ... the statistical postulate, if applied to the present, is *flatly inconsistent* with what we take to be true, with what we *remember*, with what is recorded ... of the past.[30]

I take Albert to be plugging the argument, familiar within the neo-Boltzmannian literature, that if our probabilistic predictions are that higher entropy states obtain in the future, then the time-symmetrical nature of the contraption entails that higher entropy states are also highly likely in the past, contrary to experience (assuming our memories and records are veridical).[31] Albert brings this out nicely by using an iterative argument involving an ice-cube in likewarm water. But at this point a nasty thought presents itself. If the probabilistic reasoning is strictly time-symmetric, why doesn't the wild conflict with empirical evidence about the past, resulting from applying it to the present, or any time after the creation of the ice-water system, not represent a *refutation* of the statistical posit *tout court*, of the kind that would hold if the future behaviour of the system turned out to be anti-thermodynamic?[32]

Thinking of a spatial analogy might help to make the problem more evident. You can buy a modern domestic heater from the British firm Dyson, which improbably looks like a scaled-up version of the head of a needle, vertically mounted on a moveable stand. The eye of the needle blows out hot air when the device is switched on. An initial cursory look at the device reveals nothing that distinguishes the front from the rear, and a not unnatural hypothesis would be that the mechanism itself is symmetric and so blows hot air in both directions. But on turning the device on, you find that hot air is blown in only one direction! In relation to the symmetry hypothesis, there seem to be two options. The first is that the hypothesis is flatly refuted by the actual operation of the device. The second – the spatial analogue of Albert's reasoning – is that by putting the device flush up against a wall, thus blocking one direction of putative heat flow, no incriminating evidence is seen, and the hypothesis is saved.

So it seems to me that unless there is a principled reason for positioning the statistical posit regarding the universe at the Big Bang or near it, the

[30] Albert (2000), p. 81.

[31] Since there is a very small chance, according to the argument, that the past actually conforms to what the records indicate, the claim that the statistical hypothesis is "flatly inconsistent" with such records is obviously an overstatement.

[32] Earman (*op.cit.*) raises the obvious question, following his demonstration that something like Albert's statistical hypothesis can only hold at one time, as to why it should be regarded as valid at that very time. Our criticism is somewhat different, but in the same spirit as Earman's.

spectre of outright refutation of the contraption looms, *if the probabilistic reasoning is time-symmetric*.

But is it? I am reminded of an observation that Gibbs made, one that Albert in particular found unacceptable: that *probabilistic inferences are of their nature time-asymmetric* in a certain sense. But to take this further means opening the proverbial can of worms that is the meaning of probability. I do this in section 7 below. In the meantime note that nothing in Albert's contraption actually exploits the alleged time-symmetric nature of probabilities. By this I mean that effectively no probabilistic retrodictions, as opposed to predictions, are found in *Time and Chance* once the final details of the Newtonian contraption are arrived at. When the Statistical Hypothesis is applied to the present world macrocondition, it is conditionalized on the known past history of the universe. No probabilistic retrodictions here. When the statistical posit is applied to the birth of the universe, retrodictions of any kind are obviously out of the question!

(iv) In Albert's contraption, the probability of a microstate, randomly chosen from the known macrostate of the system in question (whether the ice cube in water, or the universe), leading to future macrostates of higher entropy, is determined by appeal to the "standard" measure on the phase space as well as a probability distribution on the space. The standard measure, which Albert also refers to as the "familiar" measure, is clearly the Liouville/Lebesgue (LL) measure. The physical credentials of the LL measure presumably have to do with the fact that it is preserved under the Hamiltonian flow (the basis of the Poincaré recurrence theorem which Albert discusses in chapter 4[33]), and the fact that it provides the phase space volumes which appear in the definition of the Boltzmann entropy. However, there appears to be a degree of arbitrariness in the statistical hypothesis.

Call the non-equilibrium macrostate of the system at a given time M, and let N be the set of what Albert calls "abnormal" microstates within M which lead in the future to lower Boltzmann entropy. Albert *assumes* that the LL measure of the subset N is vastly smaller than that of its complement in M, denoted here by $M - N$.[34] Then assigning a probability distribution

[33]For a critical discussion of Albert's proof of the theorem, as well as his account of Boltzmann's H-theorem, see Brown *et al.* 2009

[34]If M refers to the universe (or branch system) and the time t is later than the Big Bang (or the creation of the branch system), then it makes no difference to the argument if M is restricted to microstates consistent with the known macrostate at t but also consistent with the initial low entropy macrostate just after the Big Bang (or creation). In his recent review of the foundations of statistical mechanics, Frigg has pointed out there has been relatively little discussion in the literature concerning the justification of the above measure theoretic assumption; see Frigg (2013). However, it is unclear whether it is even

that is *uniform* over M and zero elsewhere in the phase space means that entropic behaviour of the system in the future will in all likelihood be what is predicted in thermodynamics, and what we actually observe to be the case. Note that in chapter 3, Albert takes it as

> ... some sort of *fact* – or at any rate it seems to make correct *predictions* to *suppose* that it is some sort of fact – that the *percentage* of any *large collection* of randomly selected systems whose microconditions lie within any particular subregion of the X-region of the phase space will be more or less proportional to the familiarly defined [LL] *volume* of that subregion. And so the sort of fact that [it] is must be an *empirical* one, a *contingent* one, a *scientific* one.[35]

The notion of "random selection" here is of course purely theoretical. As E. T. Jaynes pointed out:

> It almost never makes sense ... to think of the probability of a microstate as a real frequency in any "random experiment". In thermodynamics the imaginary experiment would have to be repeated for perhaps $\exp(10^{24})$ times before there was much chance of that particular microstate appearing even once.[36]

Suppose then, for the sake of argument, the LL measure on the phase space assigns roughly equal weights to N and $M-N$ as defined above. One can still maintain that the probability of observing anti-thermodynamic behaviour is very small as long as a suitable, *non-uniform* probability density is assigned to M, whose (Lebesgue) integral over N leads to a number much smaller than the corresponding integral over $M - N$. If the probability density is supposed to be objective, how would we know whether this version of the statistical hypothesis is wrong, when it gives us the same phenomenological results as Albert's?

Of course this rival suggestion is degenerate: there is a plethora of distinct combinations of measure assignments to N and $M-N$ and non-uniform probability distributions that do the trick; we cannot know which one to choose. Albert's contraption is not degenerate in the same sense: there

uniquely specifiable within a neo-Boltzmannian account of irreversible behaviour, given the conceptual distinction between measures and probability distributions; see below.

[35] *Op. cit.* p. 65. Presumably this "fact" is related to Albert's further assumption in chapter 3 that within the low-entropy macrostate M, the subset N of "abnormal" microstates is composed of pockets randomly scattered throughout M.

[36] Jaynes (1985).

is only one uniform distribution, in which case the measure assignment is largely fixed: the measure of N is vastly larger than that of $M - N$. (It is not clear that this is really the simplest hypothesis; what about the possibility involving the measures of N and $M - N$ being exactly equal?) But surely the obvious justification for choosing a uniform probability distribution in the first place is that it reflects our knowledge that the initial microstate lies within M, and our undifferentiated ignorance as to where it is within M. This is hardly in tune with the notion that the distribution reflects something objective about the system.[37]

(v) Albert is fully aware of the fact that for macrosystems, the equilibrium macrostate as defined by Boltzmann is overwhelmingly larger (according to the LL measure) than any other macrostate. In Chapter 3 of *Time and Chance*, Albert says that it is "precisely this imbalance ... that gets the statistical-mechanical account of the second law of thermodynamics off the ground."[38] It is noteworthy, then, that in Chapter 4, he appears to treat the imbalance as having heuristic significance and no more; in particular he distances himself from the well-known neo-Boltzmannian combinatorial argument to the effect that given such huge imbalance, a system starting in a non-equilibrium macrostate will, with high probability, find itself "reasonably quickly" in the equilibrium state — unless the dynamics is "ridiculously special".[39] At any rate, the contraption Albert develops in Chapter 4, which we have just been discussing, provides quite a different explanation of equilibration, or at least an entropic gradient. As far as I can see, nowhere in the contraption is there an appeal, or the need for an appeal, to this huge asymmetry in the LL-volume of the equilibrium macrostate in relation to any other macrostates, at least as it applies the universe (which is not currently in equilibrium). It is consistent with the contraption that the macrostates that the universe has been displaying throughout its history have been successively larger in the LL-measure — but not hugely so. There is no reason why this cannot continue to equilibrium, although of course there are very good *independent* arguments for the equilibrium macrostate being vastly larger than any other. (We shall see later that Wallace's contraption likewise makes little use of the neo-Boltzmannian combinatorial argument.)

(vi) Recall Sklar's frustration (see the end of section 2 above) with the

[37]Earman (2006), section 3, also raises the question of the connection between the Liouville measure and physical probabilities, and uses the ergodic hypothesis – which Albert repudiates – to shed light on the matter.

[38]*Op. cit.*, p. 45.

[39]See Goldstein (2001). Albert regards the argument as no more than "a crude stab" at explaining irreversibility.

view that the mechanical derivation of the second law of thermodynamics (to the extent to which it is valid) boils down to the bald claim that the initial (micro-)conditions lead to it. It is not clear to me exactly what would count as a remedy for Sklar. But it would surely be nice if within cosmology we could find some kind of explanation as to why the universe spawns off branch systems in low entropy states – a key aspect of Davies' "origin" question.[40] It would be nice too to have mechanical models of thermal relaxation processes for branch systems that provide quantitative details of diffusion coefficients, relaxation times, and so on – issues arguably related to Davies' "nature" question.

Albert's contraption is not designed to meet either of these desiderata (more on this below). Be that as it may, it is not clear to me whether those nurturing such Sklarian frustration should be assuaged when contemplating Albert's contraption. If the statistical posit is applied at the birth of the universe, the initial conditions lead to the second law, *but only probably.* Is this helpful? When the Statistical Hypothesis is applied at the present moment, then the contraption apparently treats all we know about the entropic history of the world itself *as a posit*; it does not explain it. And for those who do not follow Sklar, and for whom the initial microcondition is unashamedly the be-all and end-all of the story, the introduction of probabilities associated with such conditions might seem an unnecessary complication, except at the present moment. But here the credences about the future are the result either of inductive reasoning based on knowledge of the past, or of a Boltzmann-type combinatorial argument, and not of solving Liouville's equation.

5 Wallace's contraption

5.1 Motivations

In *Time and Chance*, when David Albert arrives at his Newtonian inferential contraption at the end of chapter 4, it is the result of systematic trial-and-

[40] Davies points out that a full explanation would

> need to consider the fact that the universe has evolved through a series of self-organizing instabilities, leading to what Freeman Dyson has called 'hang-ups', where subsystems get 'hung up' for long durations in quasi-stable, quasi-isolated states. The reason for the existence of these hang-ups clearly involves aspects of the laws of physics (e.g. the values of some constants) as well as initial conditions. (Davies (1994), p. 129)

error reasoning for the length of this and the previous chapter, without introducing a single equation. In his 2011 paper *The Logic of the Past Hypothesis*, David Wallace arrives at his version of the past hypothesis after about nineteen pages of relatively heavy technical machinery. Indeed, when you have worked through this machinery and find that the central notion is called the "Simple Dynamical Conjecture", you cannot but feel that this is a testament to Wallace's sense of humour.

There are three basic elements in Wallace's treatment of irreversibility: *description*, *justification* and *the quantum*.

The first has to do with his claim that prior versions of the Past Hypothesis fail to take into account the fact that in modern chemistry, in particular, the study of processes of equilibration involves not just qualitative but quantitative analysis. He writes:

> It is, at best, very difficult to see how these quantitative theories of the approach to equilibrium fit into the very general argument for equilibration given by Albert, Goldstein *et al.*[41]

Wallace is of the view that successful quantitative descriptions of the process of equilibration almost invariably involve appeal to a kind of coarse-graining procedure applied to Gibbsian probability distributions over microstates of the system in question.

The second element in Wallace's treatment concerns the attempt to provide the "conceptual explanation", or "justification", of such a construction. Hence the role played by his version of the past hypothesis, which is quite different in detail from Albert's.

The third feature that distinguishes Wallace's analysis from Albert's is that Wallace is intent on developing a formal approach that applies to both classical and quantum physics, where the latter is understood in the sense of the Everett interpretation. It is important at this point to emphasise Wallace's thinking:

> So my approach is in general to study the classical and quantum cases in parallel, and to neglect the classical theory in favour of the quantum one where they differ in important respects. If we are interested in understanding irreversibility in our world, after all, classical systems should be of interest to us only insofar as they are good approximations to quantum systems.[42]

[41] Wallace (2011).
[42] *Op. cit.*

As I mentioned in the Introduction, I will be concerned mostly with the study of the classical case, but I include some remarks about the quantum case at the end. Here is a brief summary of Wallace's logic.

5.2 The Simple Past Hypothesis

This is stated as:

> There is some Simple distribution function ρ over the phase space of the Universe such that for any microstate x, $\rho(x)\delta V$ is the objective probability of the initial state of the Universe being in some small region δV around x.[43]

Note that such a probabilities are "are not mere expressions of our ignorance, but are in some sense objectively correct." Much hangs of course on what sense this is, and what the Simplicity criterion amounts to, and for the moment I shall concentrate on the latter.

Consider a macrosystem (not necessarily the universe) whose state is described by probability distribution ρ over its phase space, and a coarse-graining map $\mathcal{C}$ which projects from the distribution space onto some subset of the distributions. Being a projection, $\mathcal{C}$ satisfies the idempotency property $\mathcal{C}^2 = \mathcal{C}$, so that the the coarse-grained distribution $\mathcal{C}.\rho$ is unchanged by the map. The coarse-graining map is such that the probability of any given macroproperty of the system is approximately unchanged by its action on ρ.

Now we come to the issue as to how to compare the standard Liouville evolution of a distribution ρ with a coarse-grained version of the evolution. Here the technicalities in Wallace's account are considerable, so the following informal synopsis must be treated with care by the reader. Wallace is interested in the macrohistory α of the macrosystem defined over an increasing sequence of times $t_1, ..., t_N$, which means the specification at each time t_i of a macro-property $\alpha(t_i)$. Describing the macrohistory dynamically means in the first instance taking the initial probability distribution ρ and alternately evolving it forward by the Liouville microdynamics and then restricting to the successive terms in α, i.e. projecting it onto the appropriate macrostate.[44] Now compare this with the coarse-grained dynamical picture, where for each interval $t_k - t_{k-1}$, the Liouville evolution is replaced in the above procedure by coarse-grained evolution relative to $\mathcal{C}$. This involves

[43] *Op. cit.*

[44] See equation (2) in *op. cit.*

taking the distribution ρ, evolving it forward using the deterministic Liouville microdynamics for some time interval Δt small compared to $t_k - t_{k-1}$, coarse-graining it, evolving it for another small interval, coarse-graining, and so on.[45] (Wallace refers to this as the forward dynamics induced by $\mathcal{C}$, or the $\mathcal{C}+$ dynamics.) Now if the results are the same in both procedures for a given history space,[46] Wallace says that ρ is *forward predictable* by $\mathcal{C}$ on that history space. This signifies a significant constraint on the initial distribution, relative to $\mathcal{C}$; it effectively introduces irreversibility into the macrodynamics of the system in question.

Now come the hypotheses. Wallace introduces the *Simple Dynamical Conjecture* for a system of the kind we are interested in with coarse-graining $\mathcal{C}$: any distribution whose structure is at all simple is forward predictable by $\mathcal{C}$. If a *Simple* distribution is defined as one specifiable in a closed form in a simple way, then Wallace says that the Simple Dynamical Conjecture is just the conjecture that all Simple distributions are forward predictable by $\mathcal{C}$. Combining this conjecture with the Simple Past Hypothesis above for the universe, we finally arrive at Wallace's account of macro-irreversibility:

> ...the initial state of the world is forward predictable by the $\mathcal{C}+$ dynamics: the macrodynamics defined by the $\mathcal{C}+$ dynamics is the same as the macrodynamics induced by the microdynamics.[47]

6 Comments on Wallace's contraption

(i) The Simple Dynamical Conjecture is defined relative to a given macrohistory of the system. If the system of interest is isolated and finite, and subject to the recurrence theorem – which in the case of the universe is implausible – then the conjecture can be true only for a history short in relation to the recurrence time (subject to the usual exclusion of non-recurring initial states which form a set of LL measure zero in the classical case[48]). This somewhat vague constraint is forced by pain of contradiction and not, as far as I can see, by the intrinsic nature of the Simple distribution. It is obvious, furthermore, that Simplicity (given the Simple Dynamical Conjecture) cannot be a necessary condition for equilibration. At any future time before the end of the specified macrohistory, the probability distribution will no longer

[45] See equation (15) in *op. cit.*

[46] See equation (21) in *op. cit.*

[47] *Op. cit.*

[48] This caveat is unnecessary in the quantum recurrence theorem. For an insightful comparison of the classical and quantum recurrence theorems, see Wallace (2015b).

be Simple (see comment (iv) below). And in the case of a finite system, even when, after an unimaginably long time, the initial non-equilibrium microstate of the system (or one arbitrarily close to it) recurs, the probability distribution will still not be Simple, even though eventually the system will re-equilibrate.

(ii) Recall that when Albert applies the statistical posit, either at the birth of the universe or at the current time, the future of the system with respect to that instant has non-decreasing entropic behaviour *only with high probability*, according to his contraption. But given Wallace's Simple Past Hypothesis at the birth of the universe, and his Simple Dynamical Conjecture, it might seem that monotonic movement towards equilibrium is *guaranteed*:

> ... any stipulation of the boundary conditions of the Universe according to which the initial distribution of the Universe is reasonably simple will (together with our microphysics) entail the correctness of our [thermodynamical] macrophysics.[49]

There is no question that given Wallace's assumptions, the coarse-grained Gibbsian entropy of the universe must have been non-decreasing since its inception, assuming that the relevant macrohistory defined in the Simple Dynamical Conjecture is sufficiently extended to include the entire history of the universe. Presumably it extends further; it would seem arbitrary to limit it to the present moment. But this means that we can expect that such entropy will be non-decreasing in the future (or if the universe or some branch system is subject to recurrence, then at least for future times small in relation to the recurrence time). Is this a different kind of forecast from that generated by Albert's contraption (or defended by neo-Boltmannians more generally) in which anti-thermodynamic future behaviour is possible but assigned very small probability? It is important to note that Wallace distinguishes his reasoning from that of some neo-Boltzmannians (see section 4(v) above) in the following way:

> ... it is not that systems are guaranteed to achieve equilibrium unless they or their dynamics are "ridiculously special"; it is that only in "ridiculously special" cases will the micro-evolution of a distribution not commute with coarse-graining. Whether, and how fast, a system approaches thermal equilibrium is then something that can be determined via these coarse-grained dynamics.

[49] *Op. cit.*, section 9.

> In particular, it seems reasonable to make the Simple Dynamical Conjecture that reasonably simple distributions do not show anomalous behaviour.

Arguably, this last sentence does not do justice to Wallace's reasoning. The ridiculously special cases that he has in mind are not associated with non-simple initial conditions, but rather with possible exceptions to the kind of irreversible behaviour we expect of thermodynamic systems even when the initial non-equilibrium conditions are simple. Like Albert, Wallace accepts that "anomalous behaviour" is possible but very unlikely. But the basic reason for this quite different from Albert's: it is that the Gibbsian entropy is defined in terms of the probability distribution ρ, and not the microstate of the system as in the Boltzmann entropy.[50]

I confess I do not find this logic entirely transparent, partly because the role of ρ in determining the probability of anomalous behaviour is unclear to me, and also because lurking in the background there is the broader question of what this probability distribution even means in the context of classical, deterministic dynamics; this question will be taken up shortly.

(iii) In Wallace's 2011 paper *The Logic of the Past Hypothesis*, there is no reference to the LL measure in defining the Simplicity criterion, and it is clear that a Simple distribution is not unique for a given macrohistory. But in his 2013 paper *Probability in Physics ...*, Wallace expands on what he means by Simple distributions:

> ... there are very good (albeit somewhat non-rigorous) grounds to believe that for a certain very wide class of probability distributions (which could be characterised as being all those which do not vary too chaotically and sharply with respect to a certain baseline distribution, the Liouville measure) that the macrodynamics generated by each of these distributions coincide (or very nearly so) and determine a unique probability distribution over future histories for any given macrostate at any time later than the time t_0 at which the probability distribution is defined.[51]

I take it the baseline distribution here is that which is uniform relative to the Liouville measure, the relevant set in the phase space presumably being the initial (Boltzmannian) macrostate of the universe. In that case, the doubts raised in comment (iii) in section 4 above in relation to the role of the Liouville measure in Albert's contraption apply equally well here.

[50] David Wallace, private communication.

[51] Wallace (2013), p. 6.

(iv) Unlike Albert, Wallace recognises that his statistical posit (the Simplicity condition) is not time-translation invariant; he shows explicitly that generally the forward time evolution of a Simple distribution is not Simple. There is a *unique*, once-and-for-all time at which the condition can be applied in the history of the universe, and thus Wallace appears to avoid the consistency problem that I raised in relation to Albert's contraption in point (ii) of section 4 above. But he argues that at any time other than (very near) the first instant

> ...the backwards dynamics induced by basically any coarse-graining process is not empirically successful at all: in general it wildly contradicts our actual records of the past.[52]

This is the critical reason that the Simplicity condition must be applied at or very near the first instant of the universe. In Wallace's words again:

> We impose this probabilistic boundary condition at the beginning of time (or at least, at the beginning of whatever period of time we can empirically access) because this is the only way to rule out the time-reversed macrodynamics that would otherwise occur to the past of whatever time we choose to impose the boundary condition.[53]

This reasoning is analogous to that found in chapter 4 of Albert's *Time and Chance* (see point (ii) in section 4 above) in justifying the placement of the statistical posit at the birth of the universe, but it is seems more persuasive. Wallace points out that a distribution will be Simple if and only if its time reverse is. If the Simple Dynamical Conjecture is true for the future, it is hard to see how it cannot be also for the past, given the time-symmetric nature of the fundamental laws of classical mechanics. So within the specified macrohistory of the system, the Simple distribution must correspond to a non-recurring entropic minimum, and all the evidence is that this must be at the beginning of the macrohistory of the system.

I think a word of caution is in order here. I questioned in point (iii) of section 4 above whether the analogous reasoning in Albert's contraption is convincing if there is reason to believe that nature of probabilistic reasoning is not time-symmetric. (I shall offer such a reason below.) Since, as we have seen, Wallace believes that the macrodynamics following the Simple

[52] Wallace (2011), p. 15.
[53] Wallace (2013).

distribution is (Boltzmann) entropy non-decreasing only with high probability, it is seems to me that a similar question may be asked of the Wallace contraption.

(v) Wallace remarks that he has no need to postulate a low entropy to the initial state of the universe relative to its present state, since the coarse-grained Gibbs entropy is non-decreasing from the initial instant according to the forward dynamics. He claims that Albert overlooked the fact that the low entropy Past Hypothesis is likewise redundant in the contraption defended in *Time and Chance*, but this fact relies, as I see it, on spurning the option raised by Albert of applying the statistical posit at the present time.

(vi) To repeat, Wallace envisages the use of coarse-graining as almost an inevitability in any satisfactory account of macroscopic dynamics:

> ... the forward dynamics induced by coarse-graining classical or quantum mechanics has been massively empirically successful. Pretty much all of our quantitative theories of macroscopic dynamics rely on it, and those theories are in general very well confirmed by experiment. With a great deal of generality – and never mind the conceptual explanation as to why it works – if we want to work out quantitatively what a large physical system is going to do in the future, we do so by constructing a coarse-graining-induced forward dynamics.[54]

Is this always so? Let's see.

6.1 The Boltzmann equation

The dilute gas (in which collisions between molecules are binary) provides, in Wallace's words, "perhaps the best-known example of an evolution equation for non-equilibrium statistical mechanics"[55]:

$$\frac{\mathrm{d}f}{\mathrm{d}t} = N \int \mathrm{d}v'\mathrm{d}u'|v - v'|\sigma(uu' \to vv')\left(f(u)f(u') - f(v)f(v')\right) \qquad (1)$$

This is of course the Boltzmann transport equation, where the "probability" density $f(\mathbf{r}, \mathbf{v}, t)$ is assumed to be spatially uniform and isotropic and so can be expressed as $f(v, t)$. N is the particle number density, σ is the scattering cross section. Derivation of the equation requires that the collisions be

[54] *Op. cit.*

[55] Wallace (2015), section 5.

subject at each instant to the *Stosszahlansatz* condition (henceforth SZA) which states that pairs of molecules about to collide are uncorrelated in velocity. Boltzmann's corollary, his famous H-theorem, established that a dilute, spatially homogenous gas spontaneously and monotonically tends to the Maxwell-Boltzmann velocity distribution.

Wallace takes this to be one of several examples of the *quantitative* treatments of irreversibility that he is trying to encapsulate in his forward dynamics. Even E. T. Jaynes, who was not a fan of Boltzmann's H-theorem, had to admit that his "collision" equation

> ... gives definite theoretical predictions for transport coefficients (viscosity, diffusion, heat conductivity) ... the fact remains that it has been very successful in giving good numerical values for these transport coefficients; and it does so even for fairly dense gases, where we really have no right to expect such success.[56]

However, I would argue that the Boltzmann transport equation, at least as originally conceived, provides a counterexample to the coarse-graining paradigm, insofar as this paradigm involves a Gibbsian probability distribution.

The key question is whether the "probability" distribution involved in the Boltzmann equation and the H-theorem is anything like the probability distribution involved in Wallace's treatment, which is defined over the phase space of the system of interest. As Wallace himself notes, Boltzmann understood $f(v, t)$ to mean the fractional number density of particles with a velocity v at time t, as opposed to the common interpretation in modern textbooks as the marginal one-particle *probability* distribution averaged over particle position (see below).[57] Under Boltzmann's reading, f *is a property of a given microstate, not a probability distribution over microstates.*[58]. Since the number of molecules in a gas is finite, f is strictly a sum of delta functions, and not a continuous function that is differentiable with respect to $\mathbf{v}$ and t. Although some theorists have concluded that f must therefore

[56] Jaynes (1967), p. 91. Further remarks supporting the importance of the Boltzmann equation in understanding the equilibration process in dilute gases and beyond are found in Wallace (2015a).

[57] In his 1872 H-theorem, Boltzmann initially awkwardly defined f as a density function on kinetic energy space (see Klein 1963). On a separate note, Jaynes (1965) argued persuasively that the negative of the H-function and the fine-grained Gibbs entropy, though similar in appearance, are different when intermolecular interactions are taken into account. There is also the obvious point that this Gibbs entropy is strictly a constant of the motion, where H is not.

[58] See, e.g., Klein (1973), Jaynes (1967) and Goldstein (2001).

be the probable, or alternatively, average number of molecules within the mentioned volume elements, this was not (to repeat) Boltzmann's original view, nor indeed Maxwell's.[59] Presumably the idealised treatment of the function is justified by the vast number of molecules involved.[60] But this makes the SZA in turn a condition on the instantaneous microstate of the gas; no ensembles of gases are involved then in interpreting the Boltzmann transport equation or the H-theorem in its original guise.[61]

Wallace nonetheless views the application of the SZA in the Boltzmann equation and the H-theorem as an instance of coarse-graining:

> This assumption [SZA] is in general very unlikely to be true . . . , but we can reinterpret Boltzmann's derivation as the forward dynamics induced by the coarse-graining process of simply discarding those correlations. . . .
>
> . . . Pretty much all of non-equilibrium kinetic theory operates, much as in the case of the H theorem, by discarding the correlations between different particles' velocities.[63]

I confess I am puzzled. The validity of the Boltzmann equation requires that the relevant correlations actually *vanish* at each moment the SZA is valid. As to whether such a condition is improbable[64], the widespread applicability of the Boltzmann equation – even in the case of galactic dynamics, as Wallace himself stresses,[65] should raise doubts.

Boltzmann's H-theorem has received rather bad press, in both the physics and philosophical literature, in the light of Loschmidt's reversibility objection and Zermelo's recurrence objection.[66] But I have come to think that neither objection is very telling, and that in trying to address Loschmidt's critique Boltzmann muddied the waters horribly by introducing a probabilistic element into the workings of the H-theorem – an element which,

[59] See Myrvold (2011).

[60] See Spohn (2001), p. 57.

[61] A simple physical ("wind-tree") model, devised originally by the Ehrenfests[62] with a view to exposing the basic elements of the H-theorem, clearly demonstrates that Gibbsian probabilities are not intrinsic to the reasoning; see Brown *et al.* (2009).

[63] Wallace (2011), section 3. Note that Wallace refers to correlations amongst particle velocities, but the SZA refers to a proper subset of particles, namely those about to collide at the instant the SZA is assumed to hold.

[64] Here Wallace *ibid.* cites an argument to this effect from Sklar (1993), pp. 224-7.

[65] Wallace (2015a).

[66] See for example Brown *et al.* (2009), Gogolin and Eisert (2016), section A.1.1.

as Jaynes scathingly noted, has no clear role in the analysis.[67] (It was of course Boltzmann's later combinatorial/probabilistic approach to the analysis of irreversible behaviour, which essentially sidestepped the collision-based considerations leading to the transport equation and the H-theorem, that influenced Albert's thinking on the past hypothesis. But it is worth noting that Boltzmann never abandoned the H-theorem.) Neither objection rules out the possibility of an initial non-equilibrium microstate of the system leading to extended periods of time (but short obviously compared to recurrence times) within which the (time-asymmetric) SZA is approximately valid at every instant, so the gas displays a spontaneous approach to the equilibrium Maxwell-Boltzmann velocity distribution.[68] Boltzmann is providing a mechanical model of "irreversible" behaviour over a certain period of time. As Wehrl nicely put it in 1978,

> . . . although the time evolution of the total system is given by the Hamiltonian dynamics, under certain conditions the time evolution of the first correlation [single particle density] function can be described, in fairly good approximation, by an irreversible equation.[69]

Let us not quibble as to whether Boltzmann's H-theorem is an *explanation* of spontaneous thermalization; after all, something time asymmetric *has* to be put in somewhere. There is of course the further question, which is why we are surrounded by branch systems which start their lives in such special initial states – what Davies calls the "origin" problem, as we have seen. This was simply not Boltzmann's concern when he derived his celebrated transport equation, though it is intimately connected with Loschmidt's objection.[70]

[67] Jaynes (1967). For a discussion of Boltzmann's probabilistic turn, see Brown *et al.* (2009).

[68] Davies in his 1974 book (section 3.1) argues that the SZA can only hold at isolated peaks in the H-curve, and repeats this claim in his 1994 paper. The error in Davies' reasoning is pointed out in Brown *et al.* (2009). Another mistaken claim is that the SZA outside equilibrium is inconsistent with the time-symmetric nature of the dynamics of collisions (based in turn on the misleading claim that collisions produce correlations). This mistake is found in the otherwise excellent books by Lockwood (2005), pp. 206-8, and Blundell and Blundell (2006), section 34.5.

[69] Wehrl (1978), p. 227.

[70] Two decades later, in responding to a criticism by Zermelo, Boltzmann wrote: "An answer to the question – how does it happen that at present the bodies surrounding us are in a very improbable state – cannot be given, any more than one can expect science to tell us why phenomena occur at all and take place according to certain laws." See Brown *et al.* (2009), p. 179.

But the key point I want to stress is that it is not strictly necessary to regard the density function involved in its workings as anything other than a property of the instantaneous microstate of the system – despite the fact that many modern treatments treat the equation as the one-particle first equation in the BBGKY hierarchy, which is built on the distinct (Gibbsian) notion of a fundamental probability distribution over microstates.[71]

Now it must be recognised that the original 1872 H-theorem is incomplete in an important sense. As long as the SZA is valid, once the equilibrium Maxwell-Boltzmann distribution is achieved, it is permanent. There is no possibility of fluctuations from the equilibrium distribution, and thus we are apparently faced with an "outrage against Gibbsian common sense".[72] Indeed, it is sometimes claimed that the original Boltzmann transport equation is inconsistent with the fluctuation-dissipation theorem (FDT), which since the 1960s has led to significant insights into non-equilibrium statistical mechanics.[73] This question deserves far more discussion than space allows in this essay, but a few remarks are in order.

One remedy to the (lack of) fluctuations problem in the context of the BBGKY hierarchy is to add to the linearised Boltzmann equation a stochastic noise term.[74] This addition is rather *ad hoc*, in the sense that it restores consistency with the FDT; no attempt is made to introduce it in a principled fashion. It is unclear to me whether such a term is involved in the usual applications of the Boltzmann equation to gases and galaxies. At any rate, it seems to me perfectly legitimate to accept that Boltzmann's original reasoning was incomplete, and to assert that it nonetheless provided a useful mechanical model for thermalization which does not involve probabilities — at least if one is prepared to put aside the above-mentioned "origin" problem concerning initial conditions for branch systems. And all this within time-reversal invariant fundamental dynamics: information is not being "thrown away", nor is there any coarse-graining in the argument. As we will see below, a close analogue of the Boltzmann equation in quantum field theory (QFT) likewise makes no appeal to Gibbsian ensembles, but, notably, *does* incorporate fluctuations around equilibrium.

[71]See for example the treatment in Harris (2004), and in Wallace (2016). In his 1978 review paper on entropy, Werhrl asserts that the density function for a single gas is "obtained from the 'true' distribution in phase space by some sort of averaging". See Wehrl (1978), pp. 227-8.

[72]Calzetta and Hu (2008), p. 58.

[73]See Kubo (1966).

[74]Calzetta and Hu (2008), p. 59.

7 Probability

As we have seen, for both Albert and Wallace the probabilities involved in their contraptions are objective. Here is Wallace:

> . . . the probability distribution in statistical mechanics grounds objective features of the world. The emergent almost-autonomous stochastic macrodynamics have a definite directedness in time and that directedness is a direct consequence of the imposition of a Simple probability distribution at the start of the universe rather than its end. . . . phenomena like the melting of ice or the diffusion of gas have a clear time direction which can be tracked back (at least in part) to the probabilistic boundary conditions. If those boundary conditions are simply a matter of credences, it is difficult to see what objective facts about the world are supposed to ground its objective dynamical asymmetries in time.[75]

Often, the notion of probability is regarded as objective if it is defined in terms of empirical (relative) frequencies, but the frequentist account of probability is beset with problems that are well known and need no rehearsing here. It seems to me that neither Albert nor Wallace provide a plausible account as to what probabilities mean operationally in their classical contraptions, and how it is that they come to be objective. In the absence of such an account, it is unclear to me to what extent Albert and Wallace are successful in explaining, as they both claim to, the thermodynamic history of the universe. Nor is it obvious that the "objective facts about the world" that are supposed to ground the objective asymmetry in its behaviour over time need involve probabilities at all. They will certainly be related to initial/boundary conditions, but a deterministic universe cares about microstate conditions, not probability distributions over microstates. (Whether these concerns arise in Wallace's account of irreversibility in quantum mechanics is a question taken up later.)

The analysis of probability in classical statistical mechanics that makes most sense to me is the subjective Bayesian approach defended by E. T. Jaynes and more recently by Jean Bricmont. Assigning a probability to an event is, in this approach, determining a rational (unbiased) estimate of likelihood of that event on the basis of prior information. Depending on the context, this information may involve the current macostate of the system, the past macro-history of the system, or both. For Jaynes, probabilistic

[75] Wallace (2013).

reasoning is a central part of ampliative or inductive reasoning that goes on all the time in life: "common sense reduced to calculation" (Laplace). He regarded his work in statistical mechanics as a natural development of the views of J. W. Gibbs, while eschewing the Gibbsian appeal to ensembles and any ergodic-type justifications probability distributions. Writing in 1985, Jaynes stressed that although "...the very idea that a probability distribution describes our state of information is foreign to almost all recent expositions of statistical mechanics"[76], the idea is essential if we want to modify our probability distribution when we have additional information in the sense of Bayesian updating. An aspect of Wallace's thinking I find hard to understand is the joint claim that the "objective" probability distribution ρ is a solution of the deterministic Liouville equation, and that

> ...if we want to retrodict we do so via the usual methods of scientific inference: we make tentative guesses about the past, and test those guesses by evolving them forward via the forward dynamics and comparing them with observation. (The best-known and best-developed account of this practice is the Bayesian one: we place a credence function on possible past states, deduce how likely a given present state is conditional on each given past state, and then use this information to update the past-state credence function via Bayes Theorem.)[77]

For as Jaynes noted,

> If one believes that a probability distribution describes a real physical situation, then it would seem wrong to modify it merely because we have additional information.[78]

[76] Jaynes (1985).

[77] Wallace (2010).

[78] Jaynes (1985). In similar vein, Bricmont wrote: "...there is no good reason why one should let probabilities change in time according to the evolution induced by the physical laws (like the the Liouville equation in classical physics). They certainly do not obey such laws when one does Bayesian updating." See Bricmont (2001), p. 19. Bricmont correctly stresses the point that probabilistic reasoning is no more falsifiable than deductive reasoning. Given a body of evidence, a probabilistic estimate is either rational or otherwise; of course the later addition of fresh evidence will generally lead to updating.

> ...probabilistic statements, understood subjectively, are forms of reasoning ...one cannot check them empirically. ...the main point of Bayesianism is to give rules that allow to update one's probabilistic estimates, given previous observations. p. 5.

This is not the place for an extended analysis of probability, and in particular of subjective Bayesianism[79], but I would like to draw attention to one aspect of the latter that I feel that Jaynes and Bricmont could, perhaps, have stressed more. It is that like all information-theoretic notions, subjective Bayesianism in practice relies on an arrow of time, on the difference between the past and the future, and on the crucial role of memory and records. In short, its application is, in an important sense, time-asymmetric. Recall Richard Feynman's 1965 definition of probability in physics as our estimate of likely frequencies in (Bernoulli trial) observations.[80] Feynman was explicit about the future nature of such observations, but surely in certain circumstances we might find ourselves estimating the likelihood of unknown past frequencies. The key point rather is that the estimation game is invariably based, partially if not entirely, on *some* knowledge of the present or past, and that any likelihoods have to be consistent with that knowledge. Gibbs may have been one of the first to note this time-asymmetry in probabilistic reasoning in physics:

> But while the distinction of prior and subsequent events may be immaterial with respect to mathematical fiction, it is quite otherwise with respect to events in the real world. It should not be forgotten, when our ensembles are chosen to illustrate the probabilities of events in the real world, that while probabilities of subsequent events may be often determined from probabilities of prior events, it is rarely the case that probabilities or prior events can be determined from those of subsequent events, for we are rarely justified in excluding from consideration the antecedent probability of the prior events.[81]

P. and T. Ehrenfest, and more recently Albert, have found this reasoning incomprehensible, but Lawrence Sklar, to his credit, did not. He thought it resonated with

> ...those doctrines that take the probabilities of the statistical theory to be subjective, the line being that knowing an event to have occurred gives it a subjective probability of one and precludes our assigning it some inferential probability on theoretical

[79]To my knowledge, the best concise defence of this view with a view to the physics readership is found in Bricmont (2001).

[80]Feynman *et al.* (1965), section 6-1; for further discussion see Brown (2011), section 3.

[81]Gibbs, reproduced in Sklar (1993), p. 58.

> grounds.[82]

This reasoning is indeed the basis of the doubt raised in section 4(iii) above concerning the widespread claim that Boltzmann's combinatorial argument for spontaneous equilibration, or Albert's variant of it, applies to the past as much as to the future. For the argument necessarily involves probabilities, and probabilities must surely reflect what Myrvold aptly calls the "asymmetry of epistemic access" to the past and future.[83]

But Sklar himself was unconvinced, claiming that inferences to past events are just as common as those to the future events. He considered a system, like a gas in a box, out of equilibrium, *whose past history is entirely unknown*, and argued that probabilistic inference would in this case suggest a past state "closer to equilibrium rather than farther away from it as we generally would, correctly, infer". Of course the subjectivist would claim that there is plenty of evidence based on observation of *other* similar systems that would block this false inference. How else do we know what the correct inference is? Sklar anticipates this response and argues:

> But that claim, of course, is the posit of irreversibility in statistical mechanics whose justification and explanation was what we wanted in the first place.

But is the claim any different from Albert's conditionalizing on "all we take ourselves to know of the world's entire macroscopic past history" when he applies his statistical posit to the present time (see section 4(i) above)?

Sklar goes on to write:

> ... it is hard to see how the entropic asymmetry itself can be thought to depend on any relativization of thermodynamic notions or their statistical surrogates to our objective states of knowledge combined with some given asymmetry of our knowledge of the world. . . . it is hard to fill in any subjectivist theory in such a way as to convince us that parallelism and asymmetry of entropy increase is only an asymmetry of our inferential applications of probability founded on asymmetry in our knowledge of past or future events.[84]

[82]Sklar (1993), p. 259. I take Bricmont's 2001 treatment (*op. cit.*) to be consistent with this position; see particularly his remarks on p. 8.

[83]See the thoughtful essay by Myrvold (2014), section 9. Note though that here Myrvold is considering the asymmetric nature of Gibbsean *equilibrium* probability distributions; it is not clear to me whether he intends the asymmetry to apply to the combinatorial argument for a system out of equilibrium.

[84]*Op. cit.*, p. 260.

And in the same vein, Albert excoriates any subjective interpretation of probabilities in statistical mechanics on similar grounds:

> Can anyone seriously think that our merely being *ignorant* of the exact micro conditions of thermodynamic systems plays some part in *bringing it about*, in *making it the case*, that (say) *milk dissolves in coffee?*[85]

Reading these remarks makes me understand the exasperation Jaynes showed at times in replying to his critics. *Who indeed in their right minds would think, or have thought, that the entropic arrow of time is ultimately explained by appeal to human ignorance?* It is worth noting, first, that Jaynes' focus, following that of Gibbs, is primarily to do with systems *in equilibrium*[86], and it is only successful because the large number of degrees of freedom associated with thermodynamic systems make the probability distributions for the relevant macroscopic properties enormously sharp.

> Evidently, such sharp distributions for macroscopic quantities can emerge only if it is true that for *each* of the overwhelming majority of those states to which appreciable weight is assigned, we would have the *same* macroscopic behaviour. We regard this, not merely as an interesting side remark, but as the essential fact without which matter would have no definite macroscopic properties, and experimental physics would be impossible. It is this principle of "macroscopic uniformity" which provides the objective content of the calculations, not the probabilities *per se*.[87]

Did Jaynes, or Gibbs for that matter, provide an explanation of *irreversibility*, of the non-decrease of entropy when the system is prodded into going adiabatically from one equilibrium state to another? Jaynes provided several explanations of the second law of thermodynamics, one close to the familiar combinatorial argument favoured by the neo-Boltzmannians,[88] and one, in a famous 1965 paper comparing Boltzmann entropy (actually, the negative of the H-function) and Gibbs' entropy,[89] that has left many readers,

[85] Albert (2000), ??. For similar remarks, see Wallace (2015a).

[86] van Lith correctly wondered what the significance is of the fine-grained Gibbs entropy for non-stationary distributions, or for stationary distributions other than Gibbsian ensembles; see van Lith (2001), p. 145.

[87] Jaynes (1957).

[88] *Op.cit.*.

[89] Jaynes (1965).

myself included, bemused. Whatever he was doing, Jaynes was not providing an explanation in the sense that Sklar is demanding. Indeed it was Sklar himself who noted that the heavy lifting in Jaynes' all-too-slick 1965 derivation of the second law of thermodynamics was done by his condition of "reproducibility", which effectively presupposes irreversible behaviour.[90] Note that it does so without any appeal to probabilistic considerations, subjective or otherwise. (Jaynes also seems to take it for granted that a spontaneous process resulting from removing a constraint from a system in equilibrium will lead to another equilibrium state, a process which is likewise irreversible.[91]) I think a generous reading of Jaynes' argument, or rather arguments, is that they attempt not to derive the second law, but to show that the law is consistent with the fact that the fine-grained Gibbs entropy is a constant of the motion. At any rate, it strikes me as grossly unfair to reject Jaynes' analysis of probabilistic inference in statistical mechanics on the grounds that it fails to provide something it never promises to provide in the absence of further dynamical considerations – an explanation of irreversibility.[92]

This brings us back to the contentious matter as to how an explanation can be provided by conferring the title "objective" to probability distributions, in the context of classical deterministic dynamics. Wallace is, unsurprisingly, sensitive to this issue. In his 2010 paper, he remains "neutral for now as to how these probabilities should be understood."[93] Early in his 2013 paper *Probability in Physics ...* he referred to the interpretation of the probability distributions in statistical mechanics as a "vexed question", and later in the paper wondered if they can somehow be interpreted in terms of objective chances, comparable perhaps to those involved in strictly stochastic dynamics. His discussion at this point is inconclusive.[94] At the end of the paper, he concluded that in both cases of classical deterministic and stochastic dynamics, "there are significant conceptual and philosophical problems in making sense of the notion of probability that is being used."[95]

[90] Sklar (1993), p. 258.

[91] See Brown and Uffink (2001).

[92] Note that Jaynes, although fully aware that it is not inevitable, at no point in the 1965 paper argues that the non-decrease of entropy is *probable* in adiabatic processes. Bricmont *op. cit.* does provide a probabilistic account of irreversibility, in keeping with his view that the probabilites are subjective, but the real work is being done by the dynamical details associated with certain idealised systems.

[93] Wallace (2010), section 6, p. 20.

[94] Wallace (2013).

[95] *Op. cit.* p. 23.

8 Quantum theory

An important part of Wallace's recent program involves the claim that not only is probability conceptually *less* problematic in quantum mechanics than in classical statistical mechanics, the quantum is the *ultimate source* of probabilities in the classical limit. In his quantum mechanical account of irreversibility, the analogue of the Simplicity requirement for initial conditions makes no appeal to probability distributions over states at all!

Wallace sees the entropic arrow of time emerging out of environment-induced decoherence in quantum mechanics as analogous to the forward macroscopic dynamics in classical mechanics; now the initial state of the system is a "Simple" quantum state. He leaves open the question as to whether this state is pure or represented by a non-idempotent statistical (density) operator; the critical point is that, unlike in his classical analysis, the Simple character of the state "is a constraint not on any probability distribution over initial states but on the actual initial state".[96] This leads Wallace to conclude that

> ... we should think of the classical limit of quantum mechanics as already being classical statistical mechanics (a point that has been stressed by Ballentine ...).

The idea then is that the mystery of the probability distribution in Wallace's classical contraption is solved by treating it as the classical limit of the appropriate quantum mechanical account of irreversibility, using an appeal to the objective probabilistic nature of the Simple states *themselves* in that account. I have my doubts.

8.1 The quantum Boltzmann equation

There is a significant recent literature dealing with the technicalities of how quantum systems interacting weakly with a secondary large system (heat bath) equilibrate and thermalize.[97] Essentially what is shown is that the density operator of the original system ends up (at least most of the time) in a "Gibbs" (alternatively "Boltzmann") thermal state, even when the initial state of the entire system – assumed to be isolated – is pure. Of course the von-Neumann entropy of the total system is a constant of the motion under unitary evolution (zero in the case of a pure state) and astonishingly

[96] Wallace (2013).

[97] See, e.g., Partovi (1989), Tasaki (1998), Maroney (2008), Linden *et al.* (2009), (2010), and Reimann (2010).

this has led some commentators to argue that an isolated quantum system never equilibrates.[98] This conclusion, if valid, would be disastrous for the theories of Albert and Wallace we have been discussing, but it is invalid for essentially the same reasons that the constancy of the fine-grained Gibbs entropy in classical statistical mechanics for a closed system fails to rule out spontaneous equilibration. Indeed, there has been significant recent work on the physics of "quenched", isolated, quantum many-body systems that describes irreversible behaviour over times short in comparison with the recurrence time, work which requires no Gibbsian ensembles and no appeal to coarse-graining. Spin-1/2 models have been developed with two-body interactions which describe real materials and which have also been simulated with optical lattices.[99] In quantum field theory, furthermore, there exist direct analogues of the classical Boltzmann equation and H-theorem for finite, isolated quantum fields involving interacting particles (field excitations), and it is this development that I want to concentrate on briefly.

Over the last thirty five years, it has been possible to test the the quantum Boltzmann equation (QBE) quantitatively in experiments on nonequilibrium systems. The ability to obtain numerical solutions of the QBE using iterative methods has resulted from the availability of cheap, fast computers, and the development of ultrafast optics methods has made observation possible of the distribution function of carriers in semiconductors, and some metals, out of equilibrium. The achievement of trapped cold atom gases has also allowed direct measurement of the distribution function of a gas.

In particular, a very good fit between experiment and theory was reported in 1991 by David Snoke and collaborators in the case of the semiconductor Cu_2O, where the energy distribution of excitons was measured at various times (at the picosecond scale) following the preparation of a non-equilibrium state using a laser pulse. The solution of the QBE for the time evolution of the population was obtained using a model based on exciton-phonon scattering.[100]

A brief and rather informal comparison of the QBE with the classical Boltzmann equation (CBE) will hopefully suffice to bring out the features that are relevant to our discussion.[101] Recall from section 5.4 that the CBE

[98] See Partovi *op. cit.* and Peres (1998), p. 267; for the classical analogue, see Blatt (1959).

[99] See, e.g. Torres-Herrera *et al.* (2016).

[100] For a review of experimental uses of the quantum Boltzmann equation, see Snoke (2011).

[101] For further details see Snoke *et al.* (2012).

establishes the rate of change of the velocity density function for the dilute gas; the contribution made by binary collisions is calculated on the basis of the SZA assumption, that at every instant the molecules about to collide are uncorrelated in velocity. In the case of a quantum field, it is assumed that the original state of the field is pure (but not necessarily a Fock state), and the rate of change of the expectation value of the number operator $\hat{N}_k$ associated with each mode $\vec{k}$ is calculated. If the state of the field evolves from $|\psi_i\rangle$ to $|\psi_t\rangle$ over some period of time t, then

$$\Delta\langle\hat{N}_k\rangle = \langle\psi_t|\hat{N}_k|\psi_t\rangle - \langle\psi_i|\hat{N}_k|\psi_i\rangle. \tag{2}$$

Consider the case analogous to the Boltzmann gas where the Hamiltonian for the system contains a nonlinear number-preserving interaction (mode-coupling) term which corresponds to the collision of two particles, applying destruction operators to two modes and creation operators to two other modes. The quantity $\Delta\langle\hat{N}_k\rangle$ is calculated using time-dependent perturbation theory, and it is assumed that "off-diagonal" terms of the form $\langle\psi_i|a_k^\dagger a_{k_3}^\dagger a_{k_2} a_{k_1}|\psi_i\rangle$, with all four $\vec{k}$'s different, are negligible (where $a_k^\dagger$ and a_k are the creation and annihilation operators associated with the mode $\vec{k}$, etc.)[102], an assumption referred to as "dephasing". It is also assumed, in analogy with Boltzmann's SZA, that expectation values factorize: $\langle\hat{N}_{k_1}\hat{N}_{k_2}\rangle = \langle\hat{N}_{k_1}\rangle\langle\hat{N}_{k_2}\rangle$.[103] Again, analogously to the Boltzmann case, it is assumed that energy states are close enough together to allow a sum over discrete $\vec{k}$ modes to be treated as an integral. If the state $|\psi_i\rangle$ is changing sufficiently slowly, then an expression for $\mathrm{d}\langle\hat{N}_k\rangle/\mathrm{d}t$ is obtained; this is the QBE. Given a standard quantum statistical mechanical formulation of the entropy (the analogue of minus Boltzmann's H functional):

$$S = -k_B \sum_k \left(\langle\hat{N}_k\rangle \ln\langle\hat{N}_k\rangle \mp (1 \pm \langle\hat{N}_k\rangle)\ln(1 \pm \langle\hat{N}_k\rangle)\right), \tag{3}$$

it follows from the QBE that S is non-decreasing in time (where the upper sign is for bosons and the lower sign is for fermions) if for every small successive interval dt, chosen such that the change in $\langle\hat{N}_k\rangle$ is small, the dephasing and factorization conditions are met. This is the quantum analogue of the Boltzmann H-theorem.

[102]This expression appears in the second order term in $\hbar$ for $\Delta\langle\hat{N}_k\rangle$; a similar assumption is also made for higher order terms.

[103]Snoke *et al.* (*op. cit.*) argue (obscurely in my opinion) that factorization holds "on average", given dephasing.

What the equilibrium distribution looks like on this account will be discussed in the next subsection. At this point it is worth summarising what the argument involves, in the words of Snoke *et al.*:

> We began with a closed, energy-conserving system, namely a quantum mechanical field Hamiltonian with no interactions with any external system, and deduced the expectation values of the many-body wave function as it evolves deterministically toward equilibrium according to the proper wave equation; we never invoked collapse, measurement, observation, or randomness. From beginning to end we have treated only the wave function, without invoking particles at all except to identify them as the natural energy eigenstates of the system. Yet we get an irreversible, deterministic approach to equilibrium.[104]

8.2 Probability again

A difficulty often pointed out in relation to Gibb's original treatment of equilibrium statistical mechanics, and its reformulation by Jaynes based on the maximum entropy principle, is the lack of a clear justification as to why the procedure works as well as it does. It is remarkable then that the canonical distribution for a quantum subsystem being thermalized by interaction with a heat bath can be obtained in quantum mechanics even when no considerations related to ensembles are introduced at the start of the analysis.

Wallace has correctly observed that the notion of fluctuations around equilibrium has different interpretations in the Gibbsian and Boltzmannian approaches to classical statistical mechanics. In the former, it is related to a probabilistic feature of the fictitious ensemble representing the system. In the latter, the consideration is dynamical: the phase point of the system will supposedly stray in and out of the equilibrium macrostate (i.e. largest according to the Liouville measure) in the course of the Hamiltonian flow, though quantitative details of the kind offered by the Gibbsian approach are lacking.[105] What Linden *et al.* stressed in 2010 is that in the quantum

[104] Snoke *et al.* (2012), pp. 1835-6. For the record, I do not find convincing the manner in which these authors respond in this paper to the Loschmidt reversibility objection, or the structural distinction they draw between their derivation of the QBE and that of the CBE. I am also concerned that the justification they provide in part V of their paper for the dephasing assumption may be inconsistent with the quantum recurrence theorem in the case of finite systems.

[105] Wallace (2015a).

mechanical analysis of the kind mentioned in the previous paragraph, for most of the time the state of the system is very close to the Gibbs state and hence almost static; the fluctuations are associated rather with quantum indeterminacies appearing in the standard "uncertainty" relations, and exposed in the act of measurement of the system.

Similar remarks apply to the distributions that arise from the QBE (as well as the fluctuations around equilibrium in the quantum quenching models mentioned earlier). The equilibrium state in the case of two-body elastic scattering above is that for which $\mathrm{d}\langle \hat{N}_k \rangle / \mathrm{d}t$ is zero. It can be shown that when $\langle \hat{N}_k \rangle$ is expressed as a function of energy, the equilibrium solution in the low particle density limit is the Maxwell-Boltzmann distribution.[106] This is not exactly the standard Maxwell-Boltzmann distribution; it is an expectation value distribution and allows for fluctuations. (The same applies to the related derivation of the Planck distribution in the case of electron-photon interactions.[107])

So let us return to Wallace's analysis of macro-irreversibility in quantum theory. He states that:

> ... the success of classical statistical mechanics gives us no particular reason to make the statistical move in the quantum case. ... debates about the nature of classical statistical-mechanical probability are not of direct relevance to our understanding of the actual world as described by contemporary physics. Probability in contemporary physics arises from the probabilistic nature of quantum theory itself, not from any additional posit.[108]

I think this conclusion is uncontestable, but the question I am interested in here is whether contemporary physics is relevant to our understanding of classical statistical-mechanical probability. The considerations in the previous two paragraphs endorse the Ballentine-Wallace claim (see above) that the classical limit of quantum mechanics is classical statistical mechanics, but they refer to the equilibrium case. Regarding non-equilibrium behaviour, I do not see that Wallace's own Simple Past Hypothesis in the classical setting is in any straightforward sense the classical limit of the QBE or for that matter any of the familiar decoherence mechanisms in the literature. The question hinges on whether the initial Simple quantum state is in itself a

[106] This distribution will also be obtained in cases involving other interactions, such as electron-phonon scattering, or exciton-phonon scattering in semiconductors; see Snoke *et al.* (2012).

[107] *Op. cit.*

[108] Wallace (2013).

probabilistic entity. My own view within the (fundamentally deterministic) Everett picture is that such a density operator – idempotent or otherwise – is no more intrinsically a carrier of probability than is the Liouville measure on the classical phase space. Probabilities in my book come into being when a decision-theoretic agent is confronted with the prospect of branching in a measurement process, and can only make sense in a world endowed with a macroscopic arrow of time and with agents who remember the past and not the future.[109] This view implies that quantum probabilities make no appearance at the start of the world, but are forced on us at the later times at which observations are made and rational agents have to bet on their outcomes. I am thus led to doubting whether the initial "vexed" probability distributions in Wallace's classical Simple Past Hypothesis find any clarification in contemporary quantum physics, as well as doubting whether the Gibbsian coarse-graining approach to macro-irreversibility is strictly needed anyway.

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[109] I fear space does not permit a systematic defence of this view; my own thoughts on the matter, which question the existence of objective chances in the world, are partly spelt out in Brown (2011).

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