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PHILOSOPHY OF TIME

Edited by

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CHAPTER 10

TIME IN THERMODYNAMICS

JILL NORTH

1. INTRODUCTION

OR better: time *asymmetry* in thermodynamics. Better still: time asymmetry in thermodynamic phenomena. "Time in thermodynamics" misleadingly suggests that thermodynamics will tell us about the fundamental nature of time. But we don't think that thermodynamics is a fundamental theory. It is a theory of macroscopic behavior, often called a "phenomenological science." And to the extent that physics can tell us about the fundamental features of the world, including such things as the nature of time, we generally think that only fundamental physics can. On its own, a science like thermodynamics won't be able to tell us about time per se. But the theory will have much to say about everyday processes that occur in time, and in particular, the apparent asymmetry of those processes. The pressing question of time in the context of thermodynamics is about the asymmetry of things *in* time, not the asymmetry *of* time, to paraphrase Price (1996, 16).

I use the title anyway, to underscore what is, to my mind, the centrality of thermodynamics to any discussion of the nature of time and our experience in it. The two issues—the temporal features of processes in time, and the intrinsic structure of time itself—are related. Indeed, it is in part this relation that makes the question of time asymmetry in thermodynamics so interesting. This, plus the fact that thermodynamics describes a surprisingly wide range of our ordinary experience. We'll return to this. First, we need to get the question of time asymmetry in thermodynamics out on the table.

2. THE PROBLEM

The puzzle that I want to focus on here, the puzzle that gets to the heart of the role of thermodynamics in understanding time asymmetry, arises in the foundations of physics, and has implications for many issues in philosophy as well. The puzzle comes up in connection with the project of explaining our macroscopic experience with micro-physics: of trying to fit the macroscopic world of our everyday lives onto the picture of the world given us by fundamental physics. The puzzle has been debated by physicists and philosophers since the nineteenth century. There is still no consensus on a solution.

Here is the problem. Our everyday experience is largely of physical processes that occur in only one direction in time. A warm cup of coffee, left on its own in a cooler room, will cool down during the day, not grow gradually warmer. A box of gas, opened up in one corner of a room, will expand to fill the volume of the room; an initially spread-out gas won't contract to one tiny corner. A popsicle stick left out on the table melts into a hopeless mess; the hopeless mess sadly won't congeal back into the original popsicle.[1]

While we would be shocked to see the temporally reversed processes, the familiar ones are *so* familiar that they hardly seem worth mentioning. But there is a problem lurking. The problem is that the physical laws governing the particles of these systems are symmetric in time. These laws allow for the time reversed processes we never see, and don't seem capable of explaining the asymmetry we experience.

Suppose I open a vial of gas in a corner of the room. The gas will spread out to fill the room. Take a film of this process, and run that film backward. The reverse-running film shows an initially spread-out gas contract to one corner of the room. This is something that we never see happen in everyday life. Yet this process, as much as the original one, evolves with the fundamental dynamical laws, the laws that govern the motions of the particles in a system like this. Consider it in terms of Newtonian mechanics. (For ease of exposition, I stick to classical mechanics. Assume this unless explicitly stated otherwise.) The dynamical law of this theory is $\mathbf{F} = m\mathbf{a}$. Now, the gas just *is* lots of particles moving around in accord with this law. And the law is time reversible: it applies to the reverse-running film as much as to the forward-playing one. Intuitively, the law does not contain any direction of time in it. We can see this by noting that each quantity in the law has the same value in both films: in the backward film, the forces between the particles are the same as in the forward film (these forces are functions of the particles' intrinsic features and their relative spatial separations); their masses are the same; and their accelerations (the second time derivatives of position) are the same. For any process that evolves with this law, the time reversed process—what we would see in a reverse-running film—will also satisfy the law.

[1] A video, with background music: http://www.youtube.com/watch?v=tPFqBrC9ynE.

This is, roughly, what we mean when we say that the laws governing a system's particles are time reversal symmetric, or time reversal invariant. The movie-playing image brings out an intuitive sense of time reversal symmetry. On a time reversal invariant theory, the film of any process allowed by the laws of the theory, run backward, also depicts a process that obeys the laws. (A bit more on this in the next section.)

In this sense, the classical laws are time reversal symmetric. Run the film of an ordinary Newtonian process backward, and we still see a process that is perfectly in accord with the Newtonian dynamical laws.[2] This time reversal symmetry isn't limited to classical mechanics, either. For (exceptions and caveats aside for now[3]) it seems that all the plausible candidates for the fundamental dynamical laws of our world are temporally symmetric in just this way.

But then there is nothing in the relevant physics of particles to prohibit the spontaneous contracting of a gas to one corner of the room, or the warming up of a cup of coffee, or the reconstitution of a melted popsicle—what we would see in reverse-running films of the usual processes. And yet we never see these kinds of things happen. That's puzzling. Why don't we ever see boxes of gas and cups of coffee behave like this, if the laws governing their particles say that it is possible for them to do so?

It's not that we never see the temporally reversed phenomena, of course. We do see water turning to ice in the freezer, coffee heating up on the stove, melted popsicles re-congealing in the fridge. But these processes are importantly different from the time reverse of the usual phenomena. These processes differ from what we would see in the reverse-running films of ice melting, coffee cooling, and popsicles dripping. For they require an input of energy. (We can formulate the asymmetry as the fact that energetically isolated systems behave asymmetrically in time.[4])

More, it seems to be a lawlike fact that popsicles melt and gases expand to fill their containers. These generalizations support counterfactuals, they are used in successful explanations and predictions, and so on. They seem to satisfy any criteria you like for lawfulness; they surely don't seem accidental. Think of how widespread and reliable they are.

And, in fact, there is a physical law that describes these processes: the second law of thermodynamics. This law says that a physical quantity we can define for all these systems, the entropy, never decreases. We'll return to entropy in section 4. For now, note that this is a time asymmetric law: it says that different things are possible in either direction of time. Only non-entropy-decreasing processes can happen in the direction of time we call the future.

It turns out that ordinary processes like the expansion of gases and the melting of popsicles are all entropy-increasing processes. All the processes mentioned so far are

[2] See Feynman (1965, ch. 5) and Greene (2004, ch. 6) for accessible discussion.

[3] Caveats in the next section; an exception in 6.2.

[4] One approach to the puzzle, which I do not discuss here, exploits the fact that typical systems are not in fact energetically isolated. This approach faces similar questions about the asymmetry of the influences themselves: Sklar (1993, 250–254); Albert (2000, 152–153). But see Earman (2006, 422) for a recent suggestion along these lines.

characterized by the science of thermodynamics; their time asymmetry, in particular, is characterized by the second law of thermodynamics. This law seems to capture the very asymmetry we set out to explain. Does this then solve our initial puzzle?

No. If anything, it makes the problem starker. The question now is where the asymmetry of the second law of thermodynamics comes from, if not from the underlying physical laws. The macroscopic systems of our experience consist of groups of particles moving around in accord with the fundamental physical laws. Our experience is of physical processes that are due to the motions of the particles in these systems. Where does the asymmetry of the macroscopic behavior come from, if not from the motions of systems' component particles? Consider that if we watch a film of an ice cube melting, we can tell whether the film is being played forward or backward. But we won't be able to tell which way the film is playing if we zoom in to look at the motions of the individual molecules in the ice: these motions are compatible with the film's running either forward or backward.[5]

Where does the observed temporal asymmetry come from? What grounds the lawfulness of entropy increase, if not the underlying dynamical laws, the laws governing the world's fundamental physical ontology? Can we can explain the asymmetry of thermodynamics, and of our experience, by means of the underlying physics?

3. INTERLUDE: TIME REVERSAL INVARIANCE

The puzzle of time asymmetry in thermodynamics stems from the temporal symmetry of the fundamental physical laws. Before moving on, a caveat and a related issue.

Caveat. We now have experimental evidence that there is a fundamental, lawful time asymmetry in our world. Given the CPT theorem, the observed parity violations in the decay of neutral (chargeless) k_0-mesons implies a violation of time reversal symmetry. Yet it is widely thought that these violations are too small and infrequent to account for the widespread macroscopic asymmetries of our experience and of thermodynamics.[6] I set this aside here.

A related issue, also interesting and important, but also to be set aside here. Whether the laws are time reversal invariant is the subject of recent debate, for two reasons.

First, what quantities characterize a system's fundamental state at a time is subject to debate. Take Newtonian mechanics. Think of a film of a Newtonian process, such as a baseball flying through the air along a parabolic trajectory. Run this film backward, and we seem to have a process that also evolves with Newton's laws. We see baseballs fly through the air in opposite directions, obeying the laws of

[5] The example is from Feynman (1965, 111–112).

[6] See Sachs (1987, ch. 9) for a summary of the empirical evidence. For dissent on the conclusiveness of this evidence, see Horwich (1987, 3.6). See Arntzenius (2010) for more on time and the CPT theorem.

physics, all the time. However, the reverse-flying ball only obeys the physics supposing that we invert the directions of the velocities at each instant in the time reversed process. Otherwise, we get a process in which, at any given instant, the ball at a later instant will have moved in the opposite direction to that in which its velocity had been pointing previously. If we simply take the time reversed sequence of instantaneous states—if we run the sequence of film frames in reverse temporal order—and those states (movie frames) include particle velocities, then not even Newtonian mechanics would be time reversal symmetric: the time reversed process would not be possible.

In other words, if our time reversal operator—the mathematical object we use to figure out whether a theory is time reversal symmetric—only inverts sequences of instantaneous states, and those states include velocities, then Newtonian mechanics will not be symmetric under the time reversal operation. In general, we apply an operator to a theory to learn about the symmetry of the theory and the world it describes. We compare the theory with what happens to it after undergoing the operation. If the theory is the same afterward, then it is symmetric under the operation: we say that the theory is invariant under the operation. And surely Newtonian mechanics is time reversal invariant, if any theory is. Newtonian mechanics, that is, should be symmetric under the time reversal operator.[7] This theory doesn't seem to indicate any asymmetric temporal structure in the worlds it describes.

Physics texts respond by allowing the time reversal operator to act on the instantaneous states that make up a given time reversed process. In Newtonian mechanics, for example, the standard time reversal operator does not just invert the time order of instantaneous states; it also flips the directions of the velocities within each instantaneous state. Then a time reversal invariant theory is one on which, for any process allowed by the theory, the reverse sequence of *time reversed* states is also allowed. This is a slightly different understanding of time reversal symmetry from the intuitive, movie-playing idea. Still, all the candidate fundamental theories (with an exception to come later, and aside from the caveat mentioned above) are time reversal symmetric, on this understanding.

This raises a question. Should we allow time reversal operations on instantaneous states, and if so, which ones? We cannot allow any old time reversal operators, else risk our theories' trivially coming out time reversal symmetric.[8]

An alternative view, advocated by Horwich (1987) and Albert (2000, ch. 1), holds that particle velocities aren't intrinsic features of instantaneous states to begin with. If velocities aren't included in the fundamental state of a system at a time, then when we line up the instantaneous states in reverse temporal order, we won't get the problem that we did above for Newtonian mechanics, the problem that motivated us to move to a slightly different time reversal operation. (Instead, velocities

[7] For disagreement on this view of classical mechanics, see Hutchison (1993, 1995); also Uffink (2002). Savitt (1994); Callender (1995) are replies.

[8] See Arntzenius (2004, 32–33).

will invert when we take the reverse sequence of position states.) On this view, for any sequence of states allowed by a time symmetric theory, the inverse sequence of the *very same states* is also allowed. Newtonian mechanics, for example, is time reversal invariant. But there are some radical consequences of this view; for example, it loses the time reversal invariance of other theories that are standardly taken to be invariant.[9]

(It is interesting to think of how the debate will go for other formulations of the dynamics. There has not been as much discussion of this.[10] Presumably, equivalent formulations of a theory should all be invariant, or not, with respect to any given operation. For a theory's (non-)invariance indicates (a)symmetries in the world it describes. And different, equivalent formulations of a theory should describe the same set of possible worlds. Thus, take the Hamiltonian formulation of classical mechanics. The equations of motion are $\frac{\partial H}{\partial p} = \frac{dq}{dt}$ and $\frac{\partial H}{\partial q} = \frac{-dp}{dt}$. On the standard view, this theory is time reversal invariant just in case H is invariant when $p \rightarrow^T -p(-t)$ and $q \rightarrow^T q(-t)$, where T is the time reversal operator. Why these time reversal properties for the momentum and position coordinates? A common suggestion is by analogy to the Newtonian case: there, we inverted velocities under time reversal; here, we invert the momenta. This suggestion, however, faces Arntzenius' (2004) challenge to justify the time reversal operators used in Newtonian mechanics. Note that it seems that someone with Albert's view can also say that Hamiltonian mechanics is time reversal invariant, but for interestingly different reasons. Albert can get this result by taking the second equation of motion above to be a definition of momentum rather than an additional fundamental law. Albert only allows time reversal operations to act on non-fundamental quantities that are the time derivatives of fundamental quantities; for example, in Newtonian mechanics, particle velocities invert because sequences of positions do. By taking momentum to be defined by this law, it becomes a non-intrinsic, non-fundamental quantity, which then inverts under time reversal because sequences of positions do.)

A second reason that the time reversal symmetry of the laws is subject to recent debate is that the proper action of the time reversal operator is under debate. (A third reason, whether a particular theory of quantum mechanics is correct, will be discussed in section 6.2.) Setting aside their differences on the intrinsic properties of instantaneous states, the above views all agree that the basic action of the time reversal operator is to invert the time order of a sequence of states. But whether this is the proper action for the time reversal operator is debatable. A different notion of time reversal, as an inverting of the temporal orientation, was recently proposed by Malament (2004), and is defended by North (2008).

[9] On this debate, see Earman (1974, 2002); Horwich (1987, ch. 3); Albert (2000, ch. 1); Callender (2000); Arntzenius (2000, 2003); Smith (2003); Malament (2004). See Arntzenius (1997*a*) for time reversal invariance of indeterministic theories. See Arntzenius and Greaves (2009) for time reversal and quantum field theory.

[10] Some discussion of this can be found in Arntzenius (2000); Uffink (2002).

4. Trouble in Thermodynamics

Thermodynamics was originally developed in the nineteenth century, by figures such as Carnot, Clausius, and Thomson (Lord Kelvin), as an autonomous science, without taking into account the constituents of thermodynamic systems and the dynamics governing those constituents.[11] The original developers of the theory didn't try to explain thermodynamics on the basis of anything more fundamental. In particular, the puzzle of thermodynamic asymmetry was not yet recognized.[12]

That changed with the advent of the atomic hypothesis, and the identification of various thermodynamic quantities with properties of systems' particles.[13] Given the atomic make-up of matter, and given the successful identification of macroscopic properties such as average temperature, pressure, and volume, with properties of groups of particles, the question arises as to where the asymmetry of macroscopic behavior comes from.

Statistical mechanics is the physical theory applying the fundamental dynamical laws to systems with large numbers of particles, such as the systems studied in thermodynamics. (Statistical mechanics adds some probability assumptions to the fundamental dynamics, more on which below.) So the question is whether we can explain thermodynamics on the basis of statistical mechanics, and in particular, whether we can locate a statistical mechanical grounding of the second law.[14]

The work of Maxwell, Boltzmann, and Gibbs, among others, led to key components of an answer. Each of them had their own approach to statistical mechanics and the explanation of entropy increase. Since there remains disagreement on the proper understanding of statistical mechanics, there remains disagreement on the proper grounding of thermodynamics. In what follows, I use a Boltzmannian

[11] Fermi (1956) is a readable book on thermodynamics. Here is Fermi on the autonomy: in thermodynamics, the "laws are assumed as postulates based on experimental evidence, and conclusions are drawn from them without entering into the kinetic mechanism of the phenomena" (1956, *x*).

[12] There are many issues that I set aside here. Throughout, I stick with the modern formulation of the second law in terms of entropy. For discussion of the historical development of thermodynamics and of other aspects of the theory, see Sklar (1993); Uffink (2001); Callender (2001, 2008*b*); and references therein. Later axiomatizations of thermodynamics, beginning with Carathéodory in 1909, rigorized the theory; Lieb and Yngvason (2000) explains a recent version. Sklar (1993); Hagar (2005); Uffink (2007); Torretti (2007) (see also references therein) contain surveys of the development of, and different approaches to, both statistical mechanics and thermodynamics. See Earman (1981, 2006); Liu (1994) on the question of a relativistic thermodynamics. On entropy in quantum statistical mechanics, see Hemmo and Shenker (2006); Campisi (2008).

[13] On whether this constitutes a reduction of thermodynamics, see Sklar (1993, ch. 9); Callender (1999); Hellman (1999); Yi (2003); Batterman (2005; 2010); Lavis (2005).

[14] Ehrenfest and Ehrenfest (2002) is a classic text on the development and foundations of statistical mechanics. See also Sklar (1973, 1993, 2000, 2001, 2007); O. Penrose (1979); Uffink (1996, 2007); Frigg (2008*a*,*b*). A different approach is discussed in Liu (2001). Wallace (2002); Emch (2007) discuss issues in the foundations of quantum statistical mechanics. Some textbooks on statistical mechanics, all with varying approaches: Khinchin (1949); Prigogine (1961); Tolman (1979); Landau and Lifshitz (1980); Pathria (1996); O. Penrose (2005*a*).

approach. Gibbs' statistical mechanics can be adapted to the discussion (and according to some, it must be); the main difference lies in the conception of entropy. I will have to stick to one version here, and choose Boltzmann's out of my own views on the matter.[15]

Boltzmann's key insights were developed in response to the so-called reversibility objections (of Loschmidt and Zermelo).[16] The objection, in a nutshell, is the one that we have already seen. If the laws governing the particles of thermodynamic systems are symmetric in time, then entropy increase can't be explained by these laws. Think of any system that has been increasing in entropy, such as a partly melted ice cube, and imagine reversing the velocities of all its particles. The determinism[17] and time reversal invariance of the dynamics entail that the system will follow the opposite time development: the system will decrease in entropy, becoming a more frozen ice cube. (Another reversibility objection, with a similar conclusion for the inability of the dynamics to ground entropy increase, employs Poincaré recurrence.) The time reversed, anti-thermodynamic behavior is just as allowed by the physics of the particles.

The first part of a reply is this. Boltzmann and others realized that, given the time reversal invariance and determinism of the underlying dynamical laws, the second law of thermodynamics can't be a strict law. It must instead be a *probabilistic* law. Entropy decrease is not impossible, but extremely unlikely.

To understand the move to a probabilistic version of the second law, Maxwell's thought experiment is illuminating.[18] Imagine that a demon, or a computer, controls a shutter covering an opening in a wall that divides a box of gas. The gas on one side of the divider is warmer than the gas on the other side. Now, the average temperature in the gas is a function of the mean kinetic energy of its molecules. Within the warmer portion of the gas, then, there will be molecules that are moving, on average, slower than the rest of the molecules. Within the cooler portion of the gas, there will be molecules that are moving, on average, faster than the rest of the molecules. Suppose that the shutter is opened whenever a slower-on-average molecule within the warmer gas moves near the opening, sending that molecule into the cooler gas, and whenever a faster-on-average molecule within the cooler gas moves near the opening, sending that

[15] For more on Gibbs' approach, see Gibbs (1902); Ehrenfest and Ehrenfest (2002); Sklar (1993); Lavis (2005, 2008); Earman (2006); Pitowsky (2006); Uffink (2007). For arguments in favor of Boltzmann's, see Lebowitz (1993*a*,*b*,*c*, 1999*a*,*b*); Bricmont (1995); Maudlin (1995); Callender (1999); Albert (2000); Goldstein (2001); Goldstein and Lebowitz (2004); against the approach, see Earman (2006). On information-theoretic notions of entropy (especially in relation to Maxwell's demon), see Earman and Norton (1998, 1999); Bub (2001); Weinstein (2003); Balian (2005); Maroney (2005); Norton (2005); Ladyman *et al.* (2007, 2008); and references therein.

[16] On the history of the debate over the reversibility objections, see Brush (1975).

[17] I set aside the cases of indeterminism in classical mechanics: see Earman (1986); Norton (2008); Malament (2008).

[18] More, the thought experiment suggests that Maxwell recognized the reversibility problem, and the probabilistic version of the second law, sooner than did Boltzmann: Earman (2006).

molecule into the warmer gas. The net result is that the two gases will become *more* uneven in temperature, against the second law of thermodynamics.[19]

Intuitively, this separation in temperature could happen. It could even happen on its own, just by accident. Imagine that there is no shutter, just an opening in the middle of the divider. The particles could just happen to wander through the hole at the right times to cause the gas to grow more uneven in temperature. This seems extremely unlikely; but it also seems possible. The fundamental laws governing the molecules of the gas do not prohibit this from happening.

Still, it would take a massive coincidence, an extremely unlikely coordination among the motions of all the molecules in the gas. In other words: the second law of thermodynamics—the tendency of systems to increase in entropy, such as the tendency of a gas to even out in temperature—holds probabilistically. Entropy decrease is possible, but unlikely. Indeed, given the huge numbers of particles in typical thermodynamic systems, and given the extent of the coordination among their motions that would be required, entropy decrease is *extremely* unlikely.

The probabilistic understanding of the second law is a first step toward a solution to the puzzle of time asymmetry in thermodynamics. But it is only a first step (albeit a very large one). For the reversibility objections apply just as much to the probabilistic version of the second law as to the non-probabilistic one. The probabilistic version of the law says that entropy decrease, while not impossible, is extremely unlikely. Given the time reversal symmetry of the underlying laws, though, where does *this* asymmetry come from? Why isn't entropy extremely unlikely to decrease in either direction of time? If it's so unlikely that my half-melted popsicle will be more frozen to the future, then why did it "unmelt" to the past? Remember, the temporally symmetric laws don't make any distinction between the past and future time directions. If I feed the current state of my half-melted popsicle into those laws, then how do the laws tell the particles to increase in entropy to the future and not to the past? The laws don't even pick out or *mention* the future as opposed to the past!

Where in the world *is* thermodynamic asymmetry?

5. Statistical Mechanics

In order to make progress at this point, we need a more detailed understanding of the statistical mechanical basis of the second law.

This takes some setting up. A typical thermodynamic system will have a large number of particles. We can specify the state of such a system by means of its macroscopic features, such as its average temperature, pressure, and volume. These features pick out the *macrostate* of the system. Another way to specify the system's state is by means

[19] This can arguably be done without doing any work. Whether or not a genuine Maxwell's demon is possible is a matter of continuing debate. See, for example, Earman and Norton (1998, 1999); Albert (2000, ch. 5); Callender (2002); Norton (2005); and references therein.

of the fundamental states of its constituent particles. This gives the system's *microstate*, its most precisely specified state, in terms of the positions and velocities and types of each of its particles. In general, corresponding to any macroscopically specified state, there will be many different compatible microstates, many different arrangements of a system's particles that give rise to the same set of macroscopic features.

Think of this in terms of phase space. The phase space of a system is a mathematical space in which we represent all its possible fundamental states. For a classical system with n particles, the phase space has $6n$ dimensions, one dimension for the position and velocity of each particle, in each of the three spatial directions. (The phase space has dimension $2nr$, where n is the number of particles and r is the number of degrees of freedom, here assumed to be the three dimensions of ordinary physical space.) Each point in phase space picks out a possible microstate for the system; a curve through the space represents a possible micro-history. A macrostate corresponds to a region in phase space, each point of which picks out a microstate that realizes the macrostate.

In the phase space of a gas, for example, each point represents a different possible way for the particles in the gas to be arranged, with different positions and velocities. Think of a macrostate of the gas, say the one where it fills half the room. Think of all the different ways the particles could be arranged to yield a gas that fills this volume of the room. Swap some of their positions, or change a few of their velocities, and we still get a gas that fills this volume of the room. These changes amount to picking out a different point in the phase space of the gas, consistent with its filling this volume of the room. The region comprising all those points represents the macrostate in which the gas fills half the room.[20]

Now we can get more precise about Boltzmann's insight. Boltzmann showed that the thermodynamic entropy, S, of a given system, the same entropy appearing in the second law of thermodynamics, is a function of how many arrangements of the system's particles are compatible with its macrostate. He found that $S = k \log n$ (up to additive constant), where n is the "number" of microstates consistent with the macrostate, and k is a constant. Here n is the "number" of particles in the sense of the size of the region in phase space that the corresponding macrostate takes up—the region's volume, on the standard measure.[21] This quantity has been empirically determined to track the thermodynamic entropy. Boltzmann thus arguably discovered a statistical mechanical correlate of thermodynamic entropy, just as had been done for other thermodynamic quantities, such as the identification of average temperature

[20] I skirt over details about how to divide up, or coarse-grain, the phase space: see Sklar (1993); Albert (2000, ch. 3); Earman (2006); Uffink (2007).

[21] The standard measure in classical statistical mechanics is the Liouville volume measure: the standard Lebesgue measure defined over the canonical coordinates. Boltzmann's equation is thus $S = k \log |\Gamma_M|$, where $|\Gamma_M|$ is the standard (normalized) volume of the phase space region corresponding to the macrostate M. Typically, the constant energy E is one of the macro constraints; in which case we use the volume induced by the standard measure on the $6n - 1$-dimensional energy hypersurface. A bit more on this later.

with the mean kinetic energy of a system's particles, or of average pressure with the rate and force of particle collisions with a system's container.[22]

Boltzmann's equation tells us that macrostates compatible with many distinct microstates—macrostates that can be formed by many different arrangements of a system's particles, macrostates that correspond to large regions of a system's phase space—have higher entropy than macrostates compatible with fewer microstates. Basic combinatorics shows that these macrostates have *overwhelmingly* higher entropy. Thus, spread-out, uniform, even-temperature macrostates have overwhelmingly higher entropy than concentrated, unevenly distributed ones. Think of the gas in the room. Intuitively, there are many more ways for the gas' particles to be arranged so that the gas is spread out in the room than concentrated in one tiny part: there are many more microstates compatible with the spread-out macrostate. This is reflected in the difference in entropy. The state in which the gas has spread out to fill the room has a much higher entropy than the state in which it is concentrated in one corner. The equilibrium macrostate, the one in which the gas has stabilized to fill the volume of the room, has the overwhelmingly highest entropy.

So higher entropy macrostates have many, many more distinct possible microstates than do lower entropy macrostates. Boltzmann showed this to be the case for all thermodynamic systems.

This suggests that we can understand entropy increase as the progression toward more and more probable macrostates. Add to our theory a natural-seeming probability assumption, that a system is as likely to be in any one of its possible microstates as any other—that is, place a uniform probability distribution, on the standard measure, over the phase space region corresponding to the system's macrostate—and we get that high entropy, large-volume-occupying macrostates are overwhelmingly more *probable* than low entropy, small-volume ones.[23] At any time, a system is overwhelmingly likely to evolve to a microstate realizing a macrostate that takes up a larger phase space region (or to stay in its current macrostate if it is already at equilibrium), the very higher entropy macrostate that thermodynamics says it should evolve into. This is because, according to the particle dynamics and the uniform probability measure, there are overwhelmingly more such states for the system to be in.

Have we finally found the statistical mechanical grounding of thermodynamics? Have we managed to derive the probabilistic version of the second law from the

[22] Rather, for a system whose microstate realizes the equilibrium macrostate, Boltzmann saw that this quantity agrees with the thermodynamic entropy defined for systems at equilibrium. Boltzmann then extends this notion of entropy to systems not at equilibrium too; indeed, that is one of the reasons to prefer his notion to Gibbs': Callender (1999); Lebowitz (1999*b*); Goldstein and Lebowitz (2004). Though whether it can be so extended is a matter of continuing debate.

[23] Where this probability distribution comes from is another large subject of debate. See Sklar (1973, 1993, 2001, 2007); Jaynes (1983); Lebowitz (1993*b*); Bricmont (1995); Strevens (1998, 2003); Callender (1999); Albert (2000, ch. 4); Goldstein (2001); Loewer (2001, 2004); Wallace (2002); North (2004, ch. 3; 2010); (2010); Goldstein and Lebowitz (2004); Lavis (2005, 2008); Earman (2006); Maudlin (2007*b*); Frigg (2008*a*).

underlying dynamics and the statistical postulate (as it is often called) of uniform probabilities over the microstates compatible with a system's macrostate?

Well, no. But it turns out to be a big start.

We know that this can't be enough to ground the thermodynamic asymmetry, for the now-familiar reason that the dynamical laws are time reversible. Take any entropy-increasing microstate compatible with a system's macrostate—any microstate for which, if the system starts out in it, the dynamical laws predict that it will (deterministically, on the classical dynamics) increase in entropy—and there will be another, entropy-*decreasing* microstate compatible with that macrostate: just reverse all the particle velocities. There is a one-one mapping between microstates and their time reverses. And for any microstate that is compatible with a given macrostate, so is its time reverse. So there will be just as many entropy-increasing as entropy-decreasing ways for the system to evolve from its current state. But then entropy increase can't be any more likely than entropy decrease.

In other words, the uniform probability distribution, combined with a dynamical law like $\mathbf{F} = m\mathbf{a}$, will predict overwhelmingly likely entropy increase to the future of any thermodynamic system. That is all to the good. But the uniform probability distribution, combined with the dynamics, also says that any system is overwhelmingly likely to increase in entropy to the *past*. That is decidedly *not* to the good. It is contrary to the second law of thermodynamics—not to mention most of our ordinary experience. We remember the coffee having been warmer than the room, the gas having been more concentrated, the popsicle more frozen. These are the mundane observations that got us going along this puzzle-solving path in the first place! Although the elements of our theory thus far predict what we expect for these systems' future behaviors, they radically contradict what we take to be the case for their pasts. Hence the depth of our problem. Statistical mechanics seems to make predictions that are radically falsified by our ordinary experience and by the evidence we have for the second law of thermodynamics.

Our problem goes deeper still. If statistical mechanics says that the past was radically different from what our current evidence suggests, then this undermines the very evidence we have for the physics that got us into this mess! Take the current macrostate of the world, a uniform probability distribution over its compatible microstates,[24] and the dynamics governing the world's particles. These are the elements of our statistical mechanical theory as it now stands. In the overwhelming majority of these microstates, the world increases in entropy to the future; but likewise, in the overwhelming majority of these microstates, the world increases in entropy to the past. In other words, the overwhelming majority of possible micro-histories for our world are ones in which the records we currently have are not, in fact, preceded by the events that they seem to depict. It is overwhelmingly *more* likely that the current state of the world, apparent

[24] Alternatively, take the macrostate of any given sub-system and a uniform distribution over its possible microstates. Whether the theory can be applied to the world or universe as a whole remains contentious. For a particularly forceful contention, see Earman (2006).

records and all, spontaneously fluctuated out of a past equilibrium state, than that our current records are veridical accounts of the world's having evolved from an extremely unlikely low entropy state. Statistical mechanics deems it extremely unlikely that any of our evidence for the world's lower entropy past is reliable. And that is the very evidence we have for the dynamical laws and the uniform distribution in the first place. Not only does this undermine the asymmetry of thermodynamics, but it undermines all of the evidence we have for thermodynamics, not to mention the rest of physics. We have on our hands the threat of "a full-blown skeptical catastrophe" (Albert, 2000, 116).[25]

At this point, it may be surprising to hear that we have made any headway; but indeed we have. The work done by Boltzmann and others in the foundations of statistical mechanics suggests that we can conclude this much: for any given macrostate, the overwhelming majority of its compatible microstates are those for which, if the system were in it, the system would (deterministically) increase in entropy. So we can reasonably infer entropy increase to the future, just as our experience leads us to expect.

The problem is that we can just as reasonably infer entropy increase to the past. That is the problem we now have to solve.

6. Different approaches

In order to explain thermodynamics, we will need a temporal asymmetry somewhere in our fundamental theory. As Price (1996) emphasizes: no asymmetry in, no asymmetry out.[26] The question is where. Answers can generally be divided into one of two camps: an asymmetry in boundary conditions or an asymmetry in the dynamics. Within each of these camps, there are differing approaches. I survey here a few of the representative and, to my mind, most promising. For discussion of other approaches (interventionism, expansion of the universe, others), I refer the reader to the references cited here; for comprehensive overviews, see especially Sklar (1993); Price (1996); Uffink (2007); Frigg (2008*b*).

6.1 Boundary conditions

Recall the problem we have now gotten ourselves into. If Boltzmann's reasoning explains overwhelmingly likely entropy increase to the future, then why isn't entropy just as likely to increase to the past?

[25] See Albert (2000, ch. 6) for more on the problem of records and the solution to it discussed below. Earman (2006) argues against both the apparent problem and this solution.

[26] See Price (1996) for arguments that many accounts can be faulted for smuggling in unwarranted asymmetric assumptions.

Another way of seeing the problem is emphasized by Albert (2000, ch. 4) in order to motivate the move to asymmetric boundary conditions. Take a partly melted popsicle. A uniform probability distribution over the microstates compatible with its macrostate, when combined with $\mathbf{F} = m\mathbf{a}$, predicts that the popsicle is overwhelmingly likely to be more melted in five minutes. But a uniform distribution over the microstates compatible with the macrostate that obtains five minutes from now, combined with $\mathbf{F} = m\mathbf{a}$, predicts that the popsicle is overwhelmingly likely to have been more melted five minutes ago—contrary to our initial assumption, as well as to thermodynamics. Not only does our theory make false predictions about the past, but it cannot be consistently applied at more than one time in a system's history. Apply the theory at one time, and the theory itself predicts that it will fail at any other time.[27]

The solution in terms of boundary conditions goes like this. The basic idea is simple. *Assume* that entropy was lower to the past, as our records and memories suggest that it was, and take the uniform probability distribution over the compatible microstates then. The dynamical laws will predict overwhelmingly likely entropy increase to the future of that time. That is what we learned from the work of Boltzmann and Gibbs.

Think of our partly melted popsicle. Our theory as it currently stands predicts that the popsicle is extremely likely to be more melted five minutes from now, and also five minutes ago. But suppose we now *posit* that the popsicle was more frozen five minutes ago; suppose we keep the more-frozen macrostate fixed to the past. Relative to this posit, the uniform distribution (taken over the microstates compatible with the five-minutes-ago macrostate) and the dynamics predict overwhelmingly likely entropy increase for the popsicle's future. Of course, this won't help if we want to make inferences about the popsicle half an hour ago: the popsicle is overwhelmingly likely to have been more melted half an hour ago. So now move the low entropy posit to the thirty-minutes-ago macrostate. Relative to that posit, the popsicle is extremely likely to keep on melting to the future.

You see where this is going. In order to predict entropy increase for the entire history of the world, posit the low entropy macrostate at its very beginning. This past hypothesis, as Albert (2000) calls it, that soon after the big bang the entropy of our universe was extremely low, disallows the high entropy inferences that statistical mechanics makes about the past.[28] Add the past hypothesis to statistical mechanics, and plausibly, we can explain the fact that thermodynamic systems behave asymmetrically in time, even though the dynamical laws governing their particles are time reversible. It is because the world started out with extremely low entropy, and at any

[27] For disagreement on this point, see Earman (2006); another source of disagreement will be discussed in section 7.2. Note that this is not the case for any probabilistic theory. In Bohmian quantum mechanics, for example, the compatibility of the dynamics and the probabilities, at all times, can be demonstrated: Dürr et al. (1992*a*,*b*).

[28] The idea has been suggested in different ways by Boltzmann (1964) (see Uffink (2007); Goldstein (2001)); Feynman (1965, ch. 5); Penrose (1989, ch. 7), (2005*b*, ch. 27); Lebowitz (1993*a*; 1993*b*; 1993*c*; 1999*b*); Bricmont (1995); Price (1996; 2002*a*; 2002*b*; 2004); Albert (2000); Goldstein (2001); Goldstein and Lebowitz (2004); Callender (2004*a*,*b*); Wald (2006). Challenges to its account of thermodynamics are in Winsberg (2004*a*); Parker (2005); Earman (2006); a bit more of which soon.

given time entropy is overwhelmingly likely to go up. Don't posit a low entropy "future hypothesis" since the evidence suggests that entropy was lower to the past and not to the future. (That is, unless we found evidence to the contrary.) On this view, the time asymmetry of thermodynamics comes from an asymmetry in the boundary conditions of our universe.

In this way, amazingly, modern big bang cosmology seems to be getting at what we need in order to explain thermodynamics. The reasoning we get in statistical mechanics from the likes of Boltzmann and Gibbs, and the empirical evidence we get from cosmology, are converging on the same initial low entropy macrostate of our universe.[29] Although we must ultimately assume the past hypothesis, since without it our evidence of past low entropy is likely mistaken, this gives us a kind of justification for the assumption: the evidence we have from both foundations of statistical mechanics and cosmology, evidence empirical and theoretical, suggests that we can reasonably assume initial low entropy. That, plus the fact that all of this evidence would be self-defeating without the assumption of the past hypothesis. Without this assumption, remember, physics deems it overwhelmingly likely that the past was completely different from what we think, and that the laws are completely different as well. We would have no reliable evidence for what the physics of our world is really like, and no reason to infer anything in particular about the past or future. We could not even trust our belief in Newtonian mechanics. For what evidence we have deems it overwhelmingly likely that this evidence is radically misleading. If we assume the past hypothesis, however, we plausibly avoid getting into that muddle.

Some objections, replies, and clarifications, before moving on.

What is the status of the past hypothesis? Some (Albert, Feynman, Penrose, among others) regard the past hypothesis as a fundamental law. Whether you agree will depend on your view of laws. The past hypothesis does satisfy many of the generally accepted criteria of lawhood (counterfactual support, explanatory and predictive success), but for its being a non-dynamical generalization. Still, if successful, the past hypothesis yields a simple and unifying theory—no need to add anything to the laws we already have other than a simple statistical constraint on initial conditions—and this counts in favor of its law status.[30] Note that if we do treat the past hypothesis like this, then there is an asymmetry in the fundamental laws after all, albeit a non-dynamical one. If not, then the past hypothesis is a contingent generalization for

[29] See Earman (2006) for sustained argument against this claim. Earman points out, for example, that not just any low entropy macrostate will be capable of grounding thermodynamics: it should be the kind of small, dense, hot, uniform state that big bang cosmology suggests it was. Even then, we need further details about the initial state to show that this should yield thermodynamics; and in Earman's view, no details likely to be forthcoming will do the job. More, the initial posit doesn't say anything about the rate at which entropy will increase. We need more details about the dynamics to show that the theory predicts a current macrostate of relatively low entropy. All of which leads Earman to conclude that this is no more than a "just-so" story, "a solution gained by too many posits and not enough honest toil" (2006, 412).

[30] In particular on a Lewisian best-system account: Loewer (2001; 2004). But see Frigg (2008*a*, 2010); Winsberg (2008) for argument against this.

which we have empirical evidence, albeit evidence that is only reliable once we assume that it is.

Why does the initial state have low entropy? Doesn't big bang cosmology say that the universe began in a uniform macrostate? Although this has not been worked out rigorously, there is a rough answer that strikes many people as plausible.[31] Immediately after the big bang, the universe was in a uniformly hot "soup," with matter and energy uniformly distributed in thermal equilibrium. This state did have high thermodynamic entropy. The thought is that it had extremely low entropy due to gravity. Gravity is an attractive force: matter tends to clump up under this force, and then to stay clumped up. We know from thermodynamics that maximal entropy states are the equilibrium states toward which systems tend to evolve and then stay. For systems primarily under the influence of gravity, then, a clumped-up state has high entropy.[32] The early state of the universe, non-clumped-up and uniformly spread out, had extremely low entropy due to gravity.[33]

Why did the universe start out in such an unlikely state? This account tells us to assume something that is extremely unlikely by its own lights.[34] Price (1996, 2002*a*,*b*, 2004), who argues in favor of the boundary conditions strategy, says that more is needed to complete the story. In his view, the real puzzle about thermodynamic asymmetry is to explain the low entropy initial state itself.[35] We know that entropy increase is extremely likely from the work of Boltzmann and others, after all. The puzzle, according to Price, is why entropy was so low to begin with. On a view which thinks of this state as a fundamental law, though, this search for explanation will seem misguided. Even without that view of the past hypothesis, one might question the need to explain initial conditions.[36]

How can the past hypothesis get us anywhere, when statistical mechanics says that all the evidence we have for it is extremely likely to be mistaken? Without the past hypothesis, our theory says it's extremely likely that our current memories and records are mistaken, and radically so. If we don't start out assuming past low entropy, then the overwhelmingly most likely scenario is that our current records and memories—

[31] Such as Penrose (1989, 317–322), (2005*b*, ch. 27). For a recent account, see Wallace (2010). See Earman (2006) for disagreement. Earman argues that even the rough answer is implausible, for we do not have, and are unlikely to get, a theory of the entropy due to gravity, let alone such a theory that allows us to calculate the entropy of the entire universe. Wald (2006) is more optimistic. See Callender (2010) for recent discussion. See Ellis (2007) for recent discussion on this and other philosophical issues in cosmology.

[32] Another way of putting it is that the state will be spread out in momentum space, even though it will be relatively clumped up in position space.

[33] Though not completely uniform: enough non-uniformities are needed to start the clumping-up process that leads to the formation of stars and galaxies and so forth.

[34] How unlikely? See Penrose (1989, 343).

[35] A similar view is in Carroll (2008, 2010).

[36] As do, for example, Boltzmann (in Goldstein (2001)); Sklar (1993, 309–318); Callender (1998, 2004*a*,*b*); North (2002). Penrose (1989, ch. 7), (2005*b*, ch. 28) argues that there may be a dynamical explanation on which the initial state is not unlikely. Carroll and Chen (2004, 2005); Carroll (2008, 2010) attempt to explain the initial state by means of the large-scale structure of the multiverse. Wald (2006) argues against these ideas.

no matter how well-correlated they are with other records and alleged states of the world—spontaneously fluctuated out of past equilibrium. Yet once we assume the past hypothesis, the suggestion is, this will no longer be the case. For, given past low entropy, it is overwhelmingly more likely that a popsicle had been more frozen to the past than that it formed spontaneously out of a homogeneous soup. Relative to the assumption that the popsicle was frozen to the past, the overwhelming majority of micro-histories yielding its current state will have come by way of that lower entropy past state; for if that weren't the case, then the entropy of the popsicle would not have been increasing since then. See a footprint on a beach, and without the past hypothesis, the footprint is overwhelmingly likely to have spontaneously formed out of past equilibrium; assume past low entropy, and this is much less likely than a person's having walked on the beach.[37] Relative to the assumption of past low entropy, it is much more likely that the world is in a microstate, compatible with its current macrostate, which evolved through a lower entropy past state with a person on the beach.

In other words, plausibly, the low entropy initial posit makes it overwhelmingly likely that our usual causal accounts for how things got to be the way that they currently are, are correct: that there was a person to cause the footprint on the beach, a more frozen popsicle to cause my memory, and not just a homogeneous equilibrium soup. For the past hypothesis makes it overwhelmingly likely that the correlations among our current records and memories are due to past states of the world. This is not a rigorous argument. It is a plausibility claim that the theory should be able to ground our records in this way, given Boltzmann's reasoning in statistical mechanics, and given big bang cosmology's account of the formation of stars and galaxies, which in turn lead to the existence of beaches and people, who in turn lead to the existence of frozen popsicles, and so on.[38] Plausibly, given the past hypothesis, we can reconstruct a picture of the world on which our inferences, and the records that they rely on, come out successful in the way that we think they are.[39]

(Take the current macrostate of the world, the fundamental dynamics, and the probability postulate. Conditionalize on the past hypothesis, and we constrain the overwhelming majority of possible world-histories to those in which our records are by and large veridical. Hence the basis for Albert's (2000, ch. 6) claim that this explains why we know more about the past than about the future: initial low entropy restricts the possible pasts of our world more than its possible futures. Not that it restricts the set of possible past microstates more than possible future microstates. By Liouville's theorem, the volume in phase space taken up by the world's macrostate at any time will be the same. Rather, there is a "branching tree structure" to the world Loewer (2007), in

[37] See Earman (2006) for disagreement on both parts of this claim.

[38] See Penrose (1989, ch. 7).

[39] Note the different sense of "record" here from that of Lewis (1979). Rather than being a determinant, a record is something in the current state relative to which, conditional on the past hypothesis, it is overwhelmingly likely that the system passed through the state that the record appears to be a record of.

which the initial low entropy macrostate constrains the possible past macrostates of the world more than the possible future macrostates, relative to the current macrostate.[40])

This only gives us records at the level of entire macrostates of the world. What about the localized records we're familiar with—footprints, photographs, and the like? More needs to be done to suggest that the past hypothesis, even if necessary for thermodynamics, is sufficient as well, in particular for the localized records that make up our ordinary evidence for thermodynamics. More details about the initial state might help. Immediately after the big bang, everything in the universe was distributed relatively uniformly, in a dense, hot, equilibrium soup; the matter and fields were evenly distributed and particles were moving around randomly.[41] Some of these randomly moving particles will eventually collide, and under gravity, some clumps of matter will begin to form. These clumps contain accelerating particles and will be hotter on average than the surrounding space. As the universe expands, then, it evolves away from its initial homogeneous state into macrostates that consist of hotter clumps of matter in a cooler surrounding space. Relative to the assumption of initial homogeneity, these later states indicate that there had been particle collisions in the past. For if everything started out moving randomly in an even-temperature soup, the later states containing warmer masses within a cooler surrounding space indicate past particle collisions and not future ones. More, the clumps are relatively localized records of past collisions.[42]

How can the past hypothesis explain entropy increase in the world's various sub-systems, even if it can ground entropy increase for the universe as a whole?[43] Given the deterministic dynamics, a probability distribution taken over the microstates compatible with the macrostate of the world at any time will induce a probability distribution over the world's possible microstates at any other time: conditionalize the initial distribution on the macroscopic constraints at the other time. By means of this conditionalizing procedure, the initial distribution will assign probabilities to the different possible fundamental states of the world at any time. But any microstate of the world includes a specification of the exact state of any sub-system. So the distribution

[40] Why this structure? Plausibly, because the microstates of the world compatible with its macrostate at any time are on trajectories that spread out, or "fibrillate," over more and more distinct macrostates to the future; see note 45. Objections to the explanation of the asymmetry of knowledge are in Parker (2005); Frisch (2005*a*, 2007, 2010); Earman (2006).

[41] We'll ultimately need quantum mechanics to describe an equilibrium state of matter and energy. Even then, you may be skeptical that such a description is possible: note 31. A very brief sketch of how that might go is at the end of North (2003).

[42] This is extremely rough, at best only a very beginning. See Elga (2007) for a more worked-out account; see also Albert (2000, ch. 6).

[43] Winsberg (2004*a*), for example, argues that we need a further posit to rule out local anti-thermodynamic behavior (since small, relatively isolated sub-systems will have randomized microstates as a result of past interactions with the rest of the universe), a posit which, moreover, we don't think is true; Earman (2006, 420) concurs. A similar criticism is in Reichenbach's "branching systems" objection Sklar (1993, 318–331). (See Winsberg (2004*b*) for an updated version of Reichenbach's idea.) Frigg (2008*b*) suggests that the standard measure cannot tell us the probabilistic behavior of an ordinary system, whose microstate is confined to an energy hypersurface, since any such lower-dimensional space gets zero measure on the standard volume measure taken over all of phase space.

taken over the phase space of the world, by assigning probabilities to its possible microstates at any time, will also assign probabilities to the possible microstates of any sub-system at any time. Restrict the initial distribution to the region representing the sub-system's macrostate; that is, conditionalize the initial universal distribution on the system's macroscopic features. More, this should yield relatively uniform probabilities over the sub-system's compatible microstates (see below).[44]

Why should this yield the same probabilities as the empirically confirmed ones of ordinary statistical mechanics? Ordinary statistical mechanics takes the uniform distribution at any time we choose to call the initial one, without conditionalizing on the past. Two reasons. The first is Boltzmann's combinatorics, which suggests that the overwhelming majority of microstates compatible with any given macrostate lie on trajectories that increase in entropy (both to the future and to the past). That is, think of the phase space region representing a system's macrostate. Boltzmann's reasoning suggests that the proportion of the volume of this region that is taken up by microstates leading to entropy increase is overwhelmingly large, and the proportion taken up by microstates leading to entropy decrease is overwhelmingly small. The second is a randomness assumption. Within any phase space region corresponding to a system's macrostate, the microstates leading to entropy decrease will be scattered, relatively randomly, throughout. Again, this is a reasonable, if unproven, assumption of ordinary statistical mechanics.[45] All of which suggests that the standard uniform distribution will yield the same probabilities of future thermodynamic behavior as the uniform distribution that is first conditionalized on the past hypothesis.[46] At the same time, the distribution conditionalized on the past hypothesis will improve upon the standard one with respect to inferences about past thermodynamic behavior.[47]

6.2 Dynamics

Another way of trying to solve our puzzle is with laws that aren't time reversal invariant. If the fundamental dynamical laws say that different things can happen to the past and to the future, then this might explain the asymmetry of thermodynamics.

[44] A standard assumption in statistical mechanics makes it plausible that, for any system, the initial distribution will be relatively uniform throughout any sub-space of the higher-dimensional phase space. So that when we conditionalize the initial distribution on the sub-space (and renormalize), we get another distribution that is relatively uniform. See Lebowitz (1993*b*,*c*, 1999*b*); and below.

[45] Thus Lebowitz: "for systems with realistic interactions the domain Γ_{Mab} will be so convoluted that it will be 'essentially dense' in Γ_{Mb}" (1993*a*, 10); M refers to the system's macrostate, Γ the phase space, Γ_M the phase space region corresponding to M, M_a the system's initial macrostate, M_b its later macrostate, and Γ_{Mab} the region of Γ_{Mb} that came via Γ_{Ma} (the set of microstates within Γ_{Mb} that are on trajectories that come from Γ_{Ma}). Again: "for systems with realistic interactions the domain Γ_{Mab} will be so convoluted as to appear uniformly smeared out in Γ_{Mb}. It is therefore reasonable that the future behavior of the system, as far as macrostates go, will be unaffected by their past history" (1999*b*, S349).

[46] More, the conditionalized distribution arguably get, more inferences about the future correct than the ordinary, unconditionalized distribution does. See the discussion of Napoleon's boot in Albert (2000, ch.4).

[47] More on this is in North (2004, ch. 3).

You might wonder if even the classical laws, time reversal invariant though they be, could do the job. You might think that these laws have some property that will show entropy likely to increase over time; say, some chaotic property. You would not be alone. There is a history of trying to show just this, a history that continues to the present.[48] Yet no approach relying on these as the fundamental laws can suffice to explain the thermodynamic asymmetry, not without asymmetric boundary assumptions. This is for the usual reasons, namely, the time reversal symmetry and determinism that lead straight to the reversibility objections.[49]

That is why ergodic theory cannot do the job, as far as explaining thermodynamics goes. This approach to statistical mechanics uses mathematical theorems from ergodic theory to pinpoint features of the dynamics to explain entropy increase. In so doing, the approach also tries to explain the probability distribution that standard statistical mechanics, and its explanation of entropy increase, relies on.[50] This is a large approach to the foundations of statistical mechanics, which I cannot adequately address here; for survey and references, see Sklar (1993); Uffink (2004, 2007). Let me mention the reasons to be skeptical of its ability to ground the second law of thermodynamics. First, it has not been shown that ordinary systems are, in fact, ergodic. Although some notions of ergodicity have been demonstrated to hold of certain simple systems, results such as the KAM theorem suggest that most statistical mechanical systems will fail to satisfy any strict notion of ergodicity.[51] More generally, the results using ergodic theory do not seem necessary to the statistical mechanical grounding of thermodynamics. Goldstein (2001) argues that many of these results (such as the technique of Gibbs phase averaging, used to calculate the values of thermodynamic quantities at equilibrium) can be shown to hold regardless of whether a system is ergodic. Ergodic theory seems insufficient for this project as well, since it cannot avoid the need for

[48] Part of this history lies in Boltzmann's own H-theorem. For more on the H-theorem and on Boltzmann's later account—that the universe as a whole is almost always at maximum entropy, but we happen to be on the up-slope of one of its fluctuations out of equilibrium—see Ehrenfest and Ehrenfest (2002); Feynman (1965); Sklar (1993, ch. 2); Price (1996, ch. 2); Uffink (2004). For more on symmetric cosmological accounts, see Price (1996, ch. 4). For a recent version of a "symmetric on the whole" theory, see Carroll and Chen (2004, 2005); Carroll (2008, 2010); also discussed in Wald (2006).

[49] Nor does this depend on your view of time reversal (section 3). Even someone like Albert, who thinks that most theories other than Newtonian mechanics are non-time reversal invariant, won't for that reason explain the second law of thermodynamics. As Albert (2000, ch. 1) puts it, these theories are still symmetric with respect to the evolutions of particle positions, and that is enough to get the puzzle about thermodynamic systems going.

[50] The tradition of invoking ergodic theory in explanations of statistical mechanics goes back to Boltzmann (Ehrenfest and Ehrenfest, 2002). Boltzmann's original idea was that a system is ergodic if, for almost all (standard measure 1) initial conditions, its trajectory passes through every point in the available phase space. A version of Birkhoff's theorem then says that for such a system, the infinite time average of the phase function corresponding to a macroscopic property equals the function's average over the phase space. (Birkhoff's theorem says that infinite time averages exist for almost all initial conditions. A corollary says that if a system is ergodic, then those infinite time averages equal the standard (microcanonical) phase averages for almost all initial conditions. See Earman and Rédei (1996).)

[51] See Sklar (1993, ch. 5); Earman and Rédei (1996).

some initial probability assumption: the ergodic approach can't derive all probabilistic posits from the dynamics alone (as in the "measure zero problem" discussed in the literature), with laws that are deterministic and time reversal invariant, though that is one of its chief motivations. In particular, it can't avoid the need for an asymmetric boundary assumption like the one above.[52] (Though not enough to solve the puzzle here, note that ergodicity could help ground the randomness assumption (section 6.1; note 45): that the compatible microstates on entropy-decreasing trajectories will be scattered randomly throughout a system's phase space; that they will spread out over the phase space.[53])

A recent proposal based on non-time reversal invariant dynamics comes from Albert (1994, 2000). Here, too, the basic idea is simple, though we must now take into account quantum mechanics.[54] The suggestion is that a certain theory of quantum mechanics, the collapse theory of Ghirardi, Rimini, and Weber,[55] or GRW, is non-time reversal invariant in a way that can account for the thermodynamic asymmetry.[56]

In quantum mechanics, questions of time asymmetry are tricky, since there are different versions of the theory on the table. But there are some broad similarities and differences that are relevant to our question here.

All theories of quantum mechanics take the Schrödinger equation to be a fundamental law. This is a deterministic and time reversal invariant equation of motion.[57] It governs the evolution of a system's wavefunction, the mathematical object that describes a system's fundamental quantum state at a time. Different theories of quantum mechanics disagree on the scope of this law. Non-collapse theories of quantum mechanics, such as Bohm's theory or many worlds, posit the Schrödinger equation as the fundamental dynamical law governing the evolution of a system's wavefunction, at all times.[58] Collapse theories, on the other hand, say that Schrödinger evolution fails

[52] See Sklar (1973), (1993, ch. 5); Friedman (1976); Leeds (1989); Earman and Rédei (1996); van Lith (2001) for presentations of the problem and various proposals for addressing it. For recent ergodic-based accounts that improve upon the traditional ones, see Malament and Zabell (1980); Vranas (1998); also Campisi (2005). Vranas, for example, suggests that something close enough to ergodicity might actually hold of ordinary systems. See Strevens (1998, 2003, 2005) for a different, non-ergodic-based approach to demonstrating that macroscopic generalizations, such as those of thermodynamics, come from chaotic properties in the micro-dynamics.

[53] See Berkowitz *et al.* (2006) for recent work along these lines. See Earman (2006, 406) for such a suggestion based on a mixing property (stronger than ergodicity). Earman, however, argues that this suggestion undermines the Boltzmann apparatus.

[54] A different approach, using quantum decoherence, is in Hemmo (2003); Hemmo and Shenker (2001, 2003, 2005). Bacciagaluppi (2007) is another. An overview of different theories of quantum mechanics is in Albert (1992), Uffink (2010) discusses others.

[55] See Ghirardi et al (1985, 1986).

[56] On the relevant sense of time reversal non-invariance, see Arntzenius (1997*a*).

[57] It is time reversal invariant given the standard time reversal operator in quantum mechanics, which maps $t \mapsto -t$ and also takes the complex conjugate. Whether this is a legitimate time reversal operator is open to question: see section 2.

[58] The guiding equation of Bohm's theory, which governs particle evolutions, comes from the Schrödinger equation plus some natural symmetry considerations: Dürr *et al.* (1992*b*, 852–854).

to hold whenever the wavefunction "collapses" onto one of its components, in accord with probabilities dictated by the theory. Collapses are non-unitary, indeterministic transitions, not governed by Schrödinger evolution.

GRW is a collapse theory. It posits a fundamental, probabilistic collapse law governing the evolution of the wavefunction, in addition to the Schrödinger equation. In GRW, the collapse law gives a probability per (small) unit time of a wavefunction collapse,[59] at which point the wavefunction is multiplied by a normalized Gaussian function. The result is that the wavefunction is localized to a small region within its phase space.[60] The probability that the multiplying Gaussian is centered on any given location in phase space depends on the wavefunction just before the collapse, in accord with the usual square amplitudes.

Not only is wavefunction collapse governed by a fundamental, indeterministic law on this theory, but by a fundamental, non-time reversal invariant law. GRW assigns probabilities to the different possible future wavefunctions that a system's current wavefunction could collapse into. (After which the wavefunction will evolve deterministically, in accord with the Schrödinger equation, until another collapse occurs.) The theory doesn't assign probabilities to different possible past wavefunctions, given a system's current wavefunction. The collapse law doesn't say anything about the chances of different past wavefunctions.[61] GRW then says that different things can happen in either direction of time: wavefunctions can collapse in accord with lawful probabilities to the future, not the past.

Time reversal invariant theories of quantum mechanics face the same problem of explaining the asymmetry of thermodynamics that classical theories do. As a result, in order to ground thermodynamics, these theories will need an asymmetric boundary assumption such as the one discussed above. This assumption is needed in order to make it unlikely that a system ever starts out in an entropy-decreasing quantum state; for if it did, then the deterministic and time reversible dynamics entails that the system will decrease in entropy to the future of that state.[62]

[59] Roughly per "particle." Recent empirical evidence suggests that a probability of collapse per atom isn't right, but there are other versions available. I say "particle" since there are no fundamental particles on this theory, and so arguably no particles at all: see Albert and Loewer (1995); Albert (1996). See Allori et al (2008) for different ways of understanding the theory's ontology.

[60] Except for the "tails": Albert and Loewer (1995).

[61] As Arntzenius (1997*a*) puts it, GRW is a theory of forward transition chances, with no backward transition chances. Against this, Price (1996, 2002*a*,*b*) argues that a theory like GRW might really be a symmetric theory, with backward transition chances in addition to the usual forward ones; the backward chances don't result in observed frequencies because they are subordinate chances that are overridden by the initial low entropy condition. One might wonder, though, why we should believe in the existence of lawful chances in that time direction, if they are never manifested in observable frequencies. More, it seems we can only add backward transition chances at the expense of empirical adequacy, since quantum phenomena don't display invariant backward transition frequencies, as argued by Arntzenius (1995, 1997*a*,*b*).

[62] There remains the question of how to define a uniform probability measure over the complex infinite-dimensional vector spaces of quantum mechanics. This is a large question for any version of quantum statistical mechanics.

Albert's suggestion is that this won't be the case for GRW. On this theory, there is a fundamental time asymmetry in the dynamics. So maybe this fundamental asymmetry can explain the macroscopic asymmetry of thermodynamics.

The reason to think that GRW might be able to do this stems from the structure of the entropy-decreasing microstates in phase space, combined with the nature of the theory's transition probabilities. Recall that, plausibly, the phase space regions consisting of the entropy-decreasing microstates (really, the microstates leading to any abnormal thermodynamic behavior) are scattered randomly, in extremely tiny clumps, throughout a system's phase space. Indeed, these entropy-deacreasing regions will be so scattered and tiny that a uniform probability distribution taken over just about any phase space region—any region that is not as tiny and scattered as the entropy-decreasing regions themselves—will deem it overwhelmingly unlikely that a system will decrease in entropy. That is, entropy decrease (to the future as well as to the past) is overwhelmingly unlikely, not only given a uniform distribution over the phase space region corresponding to a system's macrostate, but also given a uniform distribution over virtually any sub-region of that phase space region, however small, including the neighborhood of any single microstate, whether entropy-decreasing or not.[63] This is what Boltzmann made plausible.

If this is right, then the entropy-decreasing microstates are extremely unstable: any system that's in an entropy-decreasing state is extremely "close" to being in an entropy-increasing one.[64] Plausibly, therefore, on a theory of fundamental, indeterministic wavefunction collapses, any system will be overwhelmingly likely to increase in entropy. For it is overwhelmingly likely that, if the system is in an entropy-increasing quantum state at some time, then a wavefunction collapse to the future will keep its state within the entropy-increasing regions of its phase space—the regions containing the quantum states that will deterministically, in accord with the Schrödinger equation, increase in entropy to the future. And it is overwhelmingly likely that, if the system is in an entropy-*decreasing* state at some time, then a wavefunction collapse to the future will cause it to jump to a state within the entropy-increasing regions. Given the extent of the instability of the entropy-decreasing microstates, these wavefunction jumps should make it overwhelmingly likely that *any*[65] system will evolve in accord with the second law of thermodynamics, even if it starts out in an entropy-decreasing microstate (a microstate on a trajectory that takes the system to a lower entropy future macrostate).

Since the abnormal microstates take up non-zero phase space volumes, not just any kind of collapse will get this result. But there are reasons to think the collapses of a theory like GRW will.[66] The GRW collapse law assigns a probability per unit time to

[63] See also note 43.

[64] But see note 62.

[65] That is, any large enough system, large enough to exhibit an entropy-increasing tendency: see Albert (1994), (2000, ch. 7); North (2002). And setting aside the possibility of Maxwell's demon type systems: Albert (2000, ch. 5).

[66] Briefly here. See Albert (1994, 2000, ch. 7), also North (2004, ch. 1), for more.

a wavefunction collapse, in accord with the usual quantum mechanical probabilities, at which point the wavefunction is multiplied by a Gaussian. At any time, that is, the theory places a probability distribution over the different possible microstates that a system could evolve into after collapse, given its wavefunction at that time, in accord with the usual Born rule. This probability distribution is centered on the system's initial microstate. It is also uniform over a very small phase space region, smaller than any region representing a macrostate. Given the level of the instability of the abnormal microstates, and given the Gaussian width of the collapsed wavefunction (the width over which the probabilities are distributed), the region over which this probability distribution is taken should be at least as large as the smallest sub-region of phase space which yields the same probabilistic predictions as the standard distribution over all the microstates compatible with a system's macrostate. At the same time, the region should be small enough that any system, even after undergoing a wavefunction collapse, will remain in one of its possible microstates. Collapses won't cause the system to behave non-thermodynamically by carrying it to some far-off region in phase space, that is.

This is non-rigorous. Calculations are needed to show that the GRW distributions are of this order. But the conclusion seems plausible, given how tiny and scattered the entropy-decreasing portions of a system's phase space should be.

Of course, whether this works as a theory of thermodynamics depends on the truth of GRW as a theory of quantum mechanics. And the account relying on asymmetric boundary conditions arguably succeeds in grounding thermodynamics. So the question is whether GRW, should it turn out to be a true theory, could explain thermodynamics *better*.

There is reason to think that it can. In order to answer the reversibility objections, any time symmetric theory of quantum mechanics will require two fundamental probability distributions: the probabilities of quantum mechanics and the statistical mechanical probability distribution. We are thus left with "two utterly unrelated sorts of chance," as Albert puts it, "one (the quantum-mechanical one) in the fundamental microscopic equations of motion, and the other (the statistical-mechanical one) in the statistical postulate" (2000, 161).

A statistical mechanics based on GRW dynamics, on the other hand, does away with the latter distribution. On this theory, it is the probability per unit time of a wavefunction collapse that yields overwhelmingly likely entropy increase, not a probability distribution over possible initial wavefunctions. There is no need for an additional probability distribution, since no matter which microstate compatible with its macrostate a system starts out in—even an entropy-decreasing one—the stochastic dynamics predicts that it is overwhelmingly likely to evolve with the second law of thermodynamics. No need for an initial distribution to make entropy-decreasing quantum states unlikely: the dynamics takes care of this for us. (The past hypothesis is still needed for inferences about the past, but not as a correction to otherwise faulty retrodictions, as it was above.)

On this theory, there would be only one probability law underlying thermodynamics: the probabilistic law of wavefunction collapse. The probability distribution

posited by GRW to yield a viable theory of quantum mechanics would also yield the probabilities of statistical mechanics and of thermodynamics. This is then a simpler, more unified theory. Other things being equal, it is preferable, assuming that an explanation is better to the extent that it is simpler and more unifying, relying on fewer independent assumptions. Although we must wait and see what the right theory of quantum mechanics is, and although asymmetric boundary conditions should do the job if GRW is not that theory, this is a better theory of thermodynamics, if GRW does turn out to be true.[67]

7. The Status of Statistical Mechanics

Another way to challenge the above attempts at a solution, whether via asymmetric boundary conditions or asymmetric dynamics, is to challenge the view of statistical mechanics as a scientific theory. So far, I've been assuming that statistical mechanics is a fundamental theory. Hence the problem with thermodynamics: if statistical mechanics is a fundamental theory and thermodynamics is not, then where does the asymmetry of the latter come from, if not an asymmetry in the former?

What, then, is the status of statistical mechanics? Could denying it fundamental status help solve the puzzle about thermodynamics?

7.1 Universal and fundamental

As Albert presents it, and as it tends to be treated in physics textbooks, statistical mechanics is a fundamental theory. Statistical mechanics consists of the fundamental dynamics, whether classical or quantum mechanical, for systems of large numbers of particles.[68]

Albert goes further than ordinary statistical mechanics books do. On his version of the theory, the probability distribution is pushed back to the initial state of the universe. This distribution is then updated, by conditionalizing, for use at all other times. Albert argues that this is the right thing to do in the face of the reversibility objections (that is, unless a GRW dynamics is true). But note the effect of this maneuver. Given the deterministic dynamics, the initial distribution yields a probability distribution over each possible microstate of the universe at any time. In so doing, it assigns a probability to anything that supervenes on the fundamental physical state of the

[67] See Albert (2000, ch. 7) for further considerations in its favor, Callender (1997) for argument against, and North (2004, ch. 1) for more discussion. See Price (2002*a*,*b*), Uffink (2002) for further argument against; North (2002) is a reply to Price.

[68] In Albert's presentation, statistical mechanics comprises the following three fundamental laws: the dynamics, the statistical postulate, and the past hypothesis (2000, 96). Remove the past hypothesis to get the version of the theory presented in textbooks.

universe at any time. This means that the theory makes probabilistic predictions for every physical event in the world's history—for every fundamental physical event, and for every event that supervenes on the fundamental physical state.[69] (Indeed, it assigns probabilities to the possible microstates of a system conditional on *any* less-precisely-specified state.[70]) Since this theory makes probabilistic predictions for everything that happens in the world, everything that happens must either conform to its predictions, or else disconfirm the theory.

If this theory is right, then statistical mechanics underlies not only the future behavior of gases, as it does in ordinary textbooks, but it underlies their past behaviors as well. It also underlies all sorts of macroscopic phenomena, even such things as the fact that people tend to keep spatulas in kitchen drawers rather than in their bathtubs, to use an example of Albert's. The idea that statistical mechanics can ground these phenomena might strike you as outlandish. But it follows naturally from the past hypothesis as a solution to the reversibility objections, combined with a realism about statistical mechanics (against a view such as Leeds', below) and a physicalism according to which everything supervenes on the world's fundamental physical state.

Of course, statistical mechanics is not ordinarily used to predict things like spatula locations. Nor should it be: the calculations involved would be much too complicated. Why then think it should do so in principle? Because the evidence we have so far supports this theory (at the least, it does not contradict it; more below), and this is the theory we end up with in reply to the reversibility objections. Or if GRW is correct, then because the statistical mechanical probabilities are the fundamental quantum mechanical probabilities.

7.2 A more limited theory

That's an awful lot to ask of statistical mechanics. Leeds (2003) argues that it's too much. Where Albert takes the reversibility objections to motivate a reformulation of the statistical postulate, Leeds suggests that we treat statistical mechanics instrumentally. Statistical mechanics is simply a successful instrument of prediction, and for just those phenomena we have evidence that it is successful for.

Ordinary statistical mechanics takes the uniform distribution over the macrostate of a system at any time we choose to call the initial one, regardless of its past behavior. And it is successful in doing so. According to Leeds, we should follow this ordinary practice, using the standard distribution to predict things such as the future behavior of gases and the values of thermodynamic quantities at equilibrium, and leave it at that. For we have no reason to think that statistical mechanics can (or should) yield successful inferences about the past, let alone where people tend to keep their

[69] We can set aside here the question of how to spell out this supervenience relation. It suffices to assume that there is such a relation.

[70] Hence Loewer's argument (2008; 2009) that this can account for the existence of the special sciences. See Albert(ms) for further discussion.

spatulas.[71,72] A more limited version of the theory is "all we need, and also the most we are likely to get" (Leeds, 2003, 126).

In Leeds' view, we needn't worry so much about the reversibility objections. For we can simply rely on our records, explaining the fact that an ice cube was more frozen ten minutes ago by describing its macrostate half an hour ago, for instance.[73] In particular, we needn't try to correct the predictions of ordinary statistical mechanics by pushing the statistical postulate back so far as to get a theory with claims to universality. Instead, we should use the standard postulate for making future thermodynamic predictions, since it has proven its mettle in that temporal direction. We should refrain from using it for the past, given its manifest failure in that direction. Empirical evidence shows that statistical mechanics only gives us rules for making inferences about the future, not about the past.

Likewise for other macroscopic phenomena, such as where people tend to keep their spatulas. Here, it's not that we have evidence that statistical mechanics fails, but that we lack any reason to think it can succeed. We certainly don't see any positive evidence in ordinary statistical mechanics textbooks: spatulas are a far cry from the systems that statistical mechanics ordinarily talks about, such as boxes of gas. So we should refrain from using statistical mechanics to predict these things, and not try to alter the theory so that we can.

The result is a statistical mechanics that is committed to less, and is correspondingly less prone to failure. This also means that we get less out of it, however: it can't be used to predict macroscopic phenomena other than the future behavior of ordinary thermodynamic parameters. And given that we don't yet have disconfirmation of the stronger theory, and given the success of statistical mechanics for other macroscopic systems made up of the same kinds of particles,[74] and given the reasoning of Boltzmann and Gibbs, it is not so crazy to hope that it could.

Leeds (and Callender, below) suggest that statistical mechanics, properly understood, doesn't make any inferences about the past; in particular, it doesn't make false inferences that need correcting with a revised statistical postulate. But is this the right view of the theory's range of predictions? Why did we entertain the idea that it makes these predictions? The reason is the time reversal invariance and determinism

[71] A similar inductive skepticism could stem from a view like that of Cartwright (1999). Cartwright argues that we have no reason to infer that the physical laws will hold of ordinary systems outside the laboratory, even granting their truth in laboratory situations we set up.

[72] Further, if we regard the distribution as no more than a successful instrument of prediction, we can consistently apply it at arbitrary times, avoiding the inconsistency with the dynamics mentioned at the beginning of 6.1. One of Leeds' motivations is a view of the statistical mechanical probabilities as subjective and epistemic; against, for example, the view of Albert (2000, ch. 4); Loewer (2001).

[73] Cf. Earman (2006, 421–422).

[74] Thus Pathria (1996, 1): "Statistical mechanics is a formalism which aims at explaining the physical properties of matter *in bulk* on the basis of the dynamical behavior of its *microscopic* constituents. The scope of the formalism is almost as unlimited as the very range of the natural phenomena, for in principle it is applicable to matter in any state whatsoever. It has, in fact, been applied, with considerable success, to the study of matter in the solid state, the liquid state or the gaseous state, matter composed of several phases and/or several components", and more (italics in the original).

of the dynamics. The classical dynamics (and the dynamics of no-collapse quantum mechanics), taken by itself, does yield inferences about the past. Plug in the state of a system at any one time, and the dynamics will predict its state at any other time—without taking into account the time of the initial state, and without making a distinction between past and future temporal directions. As far as the dynamics is concerned, the prediction could hold to the past or to the future of the state we plug in. Statistical mechanics, which takes this dynamics and applies it to large systems, should be likewise temporally symmetric, leading again to the reversibility objections and the past hypothesis as a means of responding to them. Without the past hypothesis, that is, statistical mechanics will yield these inferences of past high entropy, inferences that disconfirm the theory unless we do something to prevent them.

But Leeds has pointed to a trade-off. Either we accept a more limited version of the theory and evade the reversibility objections in that way, or end up with a theory that is committed to a lot, including much that it can get wrong. Since the stronger theory would be deeper and more unifying, explaining not only individual systems' behavior, but the success of thermodynamics as a whole, and other macroscopic phenomena besides, it seems worthwhile to aim for it, unless we get evidence otherwise.

7.3 Special science

Another take on statistical mechanics comes from Callender (1997, 2008*a*). Callender argues that the thermodynamic puzzle shows that statistical mechanics is a special science rather than a fundamental theory (let alone a theory with ambitions to universality). This is because it bears the hallmark of a special science, namely, the requiring of special initial conditions—in this case, initial low entropy—for its generalizations, such as the second law of thermodynamics, to hold.

Compare this with a special science generalization such as Fisher's fundamental theorem of natural selection, which says that the rate of evolution in a population is roughly equal to the variance in fitness. This law does not always hold of real organisms (as in artificial selection by breeders). But by regarding it as a special science law with an implicit ceteris paribus clause, we understand that it is only supposed to hold given initial conditions where natural selection is the lone force at work.

Similarly here. By regarding statistical mechanics as a special science, we understand that its predictions are only supposed to hold when the requisite initial conditions are in place. Statistical mechanics simply does not hold of models of the world with high entropy pasts.

This view of statistical mechanics has its own solution to the puzzle of thermodynamics: there was no problem to begin with. Statistical mechanics, properly understood, does not make any high entropy predictions about the past. Not once the requisite boundary conditions are in place.

There are reasons to disagree with this view of statistical mechanics, however. Take the past hypothesis as a fundamental law, and statistical mechanics is a fundamental

theory which rules out past high entropy, without the need for special initial conditions; the initial state is itself a law that rules out models with high entropy pasts. Take the fundamental dynamics to be GRW, and there is no problem of past high entropy, because the theory does not say anything about the past. Both these accounts avoid the conflict with thermodynamics. Yet neither one requires statistical mechanics to be a special science.

Even without those views, one might deny that statistical mechanics is a special science, for the way in which statistical mechanics requires special boundary conditions is different from the way that ordinary special sciences do. First, the initial constraints of statistical mechanics, initial low entropy and a uniform probability distribution over the microstates compatible with that state, are very simple and natural. A special science generalization like Fisher's law posits constraints that are intuitively contrived: the initial state must be such that there are no artificial breeders, for instance. Second, statistical mechanics requires an initial state for which we arguably have independent evidence from cosmology. Fisher's law approximates what happens only by ignoring intervening factors that actually occur. Third, statistical mechanics makes extremely successful predictions about the future; it is in order to get it to succeed for the past that we need the initial constraint. None of the predictions of Fisher's theorem would come out absent its initial constraints. Fourth, without the past hypothesis, it is not just that statistical mechanics makes predictions that conflict with thermodynamics. Most of our inferences about the world would fail, and fail radically. If the initial conditions required of Fisher's law did not hold, though, we wouldn't lose the same handle on our evidence about the world.

Here's a different idea. Suppose that statistical mechanics comprises the fundamental dynamical laws and a statistical postulate. (Add the past hypothesis to get Albert's version.) The second law of thermodynamics is then a consequence of statistical mechanics, not a generalization of statistical mechanics itself. In that case, it is not the generalizations of statistical mechanics that fail absent special initial conditions; it is the generalizations of thermodynamics, and even then, only when applied to the past. Thermodynamics is the special science here, not statistical mechanics.

Of course, if statistical mechanics is fundamental and thermodynamics is not, then we will want an account of the latter on the basis of the former. Callender suggests that we elude this puzzle with a different conception of statistical mechanics. But the puzzle stems from the fact that statistical mechanics applies directly to the fundamental constituents of the world: it describes macroscopic systems in virtue of their comprising fundamental particles. That's why it is so puzzling that it should fail to ground the widespread and familiar macroscopic regularities. The generalizations of thermodynamics (or of a science like evolutionary theory), on the other hand, are stated independently of the fact that systems are composed of particles (see note 11). Ordinary special science generalizations hold without reference to the fundamental physical ontology; that is part of why they are special sciences. And if GRW is true, all the more reason to think that statistical mechanics is not a special science in the way that evolutionary theory is, for statistical mechanics would be a direct consequence of the fundamental dynamics, even without the initial constraint.

8. OTHER TIME ASYMMETRIES AND THE DIRECTION OF TIME

Thermodynamics covers a surprisingly wide range of the time asymmetric phenomena of our ordinary experience: the spreading of gases, the cooling of cups of coffee, the melting of popsicles, and more besides. This raises a tempting prospect. Perhaps whatever explains thermodynamics can explain all of the widespread macroscopic asymmetries we are familiar with—the asymmetry of knowledge, of counterfactuals, of causation, and more.

It might seem strange to hope that it could. Less strange, once we notice that these other asymmetries, like the thermodynamic one, all involve sequences of fundamental physical states that can occur in one temporal order and not the other. This raises the same question. Where do these macroscopic asymmetries come from, if not from asymmetries in the underlying laws?

As an example, take the wave asymmetry. Waves (water waves, electromagnetic waves) behave asymmetrically in time. Waves propagate away from their sources to the future and not to the past. We see waves spread out from their sources after those sources (a rock dropped in a pond, a light switch flipped on) begin to accelerate; we don't see waves converging on sources which then begin to accelerate. The puzzle is that the physical laws governing waves are symmetric in time. This is similar to the puzzle of thermodynamics, and solutions tend to fall into one of the two same camps: posit an asymmetry in boundary conditions or in the dynamics. Thus, Frisch (2000, 2005*b*, 2006) argues that the wave asymmetry is an additional fundamental dynamical law, whereas Price (1996, 2006, ch. 3) argues that it stems from special initial conditions. My own view (North, 2003) is that the wave asymmetry can be explained analogously to the thermodynamic one, by means of initial low entropy, so that it is not the additional law it is for Frisch, but neither is it the same explanation as the one for Price.[75] I argue that this is a reason to prefer the account: it can explain, in one simple and unified theory, both the asymmetry of thermodynamics and the asymmetry of wave phenomena.

More generally, if any account of the thermodynamic asymmetry is able to explain other macroscopic time asymmetries, then this would be a reason to prefer it. Indeed, if one such account could give a single, unified explanation for all the pervasive time asymmetries of our experience, then that would be a huge—perhaps decisive—mark in its favor. Some accounts of thermodynamics aim to do just this.[76]

[75] See also Arntzenius (1993); North (2004, ch. 2); Atkinson (2006). Zeh (1999) is a different initial conditions approach. See Price (2006); Earman (2010) for more on the radiation asymmetry.

[76] This is a current area of research in philosophy. On other time asymmetries and ways of accounting for them, see, among others: Reichenbach (1999); Horwich (1987); Savitt (1995, 1996); Price (1996); Callender (1998, 2008*b*); Zeh (1999); Albert (2000, ch. 6); Rohrlich (2000); Elga (2001); Huggett (2002); Kutach (2002, 2007, 2010); Rovelli (2004); Frisch (2005*a*); Eckhardt (2006); and references therein.

One reason that thermodynamics seems so central to questions about time and our experience is that thermodynamics covers such a wide range of the everyday processes we experience. The correct theory of thermodynamics might even account for the asymmetry of records, as well as the asymmetries of knowledge and memory (see section 6.1), all of which are particularly central to our ordinary experience in time.[77]

Another is that the best theory of the thermodynamic asymmetry may be able to tell us whether time itself has a direction: whether there is an objective distinction between past and future, a distinction that is intrinsic to the nature of time itself. (That is, whether there is a temporal orientation on the space-time manifold.) We can't directly observe whether time has this structure. Nor do the phenomena, however asymmetric they appear to be, suffice to tell us this. Things are asymmetrically distributed in space, but we don't conclude from this alone that space itself is asymmetric.

We can learn about the structure of time another way: from the fundamental dynamical laws. In general, we infer a certain structure to the world from features of the dynamics. If the fundamental dynamical laws can't be formulated without referring to some structure, then we infer that this structure must exist in order to support the laws—"support" in the sense that the laws could not be formulated without it. Thus, if these laws are non-time reversal invariant, then we couldn't state them without presupposing an objective distinction between the two temporal directions: a structural difference picking out which time direction things are allowed to evolve in and which they are not. This would then give us reason to infer that time has a direction. If the laws are time reversal invariant, on the other hand, then they do not presuppose a temporal direction. They "say the same thing" regardless of which direction things are evolving in. In that case, we would not infer a direction of time.

Non-time reversal invariant, fundamental laws would thus give us reason to believe that time has a direction. However, this inference won't be conclusive. There could instead be highly non-local laws or asymmetric boundary conditions, neither of which suggest a direction of time. How do we decide?

Any inference to fundamental structure in the world, whether a direction of time or some other, must take into account the best, most fundamental physical theory. And this is where the account of thermodynamics comes in. Thermodynamic phenomena are asymmetric in time; they encompass much of our everyday experience of asymmetric processes in time. Recall the two approaches to explaining thermodynamics: posit an asymmetry in boundary conditions or in the dynamics. If the former is the best account of the thermodynamic asymmetry, then we arguably would not have reason to infer that time has a direction.[78] If the latter is the best account of thermodynamics, then we arguably would have reason to infer that time is asymmetric, ultimately responsible for the asymmetries we observe.[79] On this view, GRW, for example, posits

[77] Against this, see Earman (2006).

[78] But see Maudlin (2007*a*) for argument that this asymmetry in boundary conditions is evidence for a direction of time.

[79] A different view says that time's passage, over and above any structural asymmetry between past and future, explains our experience in time: Maudlin (2007*a*).

non-time reversal invariant laws that would give us a reason to infer a direction of time.[80] A no-collapse theory such as Bohmian mechanics does not. (On that theory, the asymmetry in the phenomena is not a matter of fundamental dynamical law, but the result of asymmetric boundary conditions.[81])

The best account of our ordinary macroscopic experience in time, in other words, can give us insight into the nature of time itself. All of which is to say that thermodynamics, which covers such a wide range of the ordinary phenomena of our experience—including, perhaps, the fact that we have memories of the past and not the future—is central to our explanation of, and our experience in, time.

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[80] Price disagrees: see note 61.

[81] See Arntzenius (1995). Which theories of quantum mechanics do, and which do not, indicate time's asymmetry is debatable: see Callender (2000).

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