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Author(s): Craig Callender
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What is 'The Problem of the Direction of Time'?

Craig Callender†‡
London School of Economics

This paper searches for an explicit expression of the so-called problem of the direction of time. I argue that the traditional version of the problem is an artifact of a mistaken view in the foundations of statistical mechanics, and that to the degree it is a problem, it is really one general to all the special sciences. I then search the residue of the traditional problem for any remaining difficulty particular to time's arrow and find that there is a special puzzle for some types of scientific realist.

1. Introduction. In Tom Stoppard's *Arcadia*, the child prodigy Thomasina considers the consequences of the reversibility of Newtonian physics and remarks to her tutor,

> When you stir your rice pudding, Septimus, the spoonful of jam spreads itself round making red trails like the picture of a meteor in my astronomical atlas. But if you stir backward, the jam will not come together again. Indeed, the pudding does not notice and continues to turn pink just as before. Do you think this is odd? (Stoppard 1993, 4–5)

With his mind on the tutoring session and unrelated delicate matters, Septimus answers "no." However, at least since Maxwell's time, most philosophers and physicists have disagreed with Septimus. They deem the behavior of the pudding to be not only odd but a full-fledged phil-

†Department of Philosophy, Logic and Scientific Method, The London School of Economics, Houghton St., London WC2A 2AE, UK.

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osophical perplexity. There is thought to be a serious conflict between the unidirectional process of the jam and pudding turning pink and the alleged time-symmetric laws of fundamental physics.

What exactly is the problem? Although many philosophers and physicists have gestured toward a problem based on the above tension, I have never seen an explicit *and* convincing statement of the problem that is supposed to worry philosophers. What might be useful, therefore, is to try to put some flesh on the bones of the above intuition in an attempt to find a substantive philosophical difficulty. To this end, I first argue that the traditional version of the problem is an artifact of a mistaken view in the foundations of statistical mechanics, and that to the degree it is a problem, it is really one general to all the special sciences. I then search the residue for any remaining problem particular to time's arrow and find that there is a special puzzle for some types of scientific realist.

2. The Traditional Problem. The question I wish to concentrate on essentially involves statistical mechanics (SM) and its attempt to explain the second law of thermodynamics. The problem arises from the fact that SM appears completely time symmetric. Given the same circumstances in the present, the inferential practices and procedures we use in SM make the same predictions in one temporal direction as in the other. Despite this, the past and future look quite different.

To see the difficulty, recall how Boltzmann treats the second law of thermodynamics. The second law states that for individual systems passing through irreversible cycles, the entropy must monotonically increase with time. This is how thermodynamics characterizes the ubiquitous 'irreversible' processes like the one described by Thomasina. SM accounts for the second law by relying on an asymmetry between microstates headed toward equilibrium and microstates headed away from equilibrium. Let X be the point representing a system's exact position and momenta in the 6N-dimensional phase space Γ appropriate to the system (for simplicity *only* we will assume the system is classical). Call microstates X(t) 'normal' if their subsequent Newtonian time development will take them to states corresponding to a higher Boltzmann entropy; call them 'abnormal' if their subsequent Newtonian time development takes them to states corresponding to lower entropy. What Boltzmann noticed is that of all the possible microstates compatible with the macrostate of a system, vastly many more are normal ones than abnormal ones. If one then imposes an initial Euclidean probability metric on Γ one can see that the phase space asymmetry between normal and abnormal states implies that it is overwhelmingly probable that the *actual* microstate of a system will subsequently evolve toward rather than away from equilibrium. By making entropy a function of the microstate of the system, S(X), Boltzmann is able—albeit controversially—to account for entropy increase in terms of mechanics.

The problem of the direction of time, as passed down to us from Boltzmann, is essentially the following (see, e.g., Lebowitz 1993). The classical equations of motion are time-reversal invariant. Roughly, this means (for deterministic systems) that if we take any nomically possible sequence of states of a system, then the reverse sequence of 'temporally reflected' states is dynamically necessary. Classically, temporally reflecting a system just comes to reversing its velocity, so reversibility implies that every trajectory has a velocity-reversed counterpart (for a debate about this, see Callender 1995). Now consider the set of all microstates $\{X_i(t)\}$ compatible with the macrostate of a system at time t. Due to the time reversibility of the classical dynamics this set includes the time reverse $X^*(t)$ of each microstate $X(t)$ in the set. If we now consider the set $\{X_i(t+\Delta t)\}$, the set of all the microstates that the microstates at time t evolve into in the span Δt, and the set $\{X_i(t-\Delta t)\}$, the set of all the microstates that precede the microstates at t by the amount Δt, the fact that each $X(t)$ can be paired with its time reverse $X^*(t)$ implies that $\{X_i(t-\Delta t)\}$ is the time reverse of $\{X_i(t+\Delta t)\}$. Reversibility implies, in effect, that the vast majority of microstates that are *antecedents* of the dynamical evolution from the microstate underlying the present macrostate correspond to a macrostate with *higher entropy* than the present one. This means that it is overwhelmingly probable that present states evolved from higher entropy states, which is obviously false. Thermodynamics and ordinary experience inform us that entropy does not increase in both temporal directions.

Concisely put, the problem is that given a nonequilibrium state at time t_2, it is overwhelmingly likely that

(1) the nonequilibrium state at t_2 will evolve to one closer to equilibrium at t_3 but that due to the reversibility of the dynamics it is also overwhelmingly likely that
(2) the nonequilibrium state at t_2 has evolved from one closer to equilibrium at t_1

where $t_1 < t_2 < t_3$. However, transitions described by (2) do not seem to occur; or phrased more carefully, not both (1) and (2) occur. For ease of exposition let us dub (2) the culprit.

The traditional problem is not merely that nomologically possible (anti-thermodynamic) behavior does not occur when it could. That is not straightforwardly a problem: *all sorts* of nomologically allowed processes do not occur. Rather, the problem is that SM makes a prediction that is radically falsified, and that is a problem according to anyone's theory of confirmation.

3. Escape via 'Many-System' Entropy? There have been many different responses to this difficulty, all of which attempt to supply a reason to rule out (2). There is no way to do this from within classical mechanics itself. This much should be clear from the objections historically made against Boltzmann's H-Theorem. Boltzmann tried to derive entropy increase directly from the dynamics, but the time reversibility and quasi-periodicy of the dynamics prohibit this. Time reversibility implies that for every solution for which entropy increases a time-reversed solution in which entropy decreases also exists. Quasi-periodicy implies that each system will eventually recur to some past state or to some state arbitrarily close to some past state; therefore, from the quasi-periodicy of the dynamics it follows that *every* solution whose entropy at some point increases will, given sufficient time, suffer entropy decrease. So long as the entropy is a function of the state of an individual system governed by quasi-periodic dynamics, it cannot possibly always monotonically increase.

This argument, or something like it, is quite well-known and never directly challenged. Yet if one picks up a textbook in SM, one is sure to find some kind of derivation of entropy increase. People try to evade the argument's consequences by rejecting one or more of its assumptions about the nature of entropy. The most common maneuver is to define a new entropy function. Starting originally with Maxwell and made famous by Gibbs, the 'many-systems' approach to SM is most popular (see Sklar 1993). Broadly speaking, it treats entropy as a function of *probability distributions* ρ on ensembles, $S(\rho)$, in contrast to Boltzmann's entropy which makes entropy a function of X. According to most many-system approaches, one considers a fictitious ensemble consisting of all the microstates compatible with a given macrostate. The macroscopic parameters pick out a distribution of representative points, which is treated as a fluid in Γ. The entropy is defined as a function of the density function characterizing this fluid. The reason why I mention this alternative approach is because switching from $S(X)$ to $S(\rho)$ allows one to evade the recurrence theorem. It is compatible with the recurrence of each individual system that the ensemble as a whole not display recurrence of any kind. If one makes entropy a property of a probability distribution on an ensemble, therefore, it is possible for it to increase monotonically without ever recurring.

But this should not be judged a 'way around' the above problem. This is not the place for a general critique of the many-systems approach (see Callender 1996); however, perhaps a few quick remarks may help one see that such a 'resolution' is misguided. Due to the dynamics' reversibility and periodicy, S(X) can decrease with time. Since S(ρ) cannot (ex hypothesis), it is thus physically possible for S(ρ) to increase with time while S(X) decreases. But if this is possible, one wonders what good a proof of S(ρ) increase is. The S(ρ) associated with an ice cube on the floor might increase even if (or when) the individual ice cube suddenly starts to freeze! How can the ensemble approach account for the approximate truth of the second law when its explanation is compatible with the real microstate being one that subsequently leads to S(X) decrease? The switch from S(X) to S(ρ) hardly appears to be a harmless case of concept extension. Instead it seems to abandon the principal goal of SM, namely, that of providing a mechanical explanation of the success of thermodynamics.

4. Special Initial Conditions. The traditional problem is that SM predicts that entropy will increase in both temporal directions. What we desire is a way to rule out transitions (2), i.e., high-to-low entropy transitions. But within the rules of the game, that is, without changing the dynamical laws, there is no way to eliminate these transitions as a matter of nomic necessity within classical mechanics. Clearly, then, the only way to solve the problem within the given framework is to appeal to temporally asymmetric boundary conditions. By assuming that *earlier* states of the universe are of comparatively low entropy *and* that *later* states are not also low entropy states, transitions (2) are prohibited. There are no high-to-low entropy processes simply because earlier entropy was very low.

Many people are unhappy with this 'solution' because it does not *feel like* a solution. There are at least two sources of worry. One is that the initial conditions needed to rule out transitions (2) are 'special'; that is, according to the uniform probability metric, the requisite conditions are exceedingly improbable. Another is that the answer is not the sort of thing for which we were looking. We began by looking at thermodynamic processes and wondering why this occurs in a world governed by time-symmetric laws. The 'explanation' is that the initial conditions of the universe are such as to make this happen. This strikes one as extraordinarily unsatisfying. It is as if we suddenly found potatoes everywhere looking like Richard Nixon and have this explained with the statement that the universe's boundary conditions must be consistent with this phenomenon. We are searching for a local cause of thermodynamic behavior and what we are answered with is a global consistency constraint. Consequently, although special initial conditions may account for thermodynamic behavior, it does not feel right to say they explain it.

Can the asymmetric boundary conditions themselves be explained? Price (1996) seems to think this is a worthwhile though ultimately question-begging enterprise. However, I am skeptical about the whole business of explaining initial conditions. As both Hume and Kant taught us, it is fantastic to believe that we could find evidence supporting a causal explanation for why the boundary conditions are what they are. Yet that is precisely the sort of explanation we desire.

According to the framework of our problem, there are no further options. We cannot derive entropy increase from the dynamical laws, nor are special initial conditions explanatory of entropy increase or likely to be explained by something further. The solution, such as it is, is our only option, even if it merely 'accounts for' time's arrow instead of explaining it.

5. The Traditional Problem Revisited. I suggest we adopt a new perspective on this situation. If the solution does not feel like a solution, I say, perhaps this is because the problem is not really a problem. SM's prediction that entropy was higher in the past generates the traditional problem. But does SM really make this faulty prediction? It does if SM holds in all the models of our fundamental theory. That is, if as is usually assumed, SM (including its version of the second law) is considered a fundamental science, a part of classical or quantum mechanics, then it does predict that the past has high entropy. But *should* we consider SM true in all the models of classical mechanics?

We have seen that it should not. Observation radically undermines SM unless one supposes the universe begins with low entropy. This means that the generalizations of the theory only apply given the existence of special boundary conditions. The nonexistence of these conditions is classically allowed, and some day, they may even be actual, e.g., the entropy could start to decrease during the recontraction period, as the cosmologist T. Gold speculated, or there might be periods when nothing like SM works. The traditional problem of time's arrow implies that the class of models corresponding to SM is a *proper subset* of the class of models corresponding to classical mechanics. SM, like the special sciences, is not a universal theory but instead tolerates exceptions.

For this reason and for some others I have not space to discuss, I believe we ought to consider SM a special science. Historically SM has always been put on the fundamental side of the basic/non-basic laws division, but the real lesson of the conventional problem of time's arrow is that we no longer have reason to uphold this tradition.

Now notice what this does to our original problem. If SM is a special science, then the laws of SM are—like special science laws generally—

ceteris paribus laws. Consider, for instance, Fisher's so-called 'fundamental theorem of natural selection', which states that the rate of evolution in a population is equal, roughly, to the variance in fitness. This generalization plainly does not hold always of real world organisms. Artificial selection by breeders can and does 'falsify' it. However, because it is a special science law making implicit reference to a ceteris paribus clause, we understand Fisher's law to hold (strictly) only when natural selection is the sole force at work (so long as it is not a disguised mathematical truth). The same goes for SM. Properly stated, SM claims 'ceteris paribus, entropy increases in both temporal directions'. SM does not make an unconditional claim that entropy is high in both the past and future; instead it makes the conditional claim that entropy will be higher if it was earlier lower. SM does not imply entropy is high in the past any more than Fisher's law states that artificial breeding must operate in accordance with the variance in fitness. And the 'problem' of time's arrow is no more and no less worrisome than the problem artificial breeding poses for evolutionary theory. Whatever one makes of this link, one should agree that it is odd to claim that positing special initial conditions is a way of solving a problem within the special sciences. *Special sciences just do not predict the existence of boundary conditions contrary to those necessary for the truth of their generalizations.* If special sciences can be said to *predict* anything about the boundary conditions of the universe, precisely the opposite is true: they predict the existence of boundary conditions *needed* by their generalizations. If this is the case, then SM predicts the existence of a low entropy past, not a high entropy past, and with this prediction the traditional problem evaporates.

The traditional problem is really a particular instance of a much more general difficulty. That difficulty, if it is one, is that the generalizations of the special sciences hold only in a proper subset of the class of all physically allowed models. The generalizations of biology, geology, etc., require the universe to have physically very 'improbable' boundary conditions. We are still left with the tension characterized by Thomasina, the striking absence of anti-thermodynamic behavior in a world governed by time reversible dynamics. But this is now accompanied by the striking absence of anti-biological behavior, anti-psychological behavior, etc., and when viewed in this context, it is doubtful that there is anything special about Thomasina's worry. Rivers keep eroding their banks and people continue to smile back at smiley face cards even though it is possible that neither occur. There is a feeling that we ought to take the 'improbable' initial conditions as a call for 'new physics'. Yet I suspect this feeling is weakened when it is realized that we would never expect new physics to come to the 'rescue' of biology, etc.[1]

6. The New Problem. The traditional problem of the direction of time, we saw, ultimately rests upon a mistaken view in the foundations of SM. The generalizations of SM require 'special' initial conditions, to be sure, but then so does the truth of the generalizations of the other special sciences. The 'improbability' of our universe may be a philosophical problem, but if it is, it is not one specific to the second law. The question is: are there any remaining problems that are special to the problem of time's arrow?

Suppose an alternative mechanical theory existed that ruled out transitions (2) *dynamically*, i.e., as a result of the dynamical laws. This would present a new problem: ought we to favor this new theory over the standard one that eliminates transitions (2) via asymmetric boundary conditions?

The history of SM is littered with failed attempts to provide theories making transitions of type (2) dynamically impossible (see Callender 1996; Sklar 1993). The most recent and promising attempt comes from quantum mechanics, and it is worth pausing a moment to discuss it. Albert (1996) points out that the interpretation of quantum mechanics known as GRW prohibits transitions (2). Among its other virtues, GRW supplies a dynamics according to which entropy increase is overwhelmingly likely in one temporal direction and overwhelmingly unlikely in the other. For every solution of the GRW dynamics it is true that the system described by that solution will probably tend toward equilibrium and probably never approach nonequilibrium (modulo subtleties described in Albert's paper). If GRW is true, SM becomes a kind of derivational consequence of the fundamental laws. Here, then, is an example of an alternative dynamics that yields thermodynamic behavior for arbitrary initial conditions.

This result, incidentally, is not a general consequence of quantum mechanics, but only a feature of the GRW dynamics. Other interpretations of quantum mechanics, e.g., Bohm's theory, share the same problems classical mechanics does with time's arrow. Bohmian mechanics is perfectly reversible (at least in the relevant sense) and quasi-

1. There may be other ways of dissolving the problem too. If I understood him correctly, Hugh Mellor suggested to me (at the 1996 annual meeting of the British Society for the Philosophy of Science in Sheffield, UK) that the probabilities in SM may be *intrinsically* time-directed. This would have the effect of prohibiting the inverse probabilities that cause the traditional problem. While this maneuver would certainly work, I am uncomfortable with imposing by fiat a solution on the physics merely from an analysis of probability.

periodic. The only way Bohm's theory can account for thermodynamic processes, then, is to posit special initial conditions. This appears to be true for all of the other interpretations of quantum mechanics.

Although GRW will rule out transitions (2), it will *not* help us with the problem besetting the special sciences generally, namely, the 'improbability' of a thermodynamic world. GRW eliminates transitions (2) because it is not time reversible. Also, because its evolution is stochastic this elimination works for all microstates: we need not suppose that the universe began in a 'normal' microstate state corresponding to a low entropy macrostate. But we do still need to assume that the world began out of equilibrium, that the world did not begin as a soup. GRW explains why entropy in the past was lower, *given* that the universe started far from equilibrium, but it does not explain why the universe began in a non-equilibrium state. Consequently it does not make the 'improbable' initial conditions probable. We still need an 'improbable' initial macrostate (though *any* microstate compatible with that macrostate will work). Of course, it may be that this is not a genuine problem, and if so, then it appears GRW solves all the remaining problems (assuming it remains empirically adequate).

For the above reason it is misleading to think of GRW as performing a task analogous to that performed by the inflationary model with regard to the standard big bang cosmological model. The standard model, it is well-known, is said to suffer from the fact that it must adopt very special initial conditions if it is to describe a universe at all like ours. According to some 'natural' a priori probability metric on the space of all the standard cosmological models, the space of solutions giving rise to a universe like ours (the right temperature, the right density, etc.) is tremendously small. The inflationary scenario allegedly avoids these special initial conditions essentially by having the universe expand much more rapidly for a certain period than it does in the standard cosmology. The effect of this rapid expansion is to allow the microwave fields to have causal contact with each other (thereby producing the right temperature) and to flatten out the curvature (thereby fixing the right density). If it works, inflation's great acheivement is its ability to turn out a universe broadly like ours with almost arbitrary initial conditions. GRW, by contrast, does not eliminate the need for 'improbable' initial conditions; instead it eliminates the need for 'probable' microstates corresponding to an 'improbable' macrostate. Like the inflationary scenario, however, it does eliminate *an* appeal to boundary conditions needed to account for a striking feature of our universe.

Returning to the main thread, the question was whether there is some special problem with time's arrow not also shared by the special sciences in general. The answer is 'yes', for there is a theory making the laws of SM derivational consequences of the fundamental laws of mechanics whereas there is no theory offering a similar opportunity to the rest of the special sciences. This is not a difference of principle, for there *could* be a new physics that makes biology a derivational consequence, although this is hardly likely.

GRW makes us an offer that is not extended to the other special sciences. The offer is to replace a special science generalization requiring special boundary conditions with a fundamental dynamical law. Ceteris paribus, how should we respond? This is a difficult question because it is unclear that favoring a dynamic explanation is a reaction to a *genuine deficiency* in time symmetric quantum mechanics, again, say Bohm's theory. That is, it is unclear that GRW is offering us a real advantage. After all, Bohm's theory is absolutely empirically adequate. Moreover, as I mentioned earlier, we know that this move can not be right in general, due to the similarity this problem has to the one afflicting the special sciences. Trying to rid biology of the need to posit these special initial conditions would be preposterous. And surely it is no mark against biology that its generalizations do not hold in all physically possible worlds.

For these reasons it is not straightforward that we should adopt the dynamic explanation rather than the non-dynamic one. The physicist J. Hartle (1986) complains that the attitude against dynamical explanations is not very "adventurous" (p. 468). Yet being faulted for a lack of spirit is hardly the equivalent of being faulted for a lack of rationality. There are undoubtedly all sorts of aesthetic reasons to prefer dynamical explanations over non-dynamical ones. The interesting question is whether there exist *epistemic reasons* to prefer the former to the latter.

Phrased like this, it is obvious that the answer depends on one's broad stance in the philosophy of science. One's views on explanation, realism, etc., are all relevant. Although a more detailed treatment of this difficult question is needed than I can give here, the bare outlines of an answer are clear enough. Empiricists who think the sole goal of scientific inquiry is empirical adequacy will not find any *epistemic* reason to prefer dynamical explanations to special initial condition explanations if the two candidates are both empirically adequate. The models used to describe the phenomena are what counts. Whether one chooses to pick out the class of relevant models with laws alone or with laws plus boundary conditions does not matter, and indeed, may be viewed as merely a difference in language. Scientific realists, by contrast, are not solely constrained by empirical adequacy in their search to find epistemic reasons to prefer a theory, and therefore, they may have reasons to prefer dynamical explanations to non-dynamical ones. I do not see good general reasons for believing dynamical explanations of a phenomenon to be more likely to be true than non-dynamical explanations. However, I do see how various properties in which realists are interested may *track* dynamical explanations. That is, given a choice between dynamical and non-dynamical explanations of a phenomenon, it may be that the dynamical explanations tend to be, e.g., more unifying than the non-dynamical explanations. Thinking in terms of Friedman's (1974) unification account, where, roughly put, a theory is more unifying than another just in case the first has fewer primitives in it than the second, one can see that conceiving of the posit of special initial conditions as an unexplained parameter will permit the dynamical explanation to be preferred. This would have to operate as only presumptive evidence in favor of the theory, of course, for otherwise one would be charged with finding a dynamical explanation for nearly everything. If one takes unification to be a mark of truth, then the realist may have an epistemic reason to prefer GRW to Bohmian mechanics (ceteris paribus). Alternatively, one may find GRW to be less simple or fruitful than Bohmian mechanics, and for this reason, some scientific realists will prefer the non-dynamical explanation in this case. Scientific realists will not all agree that dynamical explanations are more conducive to the truth than their non-dynamical rivals; nor need each agree across the board, since, for instance, greater unifying ability may attach to the dynamical theory in one situation but the non-dynamical one in another. Unless one has both the specific theories under consideration and the theory of explanation at hand, then the most one can say is that some scientific realists will find the new problem of the direction of time to be a genuine difficulty.

In sum, the problem of the direction of time is not what it appears to be. The traditional problem is an artifact of a misguided view in the foundations of SM and even the new problem formulated here is not a problem for everyone. Whether there is a philosophical puzzle depends upon one's general stance in the philosophy of science, as does the question of whether or not SM is a special science. Returning to the quote with which I began, when Thomasina asks Septimus if he thinks the behavior of the jam in her pudding is odd, he should have answered "Yes, it is odd, but it is not a genuine problem except to some scientific realists."

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