Chapter 20
Two-Way Thermodynamics: Could It Really Happen?

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Abstract In previous publications I have suggested that opposite thermodynamic arrows of time could coexist in our universe. This letter responds to the comments of H.D. Zeh (elsewhere in this volume).

Keywords Time's arrows · Two-time boundary conditions · Causality · Cosmology · Quantum measurement theory

1 Context

In 2002 a conference took place in Bielefeld, entitled, "The direction of time: The role of reversibility/irreversibility in the study of nature" [1]. In my presentation I spoke about recent work in which I had demonstrated the compatibility of opposite arrows of time for two subsystems within a larger "universe." As Dieter Zeh explains in his companion article [2], he has reservations about the physical realizability of this phenomenon and our present articles address this issue. However, as a preface to my response to his remarks, I will give a brief review, plus references, to the work that has given rise to this dialog.

When this response was originally written I gave it the title, "The slings and arrows ... whips and scorns of time," [3], not because of the barbs that Zeh was throwing my way, but quite the opposite, because of a review I had written years ago of the first edition of his book, [4], for Science magazine [5]. Instead of giving it its deserved high praise, I looked for faults, some it turns out of my own invention. So this "response" gives me the opportunity to apologize for that review.

In the endnotes there are postscripts added after the original writing of this article.

2 Opposite Arrows

The thesis that the thermodynamic arrow of time follows the cosmological arrow (the universe is *expanding*) was put forth by Gold about 1960 [6]. My contribution,

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about 10 years later [7], was to observe that if you wanted to make this case in a logical way, you needed to recognize that the choice of giving *initial* or *final* conditions was itself a choice of the arrow (and that you needed to be careful about this in the sort of arguments Gold was offering). So that to establish Gold's thesis one should argue from time-symmetric boundary conditions. For elaboration and references see my book [8], where I do indeed make such arguments. With this perspective, the thermodynamic arrow of time becomes a *consequence* of the cosmological geometry rather than an independent physical input. This leads to a problem that can be posed purely mathematically. We know that if a low-entropy macroscopic state is given as an initial condition, the entropy will increase. (The definition of "entropy" is discussed below.) That is the Boltzmann H-theorem. By symmetry, if low-entropy data are given as a final condition, then entropy will drop as one approaches T—a bit unintuitive, but it is what you explain to students after they've been exposed to that same theorem and some of its puzzles. But now one can consider a more complicated situation: a compound system, for a part of which initial data are given, and for the other part final data are given. If these portions did not interact, the result is obvious; each acquires its own arrow as if the other were not there. The surprise (perhaps) comes when they *are* permitted to interact. What I showed [9] is that if the interaction is not too strong, the separate portions retain their arrows.

Several questions immediately arise: can sentient beings having opposite arrows communicate? This is perhaps the most entertaining issue [10]. At first I thought they could; now I am not so sure [11]. More important I would say, is whether, if this actually happened, would you notice it? In [9] the effect of one system on the other is an increase of noise, a slight (for small interaction) increase in the rate of entropy increase. At this point I believe that if we were to illuminate such a region with our own light sources and take successive photographs, we would see backward-arrow events, people growing younger, that sort of thing.

Before addressing Zeh's specific criticisms, let me say how I imagine opposite-arrow regions could exist in our actual universe. First, I can think of no way for this to happen except if in our distant future the universe is headed for collapse. Assuming then that the overall geometry of the universe is roughly symmetric, I would expect that the thermodynamic arrow of time would also be symmetric. This is a kind of temporal cosmological principle: our direction of time is not special (with a nod towards Occam's razor as well). Again, these matters are discussed at length in [8]. So sentient creatures in this distant future would also see an expanding universe (this suggestion was made almost immediately after Gold put forward his thesis, although he has said that it was not he who made it). In this distant future, with its time-reversed arrow, one can now imagine that some region and its constituent, highly stable, matter becomes isolated from everything else and is able to avoid equilibration for a very long time (in *its* forward time direction). If this stuff were to show up our neighborhood, the end of its isolation could trigger all sorts of processes that would be visible to us as a decrease in its entropy (in *our* forward direction).

I do not expect to run across this stuff any day now. Not only does it require a roughly symmetric cosmology, but the bang-crunch interval cannot be so long that *everything* has come to equilibrium. And you would need a lot of luck to have a substantial chunk of unequilibrated matter come close enough to observe the destruction of its arrow. (On the other hand, if you were that lucky, you would have found evidence for a time-symmetric universe, exactly because it is so difficult to think of any other way this could happen.)

Besides the material I have already cited, a number of other publications on this subject have appeared[1] [16–18]. In the present article I do not focus on the source of the thermodynamic arrow itself, but recent work on this appears in [19].

3 Dialogue on Opposite Arrows

To keep this response from running to book length, I will focus on what seem to me the most significant points, with bias toward those that can be dealt with most directly.

3.1 Solving the Two-Time Boundary Value Problem

Zeh rightly divides the issue of two-time boundary problems into a number of categories. There is the question of existence, and there is the question of finding the solution. Then there is the important distinction between classical and quantum mechanics.

First, existence: usually I phrase my boundary value problems in terms of macroscopic properties at two widely separated times,[2] so I am asking whether there are paths from one relatively large region of phase space to another. For the classical systems of our usual experience, the answer is yes. I am being cautious in characterizing the dynamics as "our usual experience," since I do not want to address the

[1]The idea of having opposite arrows has been taken up by other authors as well. Wiener [12] speculated on this subject, and Creswick [13] has looked into the possibility physically producing systems that in a sense evolve backward in time. Finally there have been recent works of science fiction [14, 15] that explored some of the consequences of these ideas.

[2]The time separation used in any particular two-time problem depends on what is being studied. For looking at Gold's proposal I generally think of the earlier time as being (approximately) the era of recombination, when our present cosmic background radiation was emitted. In a time-symmetric cosmology, I take the other time to be a corresponding time interval before the big crunch (or oscillation minimum). These are times for which matter should be distributed (roughly) uniformly, representing an entropy maximum for a system dominated by short-range forces. As the universe expands and gravitational forces dominate, uniformity becomes an extremely unlikely circumstance, so that what was a maximum becomes a minimum. This justifies two-time low-entropy boundary conditions. For the opposite-arrow boundary value problem, I have in mind a smaller time interval (later and "earlier" than recombination) and regions of space smaller than the entire universe. Finally, for the quantum problems associated with finding "special states". my time range is before and after the operation of a particular apparatus.

question of whether the usual dynamics is ergodic or mixing or whatever. We do know that equipartition is satisfied, that the Fourier heat law is in practice satisfied, so most trajectories do wander quite a bit in phase space, even if the dynamics falls short of certain mathematical idealizations.

A more delicate classical problem occurs when the data specification is set up to give opposite arrows in different regions. The paradoxes that arise in this context may be thought of as assertions on the non-existence of solutions. So there is interest in studying these paradoxes whether or not the two regions can communicate. Zeh refers to my "wet carpet" paradox, which is related to the "grandfather" paradox of time-travel fame. In [10] I take up this existence problem in detail. The simplest resolution, i.e., an existence proof, appeals to continuity and is a takeoff on the treatment of Wheeler and Feynman in their discussions of advanced interactions [20]. But [10] also contains some more down-to-earth mathematical arguments for *microscopic*, mixed boundary conditions. For some boundary value problems, indeed, there is no solution, but for those that most closely resemble the paradox scenario, there are solutions.

What about existence for the quantum problem? I agree with Zeh that this requires more than mere appeal to mixing (or similar) properties of the dynamics. I will argue on several levels.

First I address the last item in Zeh's Sect. 1. How many solutions can one hope for? Even if there are regions of classical phase space that satisfy the two-time boundary conditions, it could happen that their measure is so small that there is not a single quantum state, the point being that quantum states require a minimum volume of $(2\pi\hbar)^N$ (with $2N$ the phase space dimension). I do not have a general answer, but can offer an informative example. Appendix A of Sect. 5.0 in [8] presents the following result: even nonequilibrium *initial* conditions imply a tremendous reduction in available phase space; two-time macroscopic boundary conditions are *also* a tremendous restriction, but not more serious than slightly more demanding initial conditions. Specifically, a cubic centimeter of a monatomic ideal gas of atomic weight 30 at room temperature and atmospheric pressure has about $10^{(10^{20.28})}$ microstates, i.e., lives in a Hilbert space of that dimension. Squeezing them into $1/64$ that volume (in coordinate space) changes the "20.28" to about 20.24. Squeezing them by only $1/8$ and insisting that they reoccupy such a region again at a later time *also* brings the "20.28" to 20.24.[3] There is plenty of room in phase space.

I urge the reader who is troubled by two-time conditioning to reflect on this example. Since we never see all the gas in a cubic cm gather into $1/8$ of its volume spontaneously, we get the impression that this might be impossible rather than

[3]The numbers given here differ slightly from those in [8]. Here I use the physically more realistic statistics of indistinguishable particles. These numbers also reflect more detailed state counting. Specifically, for an ideal gas the number of states is $\mathcal{N} = \exp(S/k) = \exp(N \log[(V/N)e^{5/2}/\lambda_{\text{th}}^3])$, using the standard expression [21] for the entropy of an ideal gas in three dimensions. N is computed from the pressure using $PV = NkT$, k is the Boltzmann constant, and $\lambda_{\text{th}} = h/\sqrt{2\pi mkT}$ is the thermal wavelength. The formula for $\mathcal{N}$ can also be used directly to see the effect of volume changes, as discussed in the text.

merely unlikely. Nevertheless, the reduction of Hilbert space dimension, measured by conventional entropy, is not at all drastic (and corresponds to a slight compression), meaning that Hilbert space has many such states. The reason we do not see this happen is that while such entropy reductions may be small with respect to what macroscopic devices can induce, they are still enormous compared to what will be seen by spontaneous fluctuation.

Now let me give more specific argumentation on the existence of quantum solutions. There is a particular quantum two-time boundary value problem whose solution allows quantum mechanics to retain pure unitary (and deterministic) time evolution (no wave function "collapse") while at the same time does not introduce probability through some back door channel, such as appeal to a collapse-inducing macroscopic world, or many worlds, or degradation of the role of the wave function. This approach involves something I call "special states." These states allow the final condition of the combined apparatus-system to be only a single one of the potential outcomes of the measurement. It would be too much of a distraction to give more detail here; see [8]. In any case, the mathematical problem is formulated as follows. You give projection operators P and Q representing the initial and final subspaces of Hilbert space, $\mathcal{H}$, in which you want your total system (apparatus plus measured system) to be. There is some unitary operator U that evolves this total system between the given times. Then what the two-time boundary value problem seeks is states $\psi(0) \in P\mathcal{H}$ that evolve entirely into $\psi(T) \in Q\mathcal{H}$ (where $[0, T]$ is the time interval for the measurement). This leads you to look at the spectrum of the operator $\widetilde{U}^\dagger \widetilde{U}$, where $\widetilde{U} = QUP$. The operator $\widetilde{U}^\dagger \widetilde{U}$ is Hermitian and has spectrum in the interval [0, 1]. Eigenvectors with eigenvalue 1 represent solutions to the boundary value problem. In [8] I report studies of this mathematical problem for several models of apparatus. Generally speaking there are many eigenvalues clustering around 1. (Interestingly, sometimes there can be none, and this gives rise to potential experimental tests of this theory. See [22].) As the size of the apparatus grows there are more and more near-unity eigenvalues as well.[4] Two remarks: (1) For these special states there is no entanglement at the end of a measurement, particularly useful if one wants to think time symmetrically about measurement. (2) A propos Zeh's remarks about trial and error in the finding of solutions, when solutions of this problem were produced, the process consisted of finding the spectrum (including eigenvectors) of the particular operator mentioned above by standard operator techniques (usually numerical).

I also mention that I am not the only one preoccupied with two-time boundary value problems, both classical and quantum. If one wants to split a molecule using a laser pulse, it turns out that simply hitting it with one of its resonance frequencies does not work very well. Instead [23–25] you must shape your pulse and the finding of an appropriate shape involves solving a future-conditioned problem (which can

[4]The terms "apparatus" and "system" do not imply that this scheme holds only for laboratory experiments. Any situation that could lead to superpositions of macroscopically different states will have this feature. Again, the present paper is not about quantum measurement theory. and for the many questions that may come to mind please consult [8].

also be considered a form of control theory). Similarly, it is of great interest to people studying tunneling [26, 27] to know the time dependence of a system when it finally does succeed in transiting a barrier. Moreover, the same issues arise in quantum computing, where one wishes to implement a quite specific transformation on a collection of states, at the same time being sure that unwanted entanglement does not arise from amplitudes for exciting other levels [28, 29].

I next turn to the question of how you actually solve two-time boundary value problems. Much of what I have to say can be found in [8], where a chapter is dedicated to this problem. Zeh declares [2] that I "mostly" do it by trial and error. Sometimes I do, sometimes I do not. In one of the articles his critique focuses on, *Causality is an Effect* [18], my calculations are analytic, except for numerical *illustrations* later in the paper. There is nothing wrong with numerical trial and error. Furthermore, for stochastic dynamics explicit analytic results can be attained, and some of my work involves such calculations [30–32].

Actually there is a deeper critique in what Zeh is saying, more than merely complaining about how I go about finding solutions. The important question is whether those relatively simple systems for which you can find solutions are reliable indicators of what happens in more realistic cases. In this regard I point out that [18] deals with recovering, analytically, my results about the flipping of arrows along the way from one low-entropy condition to another. I did not explicitly do the calculation for the simultaneous opposite-arrows case, but it should be an exercise using the same techniques already used for the other result. I mention that in those demonstrations I make fairly strong assumptions on the way the systems go to equilibrium.

However, to judge this last issue, whether the numerical and analytic results may be expected to hold in more general systems, it helps to step back and ask why they hold for the cases that *have* been studied. Consider the case of boundary conditions at $t = 0$ and $t = T$ (>0) with low-entropy macrostates at both ends. I have shown that moving inward—from *both* ends—entropy increases. In the middle, if T is big enough, you have equilibrium. Most significantly, the passage to equilibrium, the entropy increase you get say in going forward from time 0, is macroscopically identical to what you would get moving forward from time 0 with *no* future conditioning. Why is that so? It is because, in a sense, the system *forgets the future*. Suppose there is a relaxation time τ associated with the dynamics. Then the condition for the situation I have described is $T > 2\tau$. Here is why: saying it relaxes in time τ means that starting from low entropy at time 0, the system is likely to reach anywhere in (allowable) phase space by time τ. But then it can also get "back" from wherever it is at time $T/2$ (which by assumption is greater than τ) to the region demanded by the time T final condition. Stated differently and thinking in terms of the backward arrow from T to $T/2$, by time $T/2$ if forgets where it "was" at time T.

This strongly suggests that whatever dynamics one has, if the concepts of relaxation time have relevance, my results on particular models should continue to be valid. Systems for which one cannot assign a relaxation time (or if the time is longer than the T associated with the particular problem) are not expected to give the same results, and indeed they hold independent interest for information such processes might provide about cosmology (again, see [8]).

3.2 Isolation

In Zeh's Sect. 2 he raises the issue of isolating a large system, something that, microscopically, is practically impossible. This observation is useful but not a serious concern. It is useful because it points out an essential feature of any two-time boundary value problem, namely that it is meaningless unless all forces on the system throughout the intermediate interval are included. As such, in their grandest form these boundary value problems should include the entire universe. But there is a second perspective, one that allows a narrower view. Suppose one pretends that one *could* isolate a portion of the universe and then reaches certain conclusions about the solutions of two-time boundary value problems in that context (for example, one might consider opposite-arrow boundary value problems [9] in this way). Then one could consider the same boundary value problem slightly modified, say by the entry of a single photon into the region. Now solve the boundary value problem with the additional force. It will change the microscopic paths, but does it change the qualitative conclusions? Generally I expect not, so that for many purposes perfect isolation is not important. Nevertheless, if problems arise in this formulation (e.g., reaction on the external system), one can go back to talking of the entire universe. These remarks apply for both classical mechanics, as in [8], and quantum mechanics, as in [8] and [33].

3.3 Closed Timelike Curves

As to closed timelike curves, they have nothing to do with my opposite-arrow scenarios. To the extent that I assume any geometrical context, I am happy with Friedmann–Robertson–Walker. So Zeh's criticism of science fiction stories has no relevance.

As an aside, I am not convinced that closed timelike curves that extend over long time spans cannot have both increase and decrease in entropy, hence dissipation and its reverse. Physically there would need to be a reason to single out a low-entropy era, and moving away from that (in both directions) entropy would increase. You also should not have shortcuts, timelike paths of varying lengths for nearby spatial regions. I do not know to what extent these conditions could or could not be met in a Gödel universe.[5] In any case the issue has little to do with my story.

[5]Postscript: I've long been suspicious of the alleged paradoxes that would arise in a Gödel universe by virtue of its closed timelike curves. I expect that there could be a reduction in the class of "initial value" problems that have a solution, as for other paradoxes mentioned in this article. ("Initial values" would also be final values and would presumably be on a single spacelike surface. They would involve test particles, not the matter giving rise to the metric itself.) Also, the usual paradoxes are macroscopic, implying the existence, at least locally, of an arrow of time. It's not clear that such could exist. In the summer of 2010 I met another person with similar ideas about this problem, Noam Erez of the Weizmann Institute and my comments here are partly informed by our conversations.

3.4 Entropy Calculation

In his Sect. 3 Zeh declares that the entropy I use is an ensemble entropy and does not assign an entropy to a microstate. This is not true. As stated in [8], page 32, entropy is the logarithm of the number of microscopic states consistent with a macroscopic description. Once you have a coarse graining (i.e., a macroscopic description), you can take any microstate and use the volume of the coarse grain to which it belongs to compute its entropy. In cat map studies I implement this as follows: the system microstate is a point in $\mathbb{I}^{2N}$, with $\mathbb{I}^2$ the unit square and N the number of "atoms" in the gas. To define coarse grains, $\mathbb{I}^2$ is divided into G regions (usually rectangles) and the number of points of the projected system point in each region is the coarse grained description. Thus if a given microstate is $(x_1, y_1, \ldots, x_N, y_N)$, its coarse grained description is $(n_1, \ldots, n_G)$, where n_k is the number of atoms (x_ℓ, y_ℓ) in grain k. Following the definition in [18], the entropy is $S = -\sum p_k \log p_k - \log G$, with $p_k = n_k/N$, if all grains are of equal coordinate space volume. The "$p_k \log p_k$" as usual arises from the logarithm of $N!/(n_1! \ldots n_G!)$ and represents the missing information associated with particle identity. The missing information associated with going from real numbers to finite volumes is the same for all $(n_1, \ldots, n_G)$, and is dropped, since in this study I am not concerned with comparing coarse grainings.

As remarked in [8] and commented upon by Zeh, the universe is richer than cat map dynamics. In particular there are fast processes and slow ones. Rather than a disability, I view this feature as a wonderful opportunity. It is precisely because the slowest processes may have two-time boundary condition solutions that differ significantly from their unconstrained counterparts that one might discover indications of a forthcoming big crunch. Specifically you would expect to see impeded or slowed-down relaxation. This idea is not mine, but was advanced by John Wheeler. I have elaborated on it in [8], in particular looking for suitable slow processes and indicators of constrained relaxation.

4 Causality

In his Sect. 4, Zeh discusses causality. Here he addresses the content of my article, *Causality is an Effect*, [18], whose main conclusion I will briefly review. This article is available on the arXiv as cond-mat/0011507. In most of my work I have shown reversals of the arrow of time by exhibiting the time dependence of the entropy. Occasionally people ask about other macroscopic quantities, whether they would show similar behavior. So I decided to deal with the most fundamental such issue, the appearance of macroscopic causality.

The first problem is defining what you mean by causality. Zeh takes me to task over this and I entirely agree that defining causality in a fully deterministic world with fixed boundary conditions at both ends is not easy. Usually what you have in mind for (macroscopic) causality is that the nail enters the wood *because of* and *subsequent to* its being hit by the hammer. It would not go into the wood if the hammer did not strike. I will not even try to be more precise. The point though is that you'd like to perturb the system and see when the changes take place. But how can one perturb a universe whose dynamics is given and whose past and future are fixed?

My solution was to consider in effect two universes, both with the same macroscopic boundary conditions (at $t = 0$ and $t = T$), but which differ in their dynamics at one particular intermediate time t_0 ($0 < t_0 < T$). So the *microscopic* dynamics will in general differ at *all* times. What I looked at was the *macroscopic* behavior. And indeed, I found that if t_0 was close to 0, all macroscopic changes were confined to $t > t_0$, while if t_0 is close to T, macroscopic changes were confined to $t < t_0$, showing that "causality" follows the same arrow as entropy increase.

As to the weakness of my definition I believe that anyone wanting to define a causality concept close to this will need some such strategy just to keep causality from already being fixed by the use of initial conditions, which as I have often noted is equivalent to fixing an arrow of time. I also remark that my definition was to some extent motivated by discussions of dispersion relations, where *perturbation* is the essential notion, but where there is also implicit a notion of an *un*perturbed system serving as a reference point.

In any case, Zeh would prefer a definition in which it is possible to assign a notion of causality to an individual microstate, which, as far as I can tell, cannot be done with the foregoing definition. Indeed my personal preference is to be able to say things about individual systems, so it would be of interest to first, find such a definition, and second establish that, as for my other definition, this kind of causality is also a *consequence* of other arrow-inducing features in a two-time boundary value problem context.

This article is not the place to carry out the aforementioned program, but I would like to make a suggestion. Again we consider the effect of a "perturbation" on a macroscopic system, but now we have only one system, so the perturbation is only some force that we single out, perhaps because it is large and macroscopically recognizable. Focus on a single microscopic state that satisfies macroscopic two-time boundary conditions (as usual), including the perturbation. The test of causality is the following: if we look at only the macroscopic state of the system on one side of the perturbation and try to calculate what happens on the other side, do we get the right answer?

It is clear that the direction of this kind of causality follows the direction of entropy increase (which I relate to proximity to one or another temporal endpoint). If the system is *not* dissipative,[6] then there is no arrow and predictions will be good from both sides. But if it is dissipative, information is lost in one direction and it is only possible to make reliable predictions in the direction of information loss. It is easy to see how this translates into cat map examples, so I omit detailed illustration. This "causality" is closely tied to arrow-of-time definitions based on the choice of what "initial" means. This is consistent with the main thesis of [18], namely that the notions of macroscopic causality and the arrow of time are essentially the same, and that in particular if one is induced by proximity to low-entropy boundary conditions, so is the other.

[6] Bear in mind that this has meaning only with respect to a particular coarse graining.

My second comment on Zeh's Sect. 4 has to do with the oscillator example. The antiquity of this model for studying two-time boundary value problems goes beyond his book [34]; in fact, based on my 1973 work [7] where I used this system for studying two-time boundary value problems, I did not in fact expect to find causality. However, the actual calculation brought a surprise—there *is* causality—and the oscillator example shows interesting subtleties. In Zeh's text he comments that he has used a much larger sample than I did, a remark that puzzled me since my calculation is analytic and only later in my article do I choose a sample for a numerical illustration. If one examines my analytic work it will be seen that there are *two* time scales in this problem. One has to do with the range of frequencies used to smear the oscillators. The other is related to the size of the coarse grains and demands a much larger time interval to see causal effects, as I have defined them. Admittedly my numerical examples are difficult to read, but I would hope the analytic portion would be clear enough. In any case, if you use a long enough total conditioning time, you get causality; if not, you do not.

On the issues raised in Zeh's Sect. 5 on Cosmology and Gravitation I will not comment except to say that everything here rests on much less reliable ground. For example, while some view black holes as the essence of the arrow of time [35, 36], others contemplate their disappearance prior to a big crunch [37]. Moreover, even without total evaporation it is now believed that information (on items that fell in) is returned to the universe through properties of the Hawking radiation. Articles referenced in this section of Zeh's article (but not written by Zeh) concerning absorbing powers of the universe have seemed to me plagued by problems of double counting and incorrect treatment of the boundary value problem that is natural to time-symmetric electrodynamics.

As to Zeh's Sect. 6 on quantum aspects, my views on this are expressed in [8] and, briefly put, are that quantum mechanics, including the measurement process, is fully time symmetric and does not introduce an arrow of time.

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