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Microscopic Origins of Irreversible Macroscopic Behavior: An Overview*

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Abstract

Time-asymmetric behavior as embodied in the second law of thermodynamics is observed in *individual macroscopic* systems. It can be understood as arising naturally from time-symmetric microscopic laws when account is taken of a) the great disparity between microscopic and macroscopic scales, b) initial conditions, and c) the fact that what we observe is "typical" behavior of real systems—not all imaginable ones. This is in accord with the ideas of Maxwell, Thomson, and Boltzmann and their natural quantum extensions. Common alternate explanations, such as those based on equating irreversible macroscopic behavior with the ergodic or mixing properties of probability distributions (ensembles) already present for chaotic dynamical systems having only a few degrees of freedom or on the impossibility of having a truly isolated system, are either unnecessary, misguided or misleading. Specific features of macroscopic evolution, such as the diffusion equation, do however depend on the dynamical instability (deterministic chaos) of trajectories of isolated macroscopic systems.Time-asymmetric behavior as embodied in the second law of

* This article summarizes some of the material in [1]; see also Appendix A.

thermodynamics is observed in *individual macroscopic* systems. It can be understood as arising naturally from time-symmetric microscopic laws when account is taken of a) the great disparity between microscopic and macroscopic scales, b) initial conditions, and c) the fact that what we observe is "typical" behavior of real systems—not all imaginable ones. This is in accord with the ideas of Maxwell, Thomson, and Boltzmann and their natural quantum extensions. Common alternate explanations, such as those based on equating irreversible macroscopic behavior with the ergodic or mixing properties of probability distributions (ensembles) already present for chaotic dynamical systems having only a few degrees of freedom or on the impossibility of having a truly isolated system, are either unnecessary, misguided or misleading. Specific features of macroscopic evolution, such as the diffusion equation, do however depend on the dynamical instability (deterministic chaos) of trajectories of isolated macroscopic systems.

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1. Introduction

Let me start by stating clearly that I am not going to discuss here—much less claim to resolve—the many complex issues, philosophical and physical, concerning the nature of time, from the way we perceive it to the way it enters into the space-time structure in relativistic theories. I will also not try to philosophize about the "true" nature of probability. My goal here is much more modest.[a] I will take (our everyday notions of) space, time and probability as primitive undefined concepts and try to clarify the many conceptual and mathematical problems encountered in going from a time symmetric microscopic dynamics to a time asymmetric macroscopic one, as given for example by the diffusion equation. I will also take it for granted that every bit of macroscopic matter is composed of an enormous number of quasi-autonomous units, called atoms (or molecules).

The atoms, taken to be the basic entities making up these macroscopic objects, will be simplified to the point of caricature: they will be thought of, to quote Feynman [2] as "little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another." While this crude picture of atoms (a refined version of that held by some ancient Greek philosophers) moving according to non-relativistic classical Hamiltonian equations contains the essential qualitative and even quantitative ingredients of macroscopic irreversibility, it needs to be reformulated to take account of quantum mechanics. This raises various issues which will be discussed briefly later; see also appendices B and C.

Much of what I have to say is a summary and elaboration of the work done over a

[a] For the interested reader I append in Appendix B and C reviews of three books which attempt to deal with such more fundamental questions.

century ago when the problem of reconciling time asymmetric macroscopic behavior with the time symmetric nicroscopic dynamics became a central issue in physics . To quote from Thomson's (later Lord Kelvin) bautiful and stilll highly recommended 1874 article [3],[4] "The essence of Joule's discovery is the subjection of physical [read thermal] phenomena to [microscopic] dynamical law. If, then, the motion of every particle of matter in the universe were precisely reversed at any instant, the course of nature would be simply reversed for ever after. The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water .... Physical processes, on the other hand, are irreversible: for example, the friction of solids, conduction of heat, and diffusion. Nevertheless, the principle of dissipation of energy [irreversible behavior] is compatible with a molecular theory in which each particle is subject to the laws of abstract dynamics."

Formulation of Problem

Formally the problem considered by Thomson is as follows: The complete microscopic (or micro)state of an isolated classical system of N particles is represented by a point X in its phase space Γ, $X = (\mathbf{r}_1, \mathbf{p}_1, \mathbf{r}_2, \mathbf{p}_2, ..., \mathbf{r}_N, \mathbf{p}_N)$, $\mathbf{r}_i$ and $\mathbf{p}_i$ being the position and momentum of the ith particle. The evolution is governed by Hamiltonian dynamics (with some specified Hamiltonian $H(X)$) which, given the microstate $X(t_0)$ at some time t_0 determines the microstate $X(t)$ at all other times t, $-\infty < t < \infty$. Let $X(t_0)$ and $X(t_0+\tau)$, with τ positive, be two such microstates. Reversing (physically or mathematically) all velocities at time $t_0 + \tau$, we obtain a new microstate. If we now follow the evolution for another interval τ we find that the new microstate at time $t_0 + 2\tau$ is just $RX(t_0)$, the microstate $X(t_0)$ with all velocities reversed; $RX = (\mathbf{r}_1, -\mathbf{p}_1, \mathbf{r}_2, -\mathbf{p}_2, ..., \mathbf{r}_N, -\mathbf{p}_N)$.

(We assume for simplicity that $H(X) = H(RX)$.) Hence, if there is an evolution, i.e. a trajectory $X(t)$ in which some property of the system, specified by a function $f(X(t))$ behaves in a certain way as t increases, then if $f(X) = f(RX)$ there is also a trajectory in which the property evolves in the time reversed direction. Thus, for example, if particle densities get more uniform in a way described by the diffusion equation then since the density profile is the same for X and RX there must be evolutions in which the density gets more nonuniform, see Fig. 1. So why is one type of evolution, consistent with "entropy" increase by the second "law", common and the other never seen?

Resolution of Problem

The explanation of this apparent paradox, due to Thomson, Maxwell and Boltzmann [1–10], which I will now describe, shows that *not only is there no conflict* between reversible microscopic laws and irreversible macroscopic behavior, but, as clearly pointed out by Boltzmann in his later writings [b], *there are extremely strong, albeit subtle, reasons to expect the latter from the former.* These involve several interrelated ingredients which together provide the sharp distinction between microscopic and macroscopic variables required for the emergence of definite time asymmetric behavior in the evolution of the latter despite the total absence of such asymmetry in the dynamics of the former. They are: a) the great disparity between microscopic and macroscopic scales, b) the fact that events are determined not only by differential equations, but also by initial conditions, and c) it is not every microscopic state of a macroscopic system that will evolve in accordance with the

[b] Boltzmann's early writings on the subject are sometimes unclear, wrong, and even contradictory. His later writings, however, are generally very clear and right on the money (even if a bit verbose for Maxwell's taste), c.f. [5]. The presentation here is not intended to be historical.

entropy increase predicted by the "second law", but only the "majority" of such states—a majority which however becomes so overwhelming when the number of atoms in the system becomes very large that irreversible behavior becomes a near certainty. To make the last statement complete we have to specify the assignment of weights, or probabilities, to different microstates consistent with a given macrostate. (Note, however, that since we are concerned with events which have overwhelming probability, many different assignments are equivalent but there is, as we shall see, a "natural" choice.) These considerations enabled Boltzmann to define the entropy of a macrosystem in a microscopic way and relate its change, as expressed by the second law, to the evolution of the system's microstate. We describe below how the above explanation works by considering first a particular situation and then the general case.

To describe the behavior of a macroscopic system of N atoms in a box V, say $N > 10^{20}$, with the volume of V, denoted by $|V|$, satisfying $|V| \gtrsim Nl^3$, where l is a typical atomic length scale, it is essential to use a much cruder description than that provided by the microstate X, a point in the $6N$ dimensional phase space $\Gamma = V^N \otimes \mathbb{R}^{3N}$. We shall denote by M such a macroscopic description or macrostate. M will consist of the specification, to within a given accuracy, of the energy (and other stable invariants, if any) of the system and of some other macroscopic variables, e.g. the fraction of particles in each half of the box. (A more refined macroscopic description would divide V into K cells, where K is large, but still $K << N$, and specify the number of particles, momentum, and energy in each cell, again with some tolerance.) Clearly M is determined by X but there are many X's which correspond to the same M. Let Γ_M be the region in Γ consisting of all microstates X

corresponding to a given macrostate M and denote by $|\Gamma_M| = (N!)^{-1} \int_{\Gamma_M} \Pi_{i=1}^N d\mathbf{r}_i d\mathbf{p}_i$, its $6N$ dimensional Liouville volume. Suppose now that only M is specified at some time t_0. We will then take the probability of $X(t_0)$ being in a subset A of Γ_M to be equal to the volume of A normalized by the volume of Γ_M, i.e. $Prob(X(t_0) \in A | A \subset \Gamma_M) = |A|/|\Gamma_M|$. (This corresponds to the classical limit of the counting weight in quantum mechanics: see later.)

Example

Consider now a situation in which there is initially a partition confining all N atoms to the left half of of the box V. Assume that this specifies the macrostate M_a in which the system finds itself when the constraint is removed at time t_a. The phase space volume available to the system for times $t > t_a$ is fantastically enlarged[c] compared to $|\Gamma_{M_a}|$, corresponding to M_α, roughly by a factor of 2^N. (We neglect the potential energy between the particles so the total (kinetic) energy is fixed for $t > t_a$ and can be ignored here). This region will contain new macrostates, corresponding to different fractions of particles in the left half of the box, with phase space volumes very large compared to the initial phase space volume available to the system. We can then expect (in the absence of any obstruction, such as a hidden conservation law) that as the phase point X evolves under the unconstrained dynamics and explores the newly available regions of phase space, it will with very high probability enter a succession of new macrostates M for which $|\Gamma_M|$ is increasing. This will continue until the system reaches its unconstrained macroscopic equilibrium state, M_{eq},

[c] If the system contains 1 mole of gas then the volume ratio of the unconstrained phase space region to the constrained one is of order $10^{10^{20}}$. This is far larger than the ratio of the volume of the known universe to the volume of one proton.

that is, until $X(t)$ reaches $\Gamma_{M_{eq}}$: M_{eq} is characterized by the fact that $|\Gamma_{M_{eq}}|/|\Sigma_E| \simeq 1$, where $|\Sigma_E|$ is the total phase space volume available under the energy constraint. (The symbol $\simeq$ mean equality when $N \to \infty$.) In this state approximately half the particles will be located in the left half of the box, i.e. N_L/N will be in an interval $(\frac{1}{2} - \epsilon, \frac{1}{2} + \epsilon)$, $\epsilon << 1$ and of course $N_R/N = 1 - N_L/N$; $N_L(t)$ and $N_R(t)$ being the number in the left and right halfs. After reaching that state we will (mostly) see only small fluctuations in $N_L(t)$, about the value $\frac{1}{2}$, well within the precision ϵ, typical fluctuations being of the order of the square root of the number of particles involved. (Of course if the system remains isolated long enough, we will eventually see a 'Poincare return' to the initial macrostate—this time is however much longer than the age of the universe and so is of no relevance here; see [5–9].)

2. Boltzmann's Entropy

This end result of the initial systematic phase of the time evolution in the above example, that of the fraction of particles becoming and remaining essentially equal in the two halves of the container when N is large enough (exactly when $N \to \infty$), is of course exactly what is predicted by the second law of thermodynamics. According to this law the final state of an isolated system is one in which the entropy, a measurable macroscopic quantity of an equilibrium system defined on a purely operational level by Claussius, has its maximum subject to the relevant constraints. In our example this entropy would be defined for all equilibrium states with a given energy and values of N_L and N_R, the number of particles (or moles) in the left and right half of the box, and maximized subject to the constraint of the given energy and of $N_L + N_R = N$. It was Boltzmann's great insight to connect the second law with the above phase space volume considerations by making the observation that for a dilute gas $\log |\Gamma_{M_{eq}}|$ is proportional, up to terms negligible in the size of the system, to the thermodynamic entropy of Clausius. Boltzmann then argued that the relation between thermodynamic entropy and $\log |\Gamma_{M_{eq}}|$ will hold for all macroscopic systems; be they gas, liquid or solid. This provided for the first time a microscopic definition of the macroscopically defined, operationally measurable, entropy of macroscopic systems in *equilibrium.*

Having made this connection Boltzmann then generalized it to define an entropy also for macroscopic systems not in equilibrium. That is, he associated with each microscopic state X of a macroscopic system, a number S_B, given, up to multiplicative and additive constants (which can depend on N), by

$$S_B(X) = \log |\Gamma_{M(X)}|. \tag{1}$$

which, following O. Penrose [11], we shall call the Boltzmann entropy of the system. Boltzmann then used phase space arguments, like those given before for the gas initially confined to one half of the box, to explain (in agreement with the ideas of Maxwell and Thomson) the observation, embodied in the second law of thermodynamics, that when a constraint is lifted, an isolated macroscopic system will evolve toward a state with greater entropy.[d] In effect Boltzmann argued that $S_B(X_t)$ will *typically* increase in a way which *explains* and

[d] When M specifies a state of local equilibrium, $S_B(X)$ agrees up to negligible terms, with the "hydrodynamic entropy". For systems far from equilibrium the appropriate definition of M and thus of S_B is more problematical. For a dilute gas (with specified kinetic energy and negligible potential energy) in which M is specified by the density $f(\mathbf{r}, \mathbf{v})$ of atoms in the six dimensional position and velocity space,

describes qualitatively the evolution towards equilibrium of macroscopic systems.

Typical, as used here, means that the set of microstates corresponding to a given macrostate M for which the evolution leads to a macroscopic decrease in the Boltzmann entropy during some fixed time period τ, occupies a subset of Γ_M whose Liouville volume is a fraction of $|\Gamma_M|$ which goes very rapidly (exponentially) to zero as the number of atoms in the system increases.

These very large differences in the values of $|\Gamma_M|$ for different M come from the very large number of degrees of freedom involved, in an additive way, in the specification of macroscopic properties. This distinguishes macroscopic irreversibility from the weak approach to equilibrium of probability distributions (ensembles) of chaotic systems (those with good ergodic properties) with only a few degrees of freedom, e.g. two hard spheres in a box [13]. While the former is manifested in a typical evolution of a single macroscopic system the latter does not correspond to any appearance of time asymmetry in the evolution of an individual system. Maxwell makes clear the importance of the separation between microscopic and macroscopic scales when he writes [14]: "the second law is drawn from our experience of bodies consisting of an immense number of molecules. ... it is continually being violated, ..., in any sufficiently small group of molecules ... . As the number ... is increased ... the probability of a measurable variation ... may be regarded as practically an impossibility."

On the other hand, because of the exponential increase of the phase space volume with particle number even a system with only a few hundred particles (commonly used in molec-

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$S_B(X) = -\int f(\mathbf{r},\mathbf{v}) \log f(\mathbf{r},\mathbf{v}) d\mathbf{r} d\mathbf{v}$. This identification is, however, invalid when the potential energy is not negligible [12]; see also Section 8.

ular dynamics computer simulations) will, when started in a nonequilibrium "macrostate" M, with 'random' $X \in \Gamma_M$, appear to behave like a macroscopic system.[e] This will be so even when integer arithmetic is used in the simulations so that the system behaves as a truly isolated one; when its velocities are reversed the system retraces its steps until it comes back to the initial state (with reversed velocities), after which it again proceeds (up to very long Poincare recurrence times) in the typical way, see Fig. 2 and 3 [15].

We might take as a summary of such insights in the late part of the last century the statement by Gibbs [16] quoted by Boltzmann (in a German translation) on the cover of his book *Lectures on Gas Theory II*: "In other words, the impossibility of an uncompensated decrease of entropy seems to be reduced to an improbability."

3. The Use of Probabilities

As already noted, typical here refers to a measure which assigns (at least approximately) equal weights to regions of equal phase space volume within Γ_M or loosely speaking to different microstates consistent with the "initial" macrostate M. (This is also what was meant earlier by the 'random' choice of an initial $X \in \Gamma_M$ in the computer simulations.) In fact, any meaningful statement about probable or improbable behavior of a physical system has to refer to some agreed upon measure (probability distribution). It is, however, very hard (perhaps impossible) to formulate precisely what one means, as a statement about the real world, by an assignment of probabilities (let alone rigorously justify any particular one) in our context. It is therefore not so surprising that this use of probabilities, and particularly the use of typicality for explaining the origin of the appar-

[e] After all, the likelihood of hitting, in the course of say one thousand tries, something which has probability of order 2^{-N} is, for all practical purposes, the same, whether N is a hundred or 10^{23}.

ently deterministic second law, was very difficult for many of Boltzmann's contemporaries, and even for some people today, to accept. (The many books on the subject are unfortunately of little help.) This was clearly very frustrating to Boltzmann as it is also to me. I have not found any better way of expressing this frustration than Boltzmann did when he wrote, in his second reply to Zermelo in 1897 [5] "The applicability of probability theory to a particular case cannot of course be proved rigorously. ... Despite this, every insurance company relies on probability theory. ... It is even more valid [here], on account of the huge number of molecules in a cubic millimetre... The assumption that these rare cases are not observed in nature is not strictly provable (nor is the entire mechanical picture itself) but in view of what has been said it is so natural and obvious, and so much in agreement with all experience with probabilities ... [that] ... *It is completely incomprehensible to me* [my italics] how anyone can see a refutation of the applicability of probability theory in the fact that some other argument shows that exceptions must occur now and then over a period of eons of time; for probability theory itself teaches just the same thing."

The use of probabilities in the Maxwell-Thomson Boltzmann explanation of irreversible macroscopic behavior is as Ruelle notes "simple but subtle" [13]. They introduce into the "laws of nature" notions of probability, which, certainly at that time, were quite alien to the scientific outlook. Physical laws were supposed to hold without any exceptions, not just almost always and indeed no exceptions were (or are) known to the second law; nor would we expect any, as Richard Feynman rather conservatively says, "in a million years". The reason for this, as already noted before, is that for a macroscopic system the fraction of microstates for which the evolution leads to macrostates with larger S_B

is so close to one (in terms of their Liouville volume) that such behavior is exactly what should be seen to "always" happen. Thus in Fig. 1 the sequence going from left to right is typical for a phase point in Γ_{M_a} while the one going from right to left has "probability" approaching zero with respect to a uniform distribution in Γ_{M_d}, for N sufficiently large.

Note that Boltzmann's argument does not really require the assumption that over very long periods of time the macroscopic system should be found in different regions Γ_M, i.e. in different macroscopic states M, for fractions of time *exactly* equal to the ratio of $|\Gamma_M|$ to the total phase space volume specified by its energy. Such behavior, which can be considered as a mild form of the ergodic hypothesis, mild because it is only applied to those regions of the phase space representing macrostates Γ_M, seems very plausible in the absence of constants of the motion which decompose the energy surface into regions with different macroscopic states. It appears even more reasonable when we take into account the lack of perfect isolation in practice which will be discussed later. Its implication for "small fluctuations" from equilibrium is certainly consistent with observations. (The stronger form of the ergodic hypothesis also seems like a natural assumption for macroscopic systems. It gives a simple (even if not the correct) derivation for equilibrium properties of macro systems.)

It should be noted here that an important ingredient in the above analysis is the constancy in time of the Liouville volume of sets in the phase space Γ as they evolve under the Hamiltonian dynamics (Liouville's Theorem). Without this invariance the connection between phase space volume and probability would be impossible or at least very problematic.

4. Initial Conditions

Once we accept the statistical explanation of why macroscopic systems evolve in a manner that makes S_B increase with time, there remains the nagging problem (of which Boltzmann was well aware) of what we mean by "with time": since the microscopic dynamical laws are symmetric, the two directions of the time variable are *a priori* equivalent and thus must remain so *a posteriori*.

Put another way: why can we use phase space arguments (or time asymmetric diffusion type equations) to predict the macrostate at time t of an *isolated* system in a nonequilibrium macrostate M_b at some time t_b, e.g. a metal bar with a nonuniform temperature[f], in the future, i.e. for $t > t_b$, but not in the past, i.e. for $t < t_b$? After all, if the macrostate M is invariant under velocity reversal of all the atoms, then the same prediction should apply equally to $t_b + \tau$ and $t_b - \tau$. A plausible answer to this question is to assume that the nonequilibrium state of the metal bar, M_b, had its origin in an even more nonuniform macrostate M_a, prepared by some experimentalist at some earlier time $t_a < t_b$ and that for states thus prepared we can apply our (approximately) equal a priori probability of microstates argument, i.e. we can assume its validity at time t_a. But what about events on the sun or in a supernova explosion where there are no experimentalists? And what, for that matter, is so special about the status of the experimentalist? Isn't he or she part of the physical universe?

Before trying to answer the last set of "big" questions let us consider whether the assignment of equal probabilities for $X \in \Gamma_{M_a}$ at t_a permits the use of an equal probability distribution of $X \in \Gamma_{M_b}$ at time t_b for predicting macrostates at times $t > t_b$ when the system is isolated for $t > t_a$. Note that those microstates in Γ_{M_b} which have come from

[f] The reader may now think of Fig. 1 as representing energy density in a metal bar

Γ_{M_a} through the time evolution during the time interval from t_a to t_b make up a set Γ_{ab} whose volume is only a very small fraction of the volume of Γ_{M_b}. Thus we have to show that the overwhelming majority of points in Γ_{ab} (with respect to Liouville measure on Γ_{ab}, which by Liouville's theorem is the same as Liouville measure on Γ_{M_a}) have *future* macrostates like those typical of Γ_b—while still being very special and unrepresentative of Γ_{M_b} as far as their *past* macrostates are concerned.[g] This property is explicitly proven by Lanford in his derivation of the Boltzmann equation (for short times) [17], and is part of the derivation of hydrodynamic equations [18]; see also [19].

To see intuitively the origin of this property we note that for systems with realistic interactions the domain Γ_{ab} will be so convoluted as to *appear* uniformly smeared out in Γ_{M_b}. It is therefore reasonable that the future behavior of the system, as far as macrostates go, will be unaffected by their past history. It would of course be nice to prove this in all cases, e.g. justifying (for practical purposes) the factorization or "Stosszahlansatz" assumed by Boltzmann in deriving his dilute gas kinetic equation for all times $t > t_a$, not only for the short times proven by Lanford. Our mathematical abilities are, however, equal to this task only in very simple situations, such as the evolution of independent particles in periodic Lorentz gas. These results should, however, be enough to convince a 'reasonable' person.

5. Velocity Reversal

[g] We are considering here the case where the macrostate $M(t)$, at time t, determines $M(t')$ for $t' > t$. This corresponds to the "Markov case" discussed in [11]. There are of course situations where $M(t')$ depends also (weakly or even strongly) on the history of $M(t)$ in some time interval prior to t', e.g. in materials with memory.

The large number of atoms present in a macroscopic system plus the chaotic nature of the dynamics also explains why it is so difficult, essentially impossible for a clever experimentalist to deliberately put such a system in a microstate which will lead it to evolve contrary to the second law. Such microstates certainly exist—just start with a nonuniform temperature, let it evolve for a while, then reverse all velocities. In fact, they are readily created in the computer simulations with no roundoff errors discussed earlier [15], see Fig. 2 and 3. To quote again from Thomson's article [3]: "If we allowed this equalization to proceed for a certain time, and then reversed the motions of all the molecules, we would observe a disequalization. However, if the number of molecules is very large, as it is in a gas, any slight deviation from absolute precision in the reversal will greatly shorten the time during which disequalization occurs." In *addition*, the effect of unavoidable small outside influences, which are unimportant for the evolution of macrostates in which $|\Gamma_M|$ is increasing, will greatly destabilize evolution in the opposite direction when the trajectory has to *be aimed* at a very small region of the phase space.

The last statement is based on the very reasonable assumption that almost any small outside perturbation of an "untypical" microstate X will tend to make it more typical of its macrostate $M(X)$, which itself is not sensibly affected by the perturbation [3, 13]. The perturbation will thus not interfere with behavior typical of Γ_M. If however we are in a micro-state x_b at time t_b, where $X_b = T_\tau X_a, \tau = t_b - t_a > 0$, with $|\Gamma_{M_b}| >> |\Gamma_{M_a}|$ (we assume τ sufficiently large for this to happen), and we now reserve all velocities then RX_b will be heading towards a smaller phase space volume during the interval $(t_b, t_b + \tau)$ and this behavior is very untypical of Γ_{M_b}. The velocity reversal therefore requires "perfect aiming" and will very likely be derailed by even small imperfections in the reversal and/or tiny outside influences. After a *very short* time interval $\tau' << \tau$, in which S_B decreases, the imperfections in the reversal and the "outside" perturbations, such as one coming from a sun flare, a star quake in a distant galaxy (a long time ago) or from a butterfly beating its wings [13], will make it increase again. This is clearly illustrated in Fig. 3, which shows how a small perturbation which has no effect on the forward macro evolution, completely destroys the time reversed evolution. The situation is somewhat analogous to those pinball machine type puzzles where one is supposed to get a small metal ball into a small hole. You have to do things just right to get it in but almost anything you do gets it out into larger regions. For the macroscopic systems we are considering, the disparity between relative sizes of the comparable regions in the phase space is unimaginably larger[e] as noted in the example in Section 1. In the absence of any "grand conspiracy", the behavior of such systems can therefore be confidently predicted to be in accordance with the second law (except possibly for very short time intervals). This is the reason why even in those special cases such as spin-echo type experiments where such procedures are possible, the "anti-second law" trajectory lasts only for a short time [20]. In addition the *total* entropy change in the whole process, including that in the apparatus used to affect the spin reversal, is positive.

6. Cosmological Considerations

Let us return now to the big question posed earlier: what is special about t_a compared to t_b in a world with symmetric laws? Put differently, where ultimately do initial conditions such as those assumed at t_a come from? In thinking about this we are led more or less inevitably to cosmological considerations and to postulate an initial "macrostate of the universe" having a very small Boltzmann entropy, (see also Appendices B and C). To again quote Boltzmann [21]: "That in nature the transition from a probable to an improbable state does not take place as often as the converse, can be explained by assuming a very improbable [small S_B] initial state of the entire universe surrounding us. This is a reasonable assumption to make, since it enables us to explain the facts of experience, and one should not expect to be able to deduce it from anything more fundamental". We do not, however, have to assume a very specific initial microstate of the universe, it is sufficient to assume that this microstate is typical of an initial macrostate M_0 which is far from equilibrium. This is a very important aspect of Boltzmann's insight as he further writes: "we do not have to assume a special type of initial condition in order to give a mechanical proof of the second law, if we are willing to accept a statistical viewpoint...if the initial state is chosen at random...entropy is almost certain to increase." To repeat, all that it is necessary to assume is a far from equilibrium initial macrostate and this is in accord with all cosmological and other independent evidence.

Feynman clearly agrees with this when he says [22], "it is necessary to add to the physical laws the hypothesis that in the past the universe was more ordered, in the technical sense, than it is today...to make an understanding of the irreversibility." More recently the same point was made very clearly by Roger Penrose in connection with the "big bang" cosmology [23]. Penrose, unlike Boltzmann, believes that we should search for a more fundamental theory which will also account for the initial conditions. Meanwhile he takes for the initial macrostate of the universe the smooth energy density state prevalent soon after the big bang. Whether this is the appropriate initial state or not, it captures an essential fact about our universe. Gravity, being purely attractive and long range, is unlike any of the other fundamental forces. When there is enough matter/energy around, it completely overcomes the tendency towards uniformization observed in ordinary objects at high energy densities or temperatures. Hence, in a universe dominated, like ours, by gravity, a uniform density corresponds to a state of very low entropy, or phase space volume, for a given total energy, see Fig. 4; see also [24] and Appendices B and C.

The local 'order' or low entropy we see around us (and elsewhere)—from complex molecules to trees to the brains of experimentalists preparing macrostates—is perfectly consistent with (and possibly even a consequence of, i.e. typical of) the initial macrostate of the universe. The value of $S_B(t)$ at the present time, corresponding to the current clumpy macrostate of the universe, consisting of planets, stars, galaxies, and black holes, is much much larger than $S_B(0)$ the Boltzmann entropy of the "initial state" (say some start time after the big bang) but still quite far away from its equilibrium value. The 'natural' or 'equilibrium' state of the universe is, according to Penrose, one with all matter and energy collapsed into one big black hole with a phase space volume some $10^{10^{123}}$ times that of the initial macrostate, see Fig. 5. (So we may still have a long way to go.)

7. Boltzmann vs. Gibbs Entropies

The Boltzmannian approach, which focuses on the evolution of a particular macroscopic system, is conceptually different from the Gibbsian approach, which focuses primarily on ensembles. This difference shows up strikingly when we compare Boltzmann's entropy—defined in (1) for a microstate X of a macroscopic system—with the more commonly used (and misused) entropy S_G of Gibbs, defined for an ensemble density $\rho(X)$ by

$$S_G(\{\rho\}) = -k \int \rho(X)[\log \rho(X)] dX. \tag{2}$$

Here $\rho(X)dX$ is the probability (obtained some way or other) for the microscopic state of the system to be found in the phase space volume element dX and the integral is over the phase space Γ. Of course if we take $\rho(X)$ to be the generalized microcanonical ensemble associated with a macrostate M,

$$\rho_M(X) \equiv \begin{cases} |\Gamma_M|^{-1}, & \text{if } X \in \Gamma_M \\ 0, & \text{otherwise} \end{cases}, \tag{3}$$

then clearly,

$$S_G(\{\rho_M\}) = k \log |\Gamma_M| = S_B(M). \tag{4}$$

Generalized microcanonical ensembles like $\rho_M(X)$, or their canonical version, are commonly used to describe systems in which the particle density, energy density and momentum density vary slowly on a microscopic scale *and* the system is, in each small macroscopic region, in equilibrium with the prescribed local densities, i.e. when we have local equilibrium [18]. In such cases $S_G(\{\rho_M\})$ and $S_B(M)$ agree with each other, and with the macroscopic hydrodynamic entropy.[d]

Note however that unless the system is in complete equilibrium and there is no further systematic change in M or ρ, the time evolutions of S_B and S_G are *very* different. As is well known, it follows from the fact that the volume of phase space regions remains unchanged under the Hamiltonian time evolution (even though their shape changes greatly) that $S_G(\{\rho\})$ never changes in time as long as X evolves according to the Hamiltonian evolution, i.e. ρ evolves according to the Liouville equation; $S_B(M)$, on the other hand, certainly does change. Thus, if we consider the evolution of the microcanonical ensemble corresponding to the macrostate M_a in Fig. 1a after removal of the constraint, S_G would equal S_B initially but subsequently S_B would increase while S_G would remain constant. S_G therefore does not give any indication that the system is evolving towards equilibrium.

This reflects the fact, discussed earlier, that the microstate $X(t)$ does not remain typical of the local equilibrium state $M(t)$ for $t > 0$. As long as the system remains truly isolated the state $T_t X$ will contain subtle correlations, which are reflected in the complicated shape which an initial region Γ_M takes on in time but which do not affect the future time evolution of M (see the discussion at end of section on Initial Conditions). *Thus the relevant entropy for understanding the time evolution of macroscopic systems is S_B and not S_G.*

Of course, if we do a "course graining" of ρ over cells Γ_M then we are essentially back to dealing with ρ_M, and we are just defining S_B in a backhanded way. This is one of the standard ways, used in many textbooks, of reconciling the constancy of S_G with the behavior of real systems. I fail to see what is gained by this. Why not use S_B from the beginning?

8. Quantitative Considerations

Let me now describe briefly the very interesting work, still in progress, in which one rigorously derives time asymmetric hydrodynamic equations from reversible microscopic laws [18]. While many qualitative features of irreversible macroscopic behavior depend very little on the positivity of Lyapunov exponents, ergodicity, or mixing properties of the microscopic dynamics, such propertiens are important for the quantitative description of the macroscopic evolution, i.e. for the existence of time-asymmetric *autonomous* equations of hydrodynamic type. The existence and form of such equations depend on the instabilities of microscopic trajectories induced by chaotic dynamics. When the chaoticity can be proven to be strong enough (and of the right form) such equations can be derived rigorously from the reversible microscopic dynamics by taking limits in which the ratio of macroscopic to microscopic scales goes to infinity. Using the law of large numbers one shows that these equations describe the behavior of almost all individual systems in the ensemble, not just that of ensemble averages, i.e. that the dispersion goes to zero in the scaling limit. The equations also hold, to a high accuracy, when the macro/micro ratio is finite but very large.

A simple example in which this can be worked out in detail is the periodic Lorentz gas (or Sinai billiard). This consists of a *macroscopic number of non-interacting particles* moving among a periodic array of fixed convex scatterers, arranged in the plane in such a way that there is a maximum distance a particle can travel between collisions. The chaotic nature of the microscopic dynamics, which leads to an approximately isotropic local distribution of velocities, is directly responsible for the existence of a simple autonomous deterministic description, via a diffusion equation, for the macroscopic particle profiles of this system [18], [24]. A second example is a system of hard spheres at very low densities for which the Boltzmann equation has been shown to describe the evolution of the density in the six dimensional position and velocity space (at least for short times) [17]. I use these examples, despite their highly idealized nature, because here all the mathematical i's have been dotted. They thus show *ipso facto*, in a way that should convince even (as Mark Kac put it) an "unreasonable" person, not only that there is no conflict between reversible microscopic and irreversible macroscopic behavior but also that, *for essentially all initial microscopic states consistent with a given nonequilibrium macroscopic state*, the latter follows from the former—in complete accord with Boltzmann's ideas. Yet the debate goes on.

I also note here that the characterization of a macrostate M usually done via density fields in three dimensional space can be extended to mesoscopic descriptions. This is particularly convenient for a *dilute gas* where M can be usefully characterized by the density in the six dimensional position and velocity space of a single molecule. The deterministic macroscopic (or mesoscopic) evolution of this M is *then* given by the Boltzmann equation and $S_B(M)$ coincides with the negative of Boltzmann's famous H-function[d].

It is important to remember however that for systems in which the potential energy is relevant, e.g. non-dilute gases, the H-function does not agree with S_B and $-H$ (but not S_B) will decrease for suitable macroscopic initial conditions. As pointed out by Jaynes [12] this will happen whenever one starts with an initial total energy E and kinetic energy $K = K_0$ such that $K_0 > K_{eq}(E)$, the value that K takes when the system is in equilibrium with energy E. This can be readily seen if the initial macrostate is one in which the spatial density is uniform and the velocity distribution is Maxwellian with the appropriate temperature $T_0 = \frac{2}{3}K_0/kN$. The temperature will then decrease as the system goes to equilibrium and $-H$ which, for a Maxwellian distribution, is proportional to $\log T$ will therefore be smaller in the equilibrium state when $T = T_{eq}(E) < T_0$.

It should also be remarked that Einstein's formula for the probability of fluctuations in an equilibrium system,

$$\text{Probability of } M \quad \sim \quad \exp\{[S(M) - S_{eq}]/k\} \tag{5}$$

is essentially an inversion of formulas (3) and (4). When combined with the observation that the entropy $S_B(M)$ of a macroscopic system, prepared in a specified nonuniform state M, can be computed from macroscopic thermodynamic considerations it yields useful results. In particular when $S_B(M)$ in the exponent is expanded around M_{eq}, and only quadratic terms are kept, we obtain a Gaussian distribution for normal (small) fluctuations from equilibrium. This is one of the main ingredients of Onsager's reciprocity relations, [25].

9. Quantum Mechanics

While the above analysis was done in terms of classical mechanics the situation is in many ways similar in quantum mechanics. Formally the reversible incompressible flow in phase space is replaced by the unitary evolution of wave functions in Hilbert space and velocity reversal of X by complex conjugation of Ψ. In particular, I do not believe that quantum *measurement* is a *new* source of irreversibility. Rather, real measurements on quantum systems are time-asymmetric because they involve, of necessity, systems with very large number of degrees of freedom whose irreversibility can be understood using natural extensions of classical ideas [26].There are however also some genuinely new features in quantum mechanics relevant to our problem.

Let me begin with the similarities. In a surprisingly little quoted part of his famous book on quantum mechanics [27], von Neumann discusses in some detail (but not very clearly) what might be taken to be the quantum analog of the classical Boltzmann entropy $S_B(X) = \log |\Gamma_{M(X)}|$. Von Neumann proposes to describe a macrostate M of an isolated macroscopic quantum system by specifying the values (within some intervals) of a set of "rounded off" commuting macroscopic observables (operators) $\hat{M}$ representing particle number, energy, etc. in each of the cells into which the box containing the system is divided to define the macrostate M. The analog of Γ_M is then taken to be the linear subspace $\hat{\Gamma}_M$ of the Hilbert space $\mathcal{H}$ of the system in which the observables $\hat{M}$ take the values corresponding to M. Labeling the set of possible macrostates by M_α, $\alpha = 1, ..., L$, we will then have, corresponding to the set $\{M_\alpha\}$, an orthogonal decomposition $\{\hat{\Gamma}_\alpha\}$ of $\mathcal{H}$. Calling E_α the projection into $\hat{\Gamma}_\alpha$, von Neumann defines the *macroscopic entropy* of a system specified by a wavefunction $\Psi \in \mathcal{H}$ as

$$\hat{S}_B(\psi) = \sum_{\alpha=1}^{L} p_\alpha(\Psi) \log |\hat{\Gamma}_\alpha| - \sum_{\alpha=1}^{L} p_\alpha(\psi) \log p_\alpha(\psi) \tag{6}$$

where $p_\alpha(\psi)$ is the probability of finding the system with wavefunction ψ in the macrostate M_α,

$$p_\alpha(\Psi) = (\Psi, E_\alpha \Psi), \tag{7}$$

and $|\hat{\Gamma}_\alpha|$ is the dimension of $\hat{\Gamma}_\alpha$.

An entirely analogous definition is made for a system represented by a density matrix $\hat{\mu}$: we simply replace $p_\alpha(\Psi)$ in (6) and (7) by $p_\alpha(\hat{\mu}) = Tr(E_\alpha \hat{\mu})$. In fact ψ just corresponds, as is well known, to a particular (*pure*) density matrix $\hat{\mu}_\psi$. I will therefore from now on use $\hat{\mu}$ to denote the microstate of a quantum system—always meaning by this the most detailed description possible, within conventional quantum mechanics, of the state of a system (see end of this section).

Von Neumann then notes (as we did earlier for classical systems) that unlike $\hat{S}_G(\hat{\mu}) \equiv -Tr(\hat{\mu}\log\hat{\mu})$, $\hat{S}_B(\hat{\mu})$ will change in time for an isolated quantum system not in equilibrium. He also gives a rather lengthy proof of the fact (which is by now well known and easy to prove) that $\hat{S}_B(\hat{\mu}) \geq \hat{S}_G(\hat{\mu})$. Thus if we start the system at $t = t_0$ with a density matrix $\hat{\mu}_0 = \sum p_\alpha E_\alpha / |\hat{\Gamma}_\alpha|$ so that, at t_0, $\hat{S}_B$ and $\hat{S}_G$ are equal then, for $t > t_0$, $\hat{S}_B(\hat{\mu}) \geq \hat{S}_G(\hat{\mu}_t) = \hat{S}_G(\hat{\mu}_0) = \hat{S}_B(\hat{\mu}_0)$. Unfortunately the fact that $\hat{S}_B(\hat{\mu}_t) \geq \hat{S}_B(\hat{\mu}_0)$, which is of course true also in the classical case, doesn't tell us much (if anything) about the actual evolution of the system. (If it did, the second law would be trivially true and we wouldn't need this article.)

On the other hand one can give arguments for expecting $\hat{S}_B(\hat{\mu}_t)$ to increase with t after a constraint is lifted in a macroscopic system until the system reaches the macrostate M_{eq}. These arguments are on the heuristic conceptual level analogous to those given above for classical systems [28], although there are at present no worked out examples analogous to those described in the last section. (This will hopefully be remedied in the near future: see [29] for some interesting work in that direction.) In fact, with the correspondence between linear dimension, $|\hat{\Gamma}_M|$, and phase space volume, $|\Gamma_M|$, the first term on the right side of equation (6) is just what we would intuitively write down for the expected value of the entropy of a classical system of whose macrostate we were unsure, e.g. if we saw a pot of water on the burner and made some guess, described by the probability distribution p_α, about its temperature (i.e. energy density) profile. (The second term in (6) will be negligible compared to the first term for a macroscopic system,classical or quantum, going to zero when divided by the number of particles in the system.)

We come now to the differences between the classical and quantum pictures. While in the classical case the p_α are just an expression of our ignorance, the actual state of the system being definitely in one of the macrostates M_α, this is not so for the quantum system—at least with the conventional Copenhagen interpretation of quantum mechanics [30–32]. In fact, even when the system is in a definite macrostate at time t_0, say $p_\alpha = \delta_{\alpha,k}$, only the classical system will remain in a unique macrostate for times $t > t_0$. The quantum system may evolve to a superposition of different macrostates, as happens in the well known Schrödinger cat paradigm or paradox: a wave functions ψ defining a particular macrostate evolves into a linear combination of wavefunctions associated with very different macrostates, one corresponding to a live and one to a dead cat.

The possibility of superposition of wavefunctions is of course a general, one might say the central, feature of quantum mechanics. It is reflected here by the fact that whereas the relevant classical phase space can be partitioned into cells Γ_M such that every X belongs to exactly one cell, i.e. every microstate corresponds to a unique macrostate, this is not so in quantum mechanics. The superposition principle rules out any such meaningful partition of the Hilbert space: all we have is an orthogonal decomposition. Thus one cannot associate a definite macroscopic state to an arbitrary wave function of the system. This in turn raises questions about the connection between the quantum formalism and our picture of reality, questions which are very much part of the fundamental issues concerning the interpretation of quantum mechanics as a theory of events in the real world; see the recent articles by S. Goldstein and references there for a careful analysis of these problems [32].

Another related difference between classical and quantum mechanics is that quantum correlations between separated systems arising from wave function entanglements preclude, in general, our assigning a wave function to a subsystem $\mathcal{S}_1$ of a system $\mathcal{S}$ in a definite state Ψ, even at a time when there is no direct interaction between $\mathcal{S}_1$ and the rest of $\mathcal{S}$. This makes the standard idealization of physics— an isolated system—much more problematical in quantum mechanics than in classical theory. One might in fact argue that any real system, considered as a subsystem of the universe described by some wavefunction Ψ, will in general not be described by a wavefunction but by a density matrix. For ways of giving meaning to the wavefunction of a subsystem see [32] and papers by Dürr et al. [33] and by Gell-Mann and Hartle [34].

10. Final Remarks

As I stated in the beginning, I have here completely ignored relativity, special or general. The phenomenon we wish to explain, namely the time-asymmetric behavior of spatially localized macroscopic objects, has certainly many aspects which are the same in the relativistic (real) universe as in a (model) non-relativistic one. In fact the only way I see relativity modifying the discussion is in the nature of the appropriate initial cosmological state and in the light it may shed on the interpretation of quantum mechanics; see [34]. This assessment is based on my belief that one can and in fact one must in order to make any scientific progress, isolate segments of reality for separate analysis. It is only after the individual parts are understood, on their own terms, that one can hope to synthesize a *complete picture.*

To conclude, I believe that the Maxwell-Thomson-Boltzmann resolution of the problem of the origin of macroscopic irreversibility contains, in the simplest idealized classical context, all the essential ingredients for understanding this phenomena in real systems. Abandoning Boltzmann's insights would, as Schrödinger says[h] be a most serious scientific regression. I have yet to see any good reason to doubt Schrödinger's assessment.

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[h] Schrödinger writes [35], "the spontaneous transition from order to disorder is the quintessence of Boltzmann's theory ... This theory really grants an understanding and does not ... reason away the dissymmetry of things by means of an a priori sense of direction of time variables... No one who has once understood Boltzmann's theory will ever again have recourse to such expedients. It would be a scientific regression beside which a repudiation of Copernicus in favor of Ptolemy would seem trifling."

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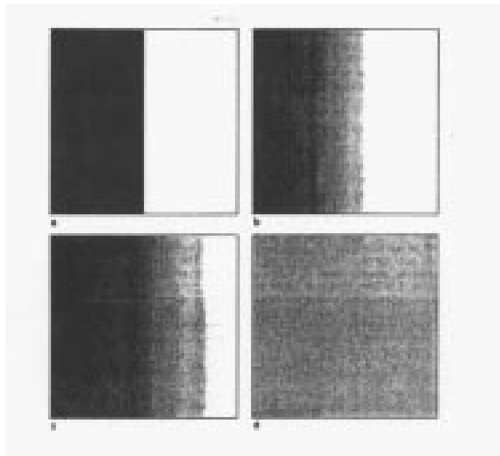

Fig. 1 A sequence of "snapshots" each representing a macroscopic state of a system, say a gas with one type of particles, or two differently colored miscible fluids. How would you order this sequence in time?

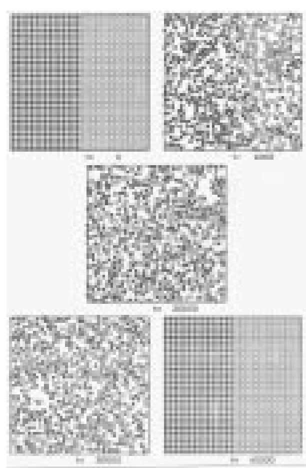

Fig. 2 Time evolution of a system of 900 particles all interacting via the same cutoff Lennard-Jones pair potential using integer arithmetic. Half of the particles are colored white, the other half black. All velocities are reversed at $t = 20,000$. The system then

retraces its path and the initial state is fully recovered. From Levesque and Verlet, Ref. [15].

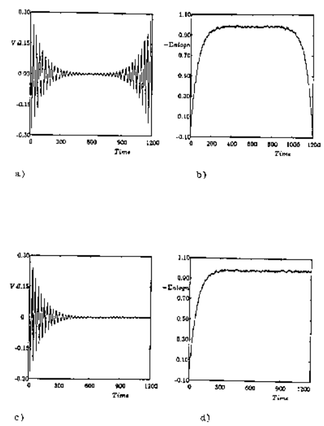

Fig. 3 Time evolution of a reversible cellular automaton lattice gas using integer arithmetic. Figures a) and c) show the mean velocity, figures b) and d) the entropy. The mean velocity decays with time and the entropy increases up to $t = 600$ when there is a reversal of all velocities. The system then retraces its path and the initial state is fully recovered in figures a) and b). In the bottom figures there is a small error in the reversal at $t = 600$. While such an error has no appreciable effect on the initial evaluation it effectively prevents any recovery of the macroscopic velocity. The entropy, on the scale of the figure, just remains at its maximum value. This shows the instability of the reversed path. From Nadiga et al. Ref. [15].

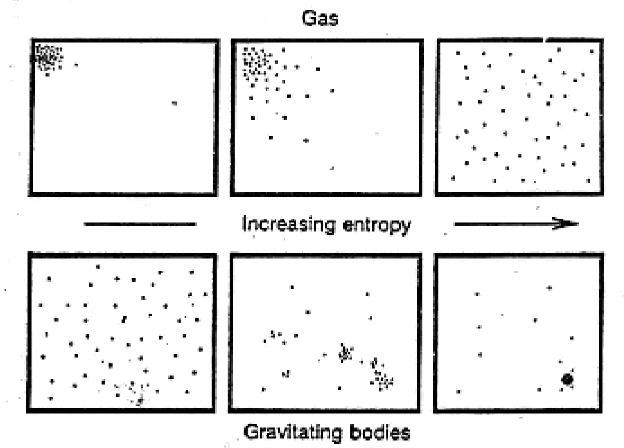

Fig. 4 With a gas in a box, the maximum entropy state (thermal equilibrium) has the gas distributed uniformly; however, with a system of gravitating bodies, entropy can be increased from the uniform state by gravitational clumping leading eventually to a black hole. From Ref. [23].

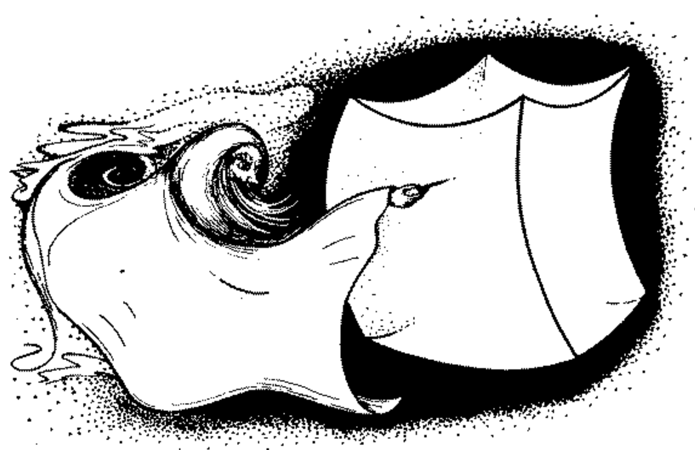

Fig. 5 The creator locating the tiny region of phase-space—one part in $10^{10^{123}}$—needed to produce a 10^{80}-baryon closed universe with a second law of thermodynamics in the form we know it. From Ref. [23].

Figure Captions

Fig. 1 A sequence of "snapshots" each representing a macroscopic state of a system, say a gas with one type of particles, or two differently colored miscible fluids. How would you order this sequence in time?

Fig. 2 Time evolution of a system of 900 particles all interacting via the same cutoff Lennard-Jones pair potential using integer arithmetic. Half of the particles are colored white, the other half black. All velocities are reversed at $t = 20,000$. The system then retraces its path and the initial state is fully recovered. From Levesque and Verlet, Ref. [15].

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Fig. 5 The creator locating the tiny region of phase-space—one part in $10^{10^{123}}$—needed to produce a 10^{80}-baryon closed universe with a second law of thermodynamics in the form we know it. From Ref. [23].

Appendix A:

1. Physics Today[i], Exchanges about Irreversibility, Letters to the Editor

Is Boltzmann Entropy Time's Arrow's Archer?

Ludwig Boltzmann's ideas on irreversibility are as controversial today as they were at their introduction a hundred years ago. In the article "Boltzmann's Entropy and Time's Arrow" (September 1993, page 32), Joel Lebowitz, by giving a modern exposition of Boltzmann's ideas, tries to assure us that the controversy is unwarranted. Readers left unpersuaded should know that they are not alone. Boltzmann's ideas are indeed controversial, because Boltzmann failed to place them on a firm conceptual foundation. Today a firm foundation can be provided—the key ideas are Claude Shannon's statistical information' and Edwin Jaynes's principle of maximum entropy[2]—but Lebowitz's update, instead of providing the necessary clarification, recapitulates the same murky concepts in modern language.

[i] These letters and my response are reproduced here for the sake of completeness.

Lebowitz addresses how time- asymmetric behavior of macroscopic variables arises from time-symmetric microscopic equations. He partitions phase space into macrostates, coarse- grained cells M_i (of phase-space volume $|\Gamma_{M_i}|$) defined by the values of the macroscopic variables of interest—for example, the numbers of particles within identical cubes that fill configuration space. To each phase-space point, or microstate, in M_i he assigns the Boltzmann entropy $S_B(M_i) = klog|\Gamma_{M_i}|$. If the system is initially confined to a small phase- space cell, then when the constraints are released, it will tend to wander into larger cells. Lebowitz quantifies this behavior in terms of the Boltzmann entropy, which tends to increase along a "typical" trajectory.

The problem here is not the story so much as the commentary; for someone outlining an avowedly statistical theory, Lebowitz betrays an odd mistrust of probability concepts. He stresses that he is dealing with the typical behavior of individual systems, not with average behavior within an ensemble. But how can one characterize typical behavior without reference to a probability distribution? Furthermore, he dismisses the Gibbs entropy $S_G = -k \int d\Gamma \rho log \rho$ of a phase-space probability distribution ρ as irrelevant to nonequilibrium phenomena, partly because it remains constant under Hamiltonian evolution, but also because it relies on probabilities. Yet what is the significance of the increase of the Boltzmann entropy when it has an interpretation as a physical quantity only in thermodynamic equilibrium? Indeed, why attribute a Boltzmann entropy to each phase-space point when the Boltzmann entropy is wholly a property of the coarse-graining?

Dealing with these questions entails using probabilities. Lebowitz implies that probabilistic predictions apply only to physical ensembles. To the contrary, when probabilities are sharply peaked, as they are for certain macroscopic variables, they make reliable predictions for *individual* systems. Probabilities provide the *only* way to define typical behavior for individual systems and to assess just how typical it is.

The phase-space probability distribution $p(t)$ at time t follows from applying the system dynamics to a uniform distribution on the initial cell. The statistics of the macroscopic variables at time t, determined by the probabilities $p_i(t) = \int_{M_i} d\Gamma \rho(t)$ to be in cell M_i, are unaffected if $\rho(t)$ is replaced, within each cell M_i, by a uniform distribution containing probability $\rho_i(t)$. This coarse-grained phase-space distribution can be characterized uniquely as having the maximum Gibbs entropy given the probabilities $\rho_i(t)$, the maximum being $\bar{S}_G = -k \sum_i p_i ln p_i + \sum_i p_i S_B(M_i)$.

Lebowitz's insistence on the primacy of Boltzmann entropy over Gibbs entropy is thus stood on its head. The Gibbs entropy $\bar{S}_G$ of the coarse-grained distribution generally increases. Moreover, the increase has a compelling interpretation: Since S_G/k is Shannon's statistical information, the difference between $\bar{S}_G$ and the initial Gibbs entropy is the amount of information discarded when one retains only the statistics of the macroscopic variables. The *average* Boltzmann entropy does contribute to $\bar{S}_G$, but this appearance of the Boltzmann entropies has nothing to do with entropies of individual phase-space points; rather, it is a direct expression of having discarded all information about the details of $\rho(t)$ within the coarse-grained cells.

As Jaynes has emphasized,[2] firm conceptual foundations are required for progress in physics. The shaky foundations provided by Boltzmann and Lebowitz obscure both what has been accomplished and what remains to be done. Boltzmann's ideas can indeed be used to derive time-asymmetric equations for macroscopic variables, once they are supported within the solid framework of Gibbs, Shannon and Jaynes; the Gibbs entropy S_G *explains* the time asymmetry as a consequence of discarding microscopic information that is unnecessary for predicting the behavior of the macro- scopic variables. Yet this explanation, like all good ones, immediately raises other questions: Why coarse-grain? Why discard information? These questions, the true puzzles of irreversibility provide the arena for further work.[3]

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Joel L. Lebowitz s article “Boltzmann’s Entropy and Times Arrow” purports that consideration of the Boltzmann entropy gives a complete resolution of the apparent irreconcilability of the observed irreversible behavior of systems in nature with the time-reversible dynamical laws governing the evolution of trajectories. Lebowitz is correct in pointing out that the Gibbs entropy is constant in all processes and so is not appropriate as a non-equilibrium entropy. However, consideration of the Boltzmann entropy does not give a complete explanation of the problem of irreversibility.

The main virtue of the Boltzmann entropy that is touted in the article is that it "captures the separation between microscopic and macroscopic scales." If the scale-separation argument were the whole story, then irreversibility would be due to our approximate observation or limited knowledge of the system. This is difficult to reconcile with the constructive role of irreversible processes.[1] Furthermore, where the scale separation takes place is not well defined. When the Boltzmann entropy apparently works, as in a gas, it describes only the approach to equilibrium of the velocity distribution for certain initial conditions and does not describe the appearance of correlations.[2]

For these reasons the Brussels- Austin group of which I am a member has for some years proceeded in a different direction. Irreversibility is not to be found on the level of trajectories or wavefunctions but is instead manifest on the level of probability distributions. Both classical and quantum mechanics therefore have to be formulated on the level of probabilities for the classes of dynamical systems where irreversibility takes place. This led to the theory of subdynamics, which allowed the treatment of irreversible processes in terms of both the velocity distribution and correlations.[3] The aim has been to obtain a formulation of the laws of nature in terms of a complex spectral representation of the time-evolution operator for probability densities that is not implementable for trajectories or wavefunctions. This aim has now been fulfilled for classes of chaotic systems[4] and so-called large Poincaré systems[5] by extending the Liouville(-von Neumann) operator to generalized functional spaces. The meaning of entropy becomes clear in this new, extended formulation of dynamics, where the original reversible group splits into trio distinct semigroups; as a result, broken time symmetry appears already at the microscopic level.

Also, a crucial point that is neglected in Lebowitz's article is that irreversible processes are well observed in systems with few degrees of freedom, such as the baker and multibaker transformations.[1,4] Hence, many degrees of freedom is not a necessary condition for irreversible behavior. It is the chaotic dynamics, associated with positive Lyapunov exponents or Poincaré resonances, that causes the system to behave irreversibly.

In conclusion. the arrow of time is not due to some phenomenological approximations but is an intrinsic property of classes of unstable dynamical systems. For these systems the dynamical laws may be formulated in extended functional spaces to include the arrow of time. In this formulation probability appears in an irreducible way. This is of special interest for quantum mechanics, as it leads to a unified formulation avoiding the collapse of the wavefunction (since the basic laws are now given on the level of density matrices).

However. dynamics cannot answer why all semigroups in nature are oriented in the same way. The orientation must be mutually compatible, though, because all systems "communicate"; that is, there are no truly isolated systems in nature. The common orientation of the semigroups expresses the unity of nature.

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Joel Lebowitz's accurate and entertaining account of Boltzmann's classic explanation of macroscopic irreversibility emphasizes *isolated* systems. Gibbs's ensembles made it possible to widen this explanation to include microscopic systems interacting with thermal reservoirs. And in 1984 Shuichi Nosé discovered a reversible dynamics[1] describing Gibbs's thermostatted systems and leading to a new and seminal view of microscopic irreversibility.

Nosée's dynamics makes it possible to generate *non*equilibrium ensembles, characterizing nonequilibrium steady states. Strain rate and heat flux can be specified, as well as composition, energy and volume. Generally, these nonequilibrium ensembles occupy fractal (fractional dimensional) portions of Gibbs's equilibrium phase space. The nonequilibrium phase volume is completely negligible relative to the phase volume of the corresponding Gibbs's equilibrium ensemble—that with the same number of particles, same energy and same volume but without the nonequilibrium fluxes.[2]

The negligible phase volume of the nonequilibrium states results from the multiplicity of constraints implicit in a "steady State". In a system undergoing steady shear at the strain rate ϵ, for instance, not only $d\epsilon/dt$ but also all the higher derivatives ($d^2\epsilon/dt^2$, $d^3\epsilon/dt^3, ...$) must vanish. It is remarkable that Nosé's thermostatted equations of motion are strictly time reversible. And their time behavior on velocity reversal is exactly that described by Lebowitz for isolated systems: The time-reversed flow is less stable with time reversal than is the forward-in-time evolution. This difference in (Lyapunov) stability has recently been rigorously quantified for a restricted set of homogeneously thermostatted nonequilibrium systems.[3] Our own very recent numerical investigations suggest strongly that this asymmetry between the two time directions in steady-state nonequilibrium ensembles can only increase as the homogeneity restriction is relaxed.[4]

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In his interesting article "Boltzmann's Entropy and Time's Arrow" Joel Lebowitz claims, following Boltzmann, that macroscopic irreversibility is explained by the large number of degrees of freedom involved. This view is incomplete. A set of time-symmetric equations evidently cannot lead uniquely to a time-asymmetric solution. There must be another cause.

This cause rests in the fact that we are always concerned with initial, not with terminal, conditions. The mechanical problems we are solving are of the form that at some initial time, say $t = 0$, some macroscopic parameters are given and other variables are random. We then follow the development for $t > O$. Following the solution for negative times, the entropy would also be larger than at $t = 0$; in other words it decreases with time.

The extension to negative times is, however, not of practical interest, because it does not describe a possible situation. In the laboratory this is due to the fact that we can remember the past and make plans for the future, but not vice versa. As regards the world around us, it is no doubt due to the fact that it all started from the Big Bang. Here lies the real reason for the asymmetry.

This is reflected in Boltzmann's *Stosszahlansatz*. This *Ansatz* is based on the seemingly innocuous assumption that the number density of molecules moving in a certain direction in a volume element from which they will in a given time collide with a scattering center is the same as in any other volume element, because "they do not know they are going to collide.” However, the molecules that have just collided (which, in the time- reversed situation, would be the ones about to collide) have a different distribution, because they have been scattered. Thus the “arrow of time” is included in Boltzmann’s treatment, and it is not surprising that it is reflected in the solution.

Lebowitz’s discussion demonstrates that our preference for following evolution forward in time is so strongly ingrained that we do not always realize that this is a choice not forced upon us by the equations of mechanics.

I have discussed these arguments in detail.[1] Similar arguments were given by Feynman.[2] The intention is not to detract from Boltzmann’s merit for having clarified so much of the problem but to point out that an extra step is needed for a complete account of the situation.

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Lebowitz Responds:

Let me deal first with Rudolf Peierls’s letter. (I was not aware of his very nice article when I wrote mine.) I agree entirely with him about the importance of initial conditions and I believe that this is stated clearly in my article; see the section “Initial conditions.” I also believe that he will agree that Boltzmann said it very elegantly in one of his responses:[1]1

From the fact that the differential equations of mechanics are left unchanged by reversing the sign of time without anything else, Herr [Wilhelm] Ostwald concludes that the mechanical view of the world cannot explain why natural processes always run preferentially in a definite direction. But such a view appears to me to overlook that mechanical events are determined not only by differential equations, but also by initial conditions. In direct contrast to Herr Ostwald I have called it one of the most brilliant confirmations of the mechanical view of Nature that it provides an extraordinarily good picture of the dissipation of energy, as long as one assumes that the world began in an initial state satisfying certain conditions. I have called this state an improbable state.

The other three letters (a subset of those received) unfortunately illustrate how much confusion still exists about the problem of macroscopic irreversibility. Each of these letters offers a different solution. According to Howard Barnum and his colleagues the solution lies in information theory; Dean J. Driebe believes that we must reformulate the laws of nature using the mathematics of subdynamics; and according to William G. Hoover and coworkers it is Nosé dynamics that saves the day.

In my opinion information theory, subdynamics and Nosé dynamics all contain interesting and useful ideas and can be illuminating when properly applied. I believe, however, that they are neither needed for nor really relevant to the problem of the asymmetry of observed macroscopic behavior. Boltzmann's ideas adequately explain these observations without requiring reliance on ignorance or modification of the laws of nature. Of course such modifications may come about for other reasons—relativity and quantum mechanics are such modifications that came after Boltzmann's work—but this is not the issue discussed in the article or in the letters.

What Driebe and Barnum and coworkers (and some other writers) have in common is their refusal to accept what to me seems an obvious fact: that irreversible behavior is observed in the evolution of a single macroscopic system that can be adequately described as isolated during the relevant period, be it a jar of fluid or the solar system. Thus when we pour some blue ink into a glass of red ink (of the same density) and seal up the glass tightly (making it an "isolated" system) we *always* see it becoming a uniform color. We don't need to repeat the experiment many times to get an ensemble or a probability distribution, nor do we need to refer to ignorance about the exact microscopic state of the system—any more than we would have needed such considerations to predict the fate of Comet Shoemaker-Levy after it hit Jupiter. Both events are described by deterministic, time-asymmetric macroscopic laws.

In deriving such time-asymmetric laws one of course has to use probability theory to characterise the typicality of the initial microstate of the system with respect to the initial macrostate discussed earlier. One shows (or proves) then that the results for macroscopic observations are so highly peaked that for large macro-to-micro ratios they amount to certainties. In this way probabilities or ensembles are convenient tools for describing "typically" observed phenomena. This is discussed in my article and in the references there; see in particular the section "Notions of probability."

This excessive obsession with probabilities is the source of Driebe's contention that irreversibility is observed in a system whose microscopic state is specified by a point X in the unit square evolving under the baker's dynamics–a paradigm of the confusion surrounding the subject. The macroscopic state of such a system (specified, say, by which half of the square the point X is in) will keep on changing back and forth with time as its microscopic state X jumps all over the square. No observations on such a system will produce anything that looks time asymmetric, just because the system does not have many degrees of freedom. As put by Maxwell, 'The second law is continually being violated... in any sufficiently small group of molecules. ... As the number ... is increased ... the probability of a measurable variation ... may be regarded as practically an impossibility."[2]

Turning now to Nosé dynamics and its various generalizations, these are useful for computer simulations and exhibit interesting analytic behavior. But as I have said in other places[3] there is no reason to believe that they have anything to do with the actual laws governing the dynamics of the microscopic constituents of our actual world. So while it is interesting to speculate on what the world would look like with such dynamics, I believe it is confusing to bring them into the discussion of the conceptual problem of macroscopic irreversibility.

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2. Exchanges about irreversibility in The Sciences

Excerpts from the article *Irreversible Differences* by Tony Rothman, which appeared on p. 26 in the July/August 1997 issue of the magazine The Sciences, published by the New York Academy of Sciences

“Most physicists believe, with Einstein, that ”the distinction between past, present and future is an illusion. ” Not so Ilya Prigogine. Time goes forward, he says, and he can prove it... .

For the past hundred years, since the Austrian physicist Ludwig Boltzmann mistakenly thought he had resolved the paradox of time, physicists have debated its resolution without reaching a consensus. Recently, the Belgian physical chemist Ilya Prigogine caused a stir in Europe by claiming that the paradox of time had been resolved. If he is right, his group has made an enormously significant contribution to physics. Yet on the American side of the Atlantic, the news has been ignored... .

IN 1872 Boltzmann announced he had found an answer to that question. Yet Boltzmann’s theorem is exceedingly strange: it derives time-irreversible behavior from time-reversible laws - a mathematical impossibility... . But there is another side to the story. Faced with the evident inconsistency of Boltzmann’s theorem, some of his contemporaries, including the German physicist Max Planck and the German chemist Friedrich Wilhelm Ostwald, argued that thermodynamics cannot be derived from Newtonian mechanics - that thermodynamics is primary, not secondary. That point of view, entirely incompatible with that of Boltzmann and his followers, has been nearly forgotten by field theorists - but not by Prigogine. Planck and Ostwald were right, he says, and he can prove it...

It is in this atmosphere of fractiousness and bickering, then, that Prigogine and his colleagues have unveiled what they regard as their great unifying theory, the solution to the paradox of time. They begin with an amazingly prescient statement by the French mathematician Henri Poincare, who was a contemporary and critic of Boltzmann's: "Perhaps the kinetic theory of gases will serve as a model.... Physical laws will then take on a completely new form; they will take on a statistical character. " In other words, contrary to Boltzmann, one should not begin with individual particles following Newtonian tra- jectories. Rather, with Planck and Ostwald, one should assume that the average, statistical properties of a gas - its density, temperature, pressure and entropy are fundamental. Thus thermodynamics is primary, not secondary.

But then physics is in a fix. As Poincare knew, one cannot pass from time-symmetric Newtonian mechanics to time-irreversible thermodynamics, and vice versa. Any theory encompassing both must include an escape hatch. The Brussels group finds its escape hatch buried in "unstable dynamical systems." In stark contrast to Weinberg, Prigogine declares that "thanks to Poincare's work, we know that unstable systems play an essential role in the universe."

Clearly, removing an electron from the far edge of the universe represents a slight perturbation, but unstable systems are highly sensitive. Strictly speaking, one needs infinite precision to follow particle trajectories in unstable systems. But infinite precision cannot be achieved in any practical sense. "In such situations, Newtonian trajectories are a mathematical idealization," Prigogine says. "They do not exist." Since in unstable systems one cannot follow trajectories forward or backward, such systems are obviously irreversible...

Although Prigogine has claimed for several decades that unstable systems are the key to understanding irreversibility, he admits that for a long time his ideas were not backed up by rigorous mathematics. Then, three or four years ago, the Brussels theorists took a deep breath and looked beyond vectorlike functions. After that, things began to fall into place... ."

Lebowitz's Response

Dear Editor:

I was dismayed to read in the July issue of The Sciences, a magazine for which I have high regard as well as emotional attachment, Tony Rothman's article "Irreversible Differences." This article deals with the problem or paradox of how observed macroscopic irreversibility arises from time symmetric microscopic laws, a subject I have spent much time studying. Unfortunately the article presents a very inaccurate portrayal of the status of this problem, one which is likely to mislead many readers. I would therefore like to set the record straight.

After some introductory paragraphs Rothman states matter of factly, as if it were something everyone should agree on, that "... the Austrian physicist Ludwig Boltzmann mistakenly thought he had resolved the paradox...". Rothman's use of the word "mistakenly", which is only the first of many such remarks, is not only a mistake on his part, but presumptuous, since the overwhelming majority of scientists agree with Boltzmann. Those who thought deeply about this problem and concluded that Boltzmann was right include, to name just a few, Einstein, Feynman, Onsager, and Schrödinger, who wrote that to go back on Boltzmann's insights "would be a scientific regression beside which a repudiation of Copernicus in favor of Ptolemy would seem trifling." Planck, whom Rothman mentions as an opponent of Boltzmann, had this to say: “The solution of this problem was the life work of the great physicist, Ludwig Boltzmann, and it is one of the finest triumphs of theoretical investigation...”

From this erroneous premise Rothman goes on to present the “great unifying theory, the solution to the paradox of time”. Now, it is certainly true that some scientists had, and some still have, difficulties accepting the resolution of this problem by Maxwell, Thomson (Lord Kelvin) and Boltzmann. There have thus been, over the past 125 years, many new theories aimed at ‘explaining’ the same old facts of macroscopic irreversibility. These have come from philosophers, fringe scientists, and even from some well known scientists. They have all been quickly forgotten – sometimes to be ‘rediscovered’. I guess it is in the nature of the subject – *time* is so close to us, and yet so mysterious – and it is only too easy to confuse problems and solutions. My attempt in the next paragraph to state yet again the Boltzmann point of view is certainly not going to put an end to such new theories, but I think the readers of The Sciences deserve to see it. I will do this mostly by quotes. These are not meant to be a “proof by authority”; it is just that I cannot say it any better than the way it is expressed in these selections.

As Ruelle (whom Rothman quotes) says: “The explanation of irreversibility that we have obtained, following Boltzmann, is at the same time simple and rather subtle. It is a probabilistic explanation. There is no irreversibility of the basic laws of physics, but there is something special about the initial [macro]state of the system that we are considering: this initial state is *very improbable*”. This means that among all the possible complete *microscopic* specifications of the state of the system, i.e. microstates, having a given amount of energy, the proportion corresponding to an initial condition specified by nonequilibrium values of the *macroscopic* variables, i.e. a nonequilibrium macrostate, is exceedingly small. The incredible smallness of this proportion in ordinary situations, typically of order $10^{-10^{20}}$, is due to the fact that even the smallest piece of macroscopic matter contains an enormous number of atoms (more than 10^{22} in one drop of water).

This disparity between microscopic and macroscopic sizes and the nature of the relevant initial conditions are crucial to understanding the observed irreversible behavior of macroscopic objects. It is the essence of Boltzmann's theory as presented in his answer to Ostwald: "From the fact that the differential equations of mechanics are left unchanged by reversing the sign of time without changing anything else, Herr Ostwald concludes that the mechanical view of the world cannot explain why natural processes always run preferentially in a definite direction. But such a view appears to me to overlook that mechanical events are determined not only by differential equations, but also by initial conditions. In direct contrast to Herr Ostwald I have called it one of the most brilliant confirmations of the mechanical view of Nature that it provides an extraordinarily good picture of the dissipation of energy, as long as one assumes that the world began in an initial state satisfying certain conditions. I have called this state an improbable state." Boltzmann further notes: "we do not have to assume a special type of initial condition in order to give a mechanical proof of the second law, if we are willing to accept a statistical viewpoint" [i.e. we do not have to assume a particular or special type of initial microstate as long as we allow that among the many microstates corresponding to an improbable macrostate not all would behave as observed, but only the truly overwhelming majority]. Feynman too makes the same point when he says, “it is necessary to add to the physical laws the hypothesis that in the past the universe was more ordered, in the technical sense, than it is today...to make an understanding of the irreversibility.”

The theory described by Rothman consists, in my opinion, mainly of an empty mathematical formalism which, correct or not, has no bearing on the issue of macroscopic irreversibility. To say that classical particle trajectories “do not exist” because of sensitive dependence on initial conditions is, I think, somewhat like saying that the square root of two doesn't exist because it involves an infinite decimal expansion whose digits become very sensitive to small changes in its value. On the other hand it is true and now well known that deterministic chaos plays an important role in the actual form of the irreversible macroscopic laws, like the diffusion equation. Furthermore, the importance of sensitive dependence on initial conditions and external influences—to the effective impossibility of time-reversing a macroscopic system— was already noted in Kelvin's famous 1874 article where the reversibility paradox was first clearly formulated and qualitatively resolved.

For a good exposition of this topic, containing references to the original works as well as a critical discussion of the “new theory” touted in the Rothman article, I refer the reader to the article by the Belgian physicist, J. Bricmont, *Science of Chaos or Chaos in Science?*, published in *The Flight from Science and Reason*, New York Academy of Science Annals, Vol. 775, p. 131 (1996). He or she can also look up the article by this writer, *Boltzmann's Entropy and Time's Arrow*, in *Physics Today*, Vol. 46, p. 32, September 1993. There is also, in the same magazine, Vol. 47, p. 113, 1994, an exchange of letters on this subject.

The reader can then decide about the 'new theory'.

Appendix B: Book Review[e] of *Time's Arrow and Archimedes' Point*, by Huw Price, Oxford U.P., New York, 1996

Huw Price, a Reader in Philosophy at the University of Sidney, Australia, has written a book addressed to physicists, philosophers, and general readers, about the perception and treatment of time in the formulation of fundamental physical theory. In particular he is concerned with questions like: "Could—and does—the future affect the past? ... What would such a world be like? Is our world like that?" He claims, quite correctly, that "philosophers as well as physicists often fail to pay adequate attention to the [asymmetric] temporal character of the viewpoint which we humans have on the world". To overcome this human bias and achieve 'temporal correctness' Price advocates the "Archimedean view of reality ... the view from *nowhen*" (recalling Archimedes' boast that he could lift the whole earth, given a fixed point outside of it and a long enough lever.)

Price then argues, to quote the book jacket, that "in missing the Archimedean viewpoint, modern physics has missed a radical and attractive solution to many of the apparent paradoxes of quantum physics... these paradoxes can be avoided by allowing that at the quantum level the future does, indeed, affect the past. This demystifies nonlocality...". Hardly a modest claim, even for a book by a philosopher, and this immodesty is unfortunately not confined to the jacket.

To reassure the skeptical reader the book comes with many jacket blurb endorsements by physicists and philosophers. The most restrained of these is by Roger Penrose, who

[e] J. L. Lebowitz, Journal of Statistical Physics, 87, 463–468, 1997

says, "Huw Price provides a thoughtful (and thought-provoking) analysis of the time-asymmetry problem of physics which is in many ways deeper and more illuminating than *accounts to be found elsewhere*" (italics mine). I agree with this evaluation. Price conveys a far better understanding of the issues than is found in most other books devoted to this topic. (I should add, however, that there are many places where, I think, the book misses its mark and also that the book's unnecessary (and unjustified) arrogance will almost surely infuriate most readers, greatly lessening its value to scientists.)

The first main theme of the book is that "The asymmetries of thermodynamics and radiation appear to depend on the fact that the universe had a particular charancter early in its history: its matter was very evenly distributed, which is a very ordered [low entropy] condition for a system in which gravity is the dominant force". Price then argues that while this initial condition explains the observed "macroscopic" asymmetry, which includes our own biological and psychological make up—the past feels very different to us from the future—it does not imply an *additional microscopic asymmetry* which physicists often mistakenly assume. This he calls μInnocence: "interacting [microscopic] systems are uncorrelated before they interact". This leads to "a deep and almost unrecognized conflict in contemporary physics. If we are to retain T-symmetry, we should abandon μInnocence." Furthermore, "quantum mechanics seems to offer empirical confirmation that μInnocence fails. The failure of μInnocence seems to open the way for a kind of backward causation."

A detailed discussion of the quantum world view is in the last part of the book. After describing the usual paradoxes, Price comes down strongly in favor of what he calls "the common future hypothesis", which is a denial of μInnocence, or independence, to objects which have an interaction in the future. “Compared to all other major approaches, its advantage seems to be that it does not conflict with special relativity,” that is, it does not require the “crude” non-locality that follows from Bell’s theorem when backward causation is excluded, because “the point at which [systems] become coupled ... lies well within the light cone of their later [interactions]”. In fact, Price advocates a *local* hidden variable theory made compatible with quantum mechanics and special relativity through *backward causation.*

On the whole Price does well in pointing out “... a variety of common mistakes and misconceptions about time”... and in “sorting out how much of the temporal asymmetry we think we see in the world is objective, and how much is simply a by-product of our own asymmetry.” The idea that this, and other macroscopic asymmetries in our world are explained by the low entropy initial state of our universe is of course not original to Price. It goes back at least to Ludwig Boltzmann, as quoted by E. Broda in *Ludwig Boltzmann, Man-Physicist-Philosopher* (Ox Bow Press (1983), p. 79). “That in nature the transition from a probable to an improbable state does not take place as often as the converse, can be explained by assuming a very improbable initial state of the entire universe surrounding us. This is a reasonable assumption to make, since it enables us to explain the facts of experience...” It is presented succinctly and elegantly by Richard Feynman in *The Character of Physical Law* (MIT Press, 1967), “it is necessary to add to the physical laws the hypothesis that in the past the universe was more ordered, in the technical sense, than it is today ... to make an understanding of the irreversibility”. Price does not quote Feynman but follows closely the recent very clear formulation of this idea in terms of the "big bang" model by R. Penrose in *The Emperor's New Mind* (Oxford U.P., 1989, Chapter 5), who takes for the 'initial state' of the universe the macroscopically smooth energy density state prevalent soon after the big bang.

Gravity, being purely attractive and long range, is unlike any of the other natural forces. When there is enough matter/energy around, it completely overcomes the tendency towards uniformization observed in ordinary objects. Hence, in a universe dominated by gravity, like ours, a uniform density corresponds to a state of very low entropy or phase space volume for a given total energy.

The present clumpy macrostate of the universe, consisting of planets, stars, galaxies, black holes has higher entropy. It can therefore be considered as the 'natural' evolution of the initial macrostatne towards one with higher entropy. The small amount of local 'order' or low entropy we see around us (and elsewhere)—from crystals to complex molecules to trees to brains—is perfectly consistent (and presumably even a consequence) of the much much much larger increase in the total entropy of the universe above its initial state. The 'natural' or 'equilibrium state' of the universe is one with all matter and energy collapsed into one big black hole which, according to Penrose would have a phase space volume some $10^{10^{120}}$ times that of the initial state.

To be able to make the above type of deductions from the smooth initial *macrostate* of the universe, one has to add something about the initial microstate. It is usually assumed, implicitly or explicitly, that the initial *microstate* was *typical* with respect to some (at least vaguely defined) weight or measure on the different microstates compatible with the initial macrostate, e.g. 'uniform' weight to all such microstates. But, accepting this reasonable minimalist assumption of typicality of the initial microstate, one should then be able to decide, at least in principle, what correlations are to be expected in particular situations; no additional independent assumptions about μInnocence would then be necessary or possible. Of course this might still require adopting, as a practical *working hypotheses*, certain rules of thumb about correlations and causations, not only in our daily lives but also in our scientific work. This, and not some unacknowledged "mistake", seems to me the justification for assuming the working assumption of the lack or irrelevance of certain correlations to which Price so strongly objects.

As a very simple analogy consider a gas in equilibrium in some confined spatial region, say half a box; it will have uncorrelated velocities despite the many interactions between the particles. If the volume of the confinement region is expanded by the removal of a partition, at $t = t_0$, the system will then find itself in an uncorrelated nonequilibrium initial state with respect to its Hamiltonian for $t = t_0$. Whether enough of this lack of correlation will persist for $t > t_0$ to make the Boltzmann equation valid at later times is a difficult mathematical problem, whose answer Boltzmann assumed to be in the affirmative; for some rigorous results in this direction see Oscar Lanford, (*Physica A*, **106**, 70, 1981). To make the questions about correlations in this simple system a bit closer to those we might ask about our universe, imagine that at time t_0 many holes are made in the box. The gas expands then into pipes, some of which meet again at a later time, t_1. It is clearly a difficult problem to decide what correlations will be present at t_1, but if $t_1 >> t_0$ and the routes taken by the different streams are sufficiently complex, a good first guess is that there are no significant correlations.

In the above 'gedanken' experiment as well as in the real world, our statements refer to microscopic configurations of the system which are *typical* at $t = t_0$, of the phase space volume associated with a macroscopic system in equilibrium. For such typical configurations we can take the velocities of the atoms in the initial equilibrium gas to be uncorrelated for all practical purposes. See Joel L. Lebowitz (*Physics Today*, **46**, 32, 1993), for a discussion in terms of classical physics and Detlef Dürr, Sheldon Goldstein and Nino Zhangi, (*Jour. of Stat. Phys.*, **67**, 843, 1992) for a quantum mechanical, a la De Broglie-Bohm, version.

I also found that Price is too insistent on the need for time correctness in cosmology, i.e. on the need to treat the final state of the universe in the same manner as the initial state. Price calls the lack of a comprehensive theory of initial and final states cosmology's **basic dilemma**. Now, while it would certainly be nice to have a theory that is able to *explain* the highly improbable smooth initial state of the universe, I am not so worried about the lack of such a theory. I am even less worried about the nature of the universe's final state. As long as we can explain the behavior of our actual and only universe on the basis of some plausible (although highly improbable according to phase space volume considerations) initial macro conditions, it seems to me quite reasonable to accept, at this time, Boltzmann's point of view that "one should not expect to deduce it [the initial state] from anything more fundamental". As for a final state, I would prefer to leave this alone for a while unless one could find any evidence that at the present time we can already feel its influence.*

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* As remarked by Dennis Sciama (*The Unity of the Universe*, Doubleday 1961, p. 70) "the uniqueness of the universe ... raises problems, and for the following reason. Scientists normally have available for study many instances of any particular phenomenon.

The most novel part of the book to me was Price's discussion of causation (I haven't read David Hume since college, if then). Causation, which is deeply rooted in our psychology as unidirectional in time (we can with our 'free will' affect the future but not the past), is a very touchy subject in any model of the universe in which time evolution—be it of the classical variables or of the wave function —is described by deterministic or specified probabilistic equations. In Chapters 6 and 7 of the book Price argues for the view that "the asymmetry of causation is a projection of our own temporal asymmetry as agents in the world." He believes, however, that this does not rule out "backward dependence, in circumstances in which an agent's access to past events is limited in certain ways". Consequently, the usual paradoxes associated with backward causation in science fiction time travel stories, like killing your mother before she gave birth to you, don't apply to the microscopic world of quantum mechanics, in which we cannot gain complete knowledge of a system's state without affecting that state by our interactions with it (measurement).

By comparing these instances with one another they are able to distinguish between the fundamental and the accidental aspects of the phenomenon. For example, by comparing many instances of motion under gravitation, Newton discovered that the shape of an orbit is fundamental but its size is not. Now with only one universe available for study, we have no basis for distinguishing between its fundamental and its accidental features. Two choices are then open to us. We can regard all its features as either equally fundamental or equally accidental. For my part, I believe that the aim of science should be to show that no feature of the universe is accidental." To me this seems quite unreasonable as a program: if such a result comes out from our study of the relationships in our actual universe, that is a great bonus, but it may just not be in the cards.

Price then goes on to present a good account of the conceptual problems present in our current view of the world, a world where results of measurements, as given by instrument readings, are wonderfully accurately predicted by quantum mechanics, but where the *true* nature of the reality described by the theory is very problematic . His discussion of the Einstein-Bohr "debate" about the *completeness* of the quantum description of reality is better than that found in much of the physics literature. He hits the nail on the head when he writes "The EPR [Einstein, Podolsky, Rosen] arguments failed by and large to sway supporters of the Copenhagen Interpretation, but this is perhaps due more to the obscurity of the Copenhagen response than to any compelling counterargument it brought to light". This debate is still very much with us, with many physicists apparently ready to deny the existence of any reality on the microscopic level. This appears to me, however, quite untenable. As R. Penrose puts it in *Shadows of the Mind* (Oxford University Press, 1994), "it makes no sense to use the term 'reality' just for objects that we can perceive, such as (certain types of) measuring devices, denying that the term can apply at some deeper underlying level. Undoubtedly the world is strange and unfamiliar at the quantum level, but it is not 'unreal'. How, indeed, can real objects be constructed from unreal constituents?'

Where Price is least convincing is in his argument about the merits of backward causation as a viable explanation of our world. It's not that what Price suggests is clearly wrong, and it certainly should not be dismissed out of hand. What Price doesn't seem to fully appreciate is the difference between having a general idea, which one can discuss at lunch, and actually providing a consistent physical theory, or even the outlines of one, which implements, in the form of equations, this backward causation. Lacking such a theory he should have put forward his ideas much more tentatively. It would also have been useful to give some discussion of the work of Yakir Aharonov and L. Vaidman (*Phys. Rev.* **A41**, 1, 1990), whose ideas about associating two wave functions to a system—corresponding to past and to future measurements—might in some ways be considered as a *start* towards a well-developed theory incorporating backwards causation.

Whether a theory of this kind could really be made viable is another matter. As John Stuart Bell puts it (*Epistemological Letters*, Feb. 1977), "A theory may appear in which such conspiracies inevitably occur, and these conspiracies may then seem more digestible than the non-localities of other theories. When that theory is announced I will not refuse to listen, either on methodological or other grounds. But I will not myself try to make such a theory". While Bell's statement does not refer explicitly to the "common future hypothesis", I believe, contrary to what Price feels, that Bell meant it to be included in his statement.

Still, as Robert Browning said, "a man's reach should exceed his grasp, or what's a heaven for?" So, despite many shortcomings and much arrogance, the book is worthy of attention. In fact it is very important to pay careful *attention* to nuances when reading the book or thinking about these matters. Price is well aware of this and nicely illustrates it by a quote from "Marx" at the very beginning of the book, "Time flies like an arrow: fruit flies like a banana."

Appendix C: Book Review[f] of *Time's Arrow and Quantum Measurement* by L. S. Schul-

[f] Physics Today, October 1998

man, Cambridge U.P., U.K., 1997, and *Time's Arrow Today: Recent Physical and Philosophical War on the Direction of Time*, Steven F. Savitt, editor.

The two books being reviewed here are part of a steady (or quasi-periodic) stream of books and articles having the phrase "Time's Arrow" (TA) in their title. Last year I reviewed in these pages Huw Price's book, *Time's Arrow and Archimedes' Point* and, perhaps not completely coincidentally, I am also the author of such an article: Boltzmann's Entropy and Time's Arrow (*Physics Today*, Sept. 1993, p. 32).

These writings differ greatly in their starting points, lengths, ambitions, and conclusions. Many (at least those I can understand) accept provisionally Newton's absolute time, which "of itself, and from its own nature, flows equally without relation to anything external," as a primitive, undefined notion and don't worry much about the changes in the notion of time introduced by relativity. They focus instead on the conflict between the TA as a physical phenomenon that has (or, at least, seems to have) a definite direction in time, and the fact that the successful *basic* theories of physics, ultimately derived from and pertaining to the same phenomena as the arrows have a time reversal invariance. There are many TA (Roger Penrose, in a 1979 article listed seven) but the time asymmetric, entropy increasing behavior of macroscopic objects is generally acknowledged to be a central one.

The explanation of this apparent paradox from Boltzmann to the present involves (among other things) the fact that events are determined not only by the basic laws but also by *initial conditions*, indeed by the way our universe started some ten or fifteen billion years ago, according to present cosmological ideas. While this seems quite satisfactory to me, most of the authors in these books are unhappy with it, albeit for reasons more philosophical than physical. In particular some argue (with reason) that the appeal to initial conditions is itself time asymmetric and that it is more natural to consider both initial and final conditions. This was the central theme of Price's book and constitutes a substantial part of both books under review here.

A completely different response to the problems posed by TA is to include asymmetry in the basic laws. A particularly tempting place to do this is in the "measurement equals wave function collapse" version of the Copenhagen interpretation of quantum mechanics. This is, however, not the point of view taken in these books. While the *quantum measurement problem* (QMP) is very much discussed in both books, most authors in the Savitt book, who consider this issue, seem to agree that the TA in the QMP is part of the TA already present in irreversible macroscopic phenomena rather than a true time asymmetry at a basic level. As Leggett emphasizes, however, this aspect of the QMP deserves further theoretical and experimental investigation. Schulman, to whose book I now turn, has a very different view about the whole QMP. (For an illuminating discussion of the QMP, independent of TA considerations, see the article by Sheldon Goldstein in the 1998 March and April issues of *Physics Today*.)

Schulman's book is based on his work (mostly solitary) over the past two decades which began with a very radical proposal for resolving the QMP. It claimed that the Schrödinger wave function resulting, via the usual unitary evolution, from interactions which might correspond to measurements on a macroscopic system—like a cloud chamber particle detector or Schrödinger's cat—is not, as usually assumed, a wave function which is a superposition of incompatible macroscopic quantum states. Rather, it is *always* a quantum state representing just one of the possible outcomes of such a measurement. Thus for Schrödinger's cat experiment in which the usual computations give

$$\Psi = \sqrt{\alpha}\Psi_{\text{alive}} + \sqrt{(1-\alpha)}\Psi_{\text{dead}}$$

with $\alpha \simeq \frac{1}{2}$ Schulman claims that α is invariably (for all practical purposes) either 0 or 1.

To make this possible Schulman invoked the existence of rare 'special states'; wave functions of macroscopic systems with just the properties required for such a definite outcome. Schulman exhibited the existence of such special states in some simple models and gave reasons for believing that they exist also for general macroscopic systems. Very roughly speaking the α for the 'special' states contains a factor like $\sin^2(\pi T/2)$, where "T" refers, in proper units, to some interaction time involved in the experiment. Now if T is an integer then indeed we don't get any superposition. But why should the state of the experimental apparatus prior to the measurement always be 'special' and T always an integer. Schulman's answer is that "in doing an experiment it is impossible to control its precise microscopic state. You set up a macroscopic situation, but the microscopic state of the system will be one of those that avoids superpositions of macroscopically different states in the course of its subsequent time evolution." Going further Schulman states "... there is a single wave function for the entire universe. This wave function has the precise correlations necessary to guarantee 'special' states at every juncture where they are needed." In particular the choices which an experimentalist makes about the knob he/she turns is very strongly correlated with the wave function of an incoming particle on which a measurement is to be made.

Later Schulman proposed making his scheme more plausible by combining it with the use of both initial and final conditions for the universe. The idea is something like this: there may exist some *natural* final conditions on the state of the universe which would force the type of correlation necessary to avoid "grotesque" superposition of macroscopic states. Furthermore, these would occur with just with the right frequencies to reproduce the standard quantum probabilities.

Schulman has some specific and interesting suggestions of where one might look for other effects of such final conditions, e.g. galactic structures. If such effects were to be found (assuming we could recognize them), this would indeed be an amazing discovery, more than justifying Schulman's efforts. How likely is this? Well, it is not impossible and who am I to insist that Nature is not capable of being weird in this particular way. (I am however quite willing to bet heavily against it.) In any case, the book is recommended for the many interesting and amusing things it says about quantum mechanics, nonequilibrium statistical mechanics and in particular "two timing" boundary conditions.

Let me now briefly describe the second book. It consists of an informative, historical introduction by the editor, followed by eleven articles by ten authors. The articles are grouped into four parts: 1) **Cosmology and time's arrow**, with articles by W. Unruh and by H. Price, 2) **Quantum theory and time's arrow**, with articles by A. Leggett, P. Stamp, S. McCall, and R. Douglas, 3) **Thermodynamics and time's arrow** with two articles by L. Sklar (the first a reprint) and one by Barrett and Sober. and 4) **Time travel and time's arrow**, with articles by P. Horwich and by J. Earman.

The articles differ greatly in point of view and quality. Perhaps not surprisingly I liked the articles by the physicists Unruh, Leggett and Stamp best. Of the philosophers' articles I found Price's and Sklar's most interesting, although I felt that there were some important things which they didn't get right.

The articles by S. McCall and R. Douglas definitely do not assume the Newtonian picture of time. Instead they discuss "branched models" of space-time which are related but not identical to the famous (or notorious) "many world interpretation" of the QMP. The Barrett and Sober article did not make much sense to me and I couldn't make myself read more than the introductions to the articles about time travel. These were informative.